

# FORBIDDEN SCIENCE

## Foreword

I perceive in *Forbidden Archeology* an important work of thoroughgoing scholarship and Intellectual adventure. *Forbidden Archeology* ascends and descends into the realms of the human construction of scientific "fact" and theory: postmodern territories that historians, philosophers, and sociologists of scientific knowledge are investigating with increasing frequency.

Recent studies of the emergence of Western scientific knowledge accentuate that "credible" knowledge is situated at an Intersection between physical locales and social distinctions. Historical, sociological, and ethnomethodological studies of science by scholars such as Hany Collins, Michael Mulkay, Steven Shapin, Thomas Kuhn, Harold Garfinkel, Michael Lynch, Steve Woolgar, Andrew Pickering, Bruno Latour, Karin Knorr-Cetina, Donna Haraway, Allucquere Stone, and Malcolm Ashmore all point to the observation that scientific disciplines, be they paleoanthropology or astronomy, "manufacture knowledge" through locally constructed representational systems and practical devices for making their discovered phenomenon visible, accountable, and consensual to a larger disciplinary body of tradition. As Michael Lynch reminds us, "scientists construct and use instruments, modify specimen materials, write articles, make pictures and build organizations."

With exacting research into the history of anthropological discovery, Cremo and Thompson zoom in on the epistemological crisis of the human fossil record, the process of disciplinary suppression,

and the situated scientific handling of "anomalous evidence" to build persuasive theory and local institutions of knowledge and power.

In Cremo and Thompson's words, archeological and paleoanthropological "'facts' turn out to be networks of arguments and observational claims" that assemble a discipline's "truth" regardless, at times, of whether there is any agreed upon connection to the physical evidence or to the actual work done at the physical site of discovery. This perspective, albeit radical, accords with what I see as the best of the new work being done in studies of scientific knowledge.

Forbidden Archeology does not conceal its own positioning on a relativist spectrum of knowledge production. The authors admit to their own sense of place in a knowledge universe with contours derived from personal experience with Vedic philosophy, religious perception, and Indian cosmology. Their intriguing discourse on the "Evidence for Advanced Culture in Distant Ages" is light-years from "normal" Western science, and yet provokes a cohesion of probative thought.

In my view, it is just this openness of subjective positioning that makes Forbidden Archeology an original and important contribution to postmodern scholarly studies now being done in sociology, anthropology, archeology, and the history of science and ideas. The authors' unique perspective provides postmodern scholars with an invaluable parallax view of historical scientific praxis, debate, and development.

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## Introduction and Acknowledgments

In 1979, researchers at the Laetoli, Tanzania, site in East Africa discovered footprints in volcanic ash deposits over 3.6 million years old. Mary Leakey and others said the prints were indistinguishable from those of modern humans. To these scientists, this meant only that the human ancestors of 3.6 million years ago had remarkably modern feet. But according to other scientists, such as physical anthropologist R. H. Tuttle of the University of Chicago, fossil bones of the known australopithecines of 3.6 million years ago show they had feet that were distinctly apelike. Hence they were incompatible with the Laetoli prints. In an article in the March 1990 issue of *Natural History*, Tuttle confessed that "we are left with somewhat of a mystery." It seems permissible, therefore, to consider a possibility neither Tuttle nor Leakey mentioned—that creatures with anatomically modern human bodies to match their anatomically modern human feet existed some 3.6 million years ago in East Africa. Perhaps, as suggested in the illustration on

the opposite page, they coexisted with more apelike creatures. As intriguing as this archeological possibility may be, current Ideas about human evolution forbid It.

Knowledgeable persons will warn against positing the existence of anatomically modern humans millions of years ago on the slim basis of the Laetoli footprints. But there is further evidence. Over the past few decades, scientists In Africa have uncovered fossil bones that look remarkably human. In 1965, Bryan Patterson and W. W. Howells found a surprisingly modern humerus (upper arm bone) at Kanapoi, Kenya. Scientists Judged the humerus to be over 4 million years old. Henry M. McHenry and Robert S. Corruccini of the University of California said the Kanapoi humerus was “barely distinguishable from modern Homo. ” Similarly. Richard Leakey said the ER 1481 femur (thighbone) from Lake Turkana, Kenya, found in 1972, was indistinguishable from that of modern humans. Scientists normally assign the ER 1481 femur, which is about 2 million years old, to prehuman Homo habilis. But since the ER 1481 femur was found by itself. one cannot rule out the possibility that the rest of the skeleton was also anatomically modern. Interestingly enough, In 1913 the German scientist Hans Reck found at Olduvai Gorge, Tanzania, a complete anatomically modern human skeleton in strata over 1 million years old. inspiring decades of controversy.

Here again, some will caution us not to set a few isolated and controversial examples against the overwhelming amount of noncontroversial evidence showing that anatomically modern humans evolved from more apelike creatures fairly recently— about 100,000 years ago, In Africa, and. in the view of some, In other parts of the world as well.

But It turns out we have not exhausted our resources with the Laetoli footprints, the Kanapoi humerus, and the ER 1481 femur. Over the past eight years, Richard Thompson and I, with the assistance of our researcher Stephen Bernath, have amassed an extensive body of evidence that calls Into question current theories of human evolution. Some of this evidence, like the Laetoli footprints, £ fairly recent. But much of it was reported by scientists In the nineteenth and early twentieth centuries. And as you can see, our discussion of this evidence fills up quite a large book.

Without even looking at this older body of evidence, some will assume that there must be something wrong with It—that it was properly disposed of by scientists long ago, for very good reasons. Richard and I have looked rather deeply into that possibility. We have concluded, however, that the quality of this controversial evidence Is no better or worse than the supposedly noncontroversial evidence usually cited in favor of current views about human evolution.

But Forbidden Archeology is more than a well-documented catalog of unusual facts. It is also a sociological, philosophical, and historical critique of the scientific method, as applied to the question of human origins and antiquity.

We are not sociologists. but our approach in some ways resembles that taken by practitioners of the sociology of scientific knowledge (SSK), such as Steve Woolgar. Trevor Pinch, Michael Mulkay, Harry Collins, Bruno Latour, and Michael Lynch.

Each of these scholars has a unique perspective on SSK, but they would all probably agree with the following programmatic statement. Scientists' conclusions do not identically correspond to states and processes of an objective natural reality. Instead, such conclusions reflect the real social processes of scientists as much as, more than, or even rather than what goes on in nature.

The critical approach we take in *Forbidden Archeology* also resembles that taken by philosophers of science such as Paul Feyerabend, who holds that science has attained too privileged a position in the intellectual field, and by historians of

science such as J. S. Rudwick, who has explored in detail the nature of scientific controversy. As does Rudwick in *The Great Devonian Controversy*, we use narrative to present our material, which encompasses not one controversy but many controversies—controversies long resolved, controversies as yet unresolved, and controversies now in the making. This has necessitated extensive quoting from primary and secondary sources, and giving rather detailed accounts of the twists and turns of complex paleoanthropological debates.

For those working in disciplines connected with human origins and antiquity, *Forbidden Archeology* provides a well-documented compendium of reports absent from many current references and not otherwise easily obtainable.

One of the last authors to discuss the kind of reports found in *Forbidden Archeology* was Marcellin Boule. In his book *Fossil Men* (1957), Boule gave a decidedly negative review. But upon examining the original reports, we found Boule's total skepticism unjustified. In *Forbidden Archeology*, we provide primary source material that will allow modern readers to form their own opinions about the evidence Boule dismissed. We also introduce a great many cases that Boule neglected to mention.

From the evidence we have gathered, we conclude, sometimes in language devoid of ritual tentativeness, that the now-dominant assumptions about human origins are in need of drastic revision. We also find that a process of knowledge filtration has left current workers with a radically incomplete collection of facts.

We anticipate that many workers will take *Forbidden Archeology* as an invitation to productive discourse on (1) the nature and treatment of evidence in the field of human origins and (2) the conclusions that can most reasonably be drawn from this evidence.

In the first chapter of Part I of *Forbidden Archeology*, we survey the history and current state of scientific ideas about human evolution. We also discuss some of the epistemological principles we employ in our study of this field. Principally, we are concerned with a double standard in the treatment of evidence.

We identify two main bodies of evidence. The first is a body of controversial evidence (A), which shows the existence of anatomically modern humans in the uncomfortably distant past. The second is a body of evidence (B), which can be interpreted as supporting the currently dominant views that anatomically modern humans evolved fairly recently, about



100,000 years ago in Africa, and perhaps elsewhere.

We also identify standards employed in the evaluation of paleoanthropological evidence. After detailed study, we found that if these standards are applied equally to A and B, then we must accept both A and B or reject both A and B. If we accept both A and B, then we have evidence placing anatomically modern humans millions of years ago, coexisting with more apelike hominids. If we reject both A and B, then we deprive ourselves of the evidential foundation for making any pronouncements whatsoever about human origins and antiquity.

Historically, a significant number of professional scientists once accepted the evidence in category A. But a more influential group of scientists, who applied standards of evidence more strictly to A than to B, later caused A to be rejected and B to be preserved. This differential application of standards for the acceptance and rejection of evidence constitutes a knowledge filter that obscures the real picture of human origins and antiquity.

In the main body of Part I (Chapters 2-6), we look closely at the vast amount of controversial evidence that contradicts current ideas about human evolution. We recount in detail how this evidence has been systematically suppressed, ignored, or forgotten, even though it is qualitatively (and quantitatively) equivalent to evidence favoring currently accepted views on human origins. When we speak of suppression of evidence, we are not referring to scientific conspirators carrying out a satanic plot to deceive the public. Instead, we are talking about an ongoing social process of knowledge filtration that appears quite innocuous but has a substantial cumulative effect. Certain categories of evidence simply disappear from view, in our opinion unjustifiably.

Chapter 2 deals with anomalously old bones and shells showing cut marks and signs of intentional breakage. To this day, scientists regard such bones and shells as an important category of evidence, and many archaeological sites have been established on this kind of evidence alone.

In the decades after Darwin introduced his theory, numerous scientists discovered incised and broken animal bones and shells suggesting that tool-using humans or human precursors existed in the Pliocene (2-5 million years ago), the Miocene (5-25 million years ago), and even earlier. In analyzing cut and broken bones and shells, the discoverers carefully considered and ruled out alternative explanations—such as the action of animals or geological pressure—before concluding that humans were responsible. In some cases, stone tools were found along with the cut and broken bones or shells.

A particularly striking example in this category is a shell displaying a crude yet recognizably human face carved on its outer surface. Reported by geologist H. Stopes to the British Association for the Advancement of Science in 1881, this shell, from the Pliocene Red Crag formation in England, is over 2 million years old. According to standard views, humans capable of this level of artistry did not arrive in Europe until about 30,000 or 40,000 years ago. Furthermore, they supposedly did not arise in their African homeland until about 100,000 years ago.

Concerning evidence of the kind reported by Stopes, Armand de Quatrefages wrote in his book *Hommes Fossiles et Hommes Sauvages* (1884): "The objections made to the existence of man in the Pliocene and Miocene seem to habitually be more related to theoretical considerations than direct observation."

The most rudimentary stone tools, the eoliths ("dawn stones") are the subject of Chapter 3. These implements, found in unexpectedly old geological contexts, inspired protracted debate in the late nineteenth and early twentieth centuries.

For some, eoliths were not always easily recognizable as tools. Eoliths were not shaped into symmetrical implemental forms. Instead, an edge of a natural stone flake was chipped to make it suitable for a particular task, such as scraping, cutting, or chopping. Often, the working edge bore signs of use.

Critics said eoliths resulted from natural forces, like tumbling in stream beds. But defenders of eoliths offered convincing counterarguments that natural forces could not have made unidirectional chipping on just one side of a working edge.

In the late nineteenth century, Benjamin Harrison, an amateur archaeologist, found eoliths on the Kent Plateau in southeastern England. Geological evidence suggests that the eoliths were manufactured in the Middle or Late Pliocene, about

1-4 million ago. Among the supporters of Harrison's eoliths were Alfred Russell Wallace, cofounder with Darwin of the theory of evolution by natural selection; Sir John Prestwich, one of England's most eminent geologists; and Ray E. Lankester, a director of the British Museum (Natural History).

Although Harrison found most of his eoliths in surface deposits of Pliocene gravel, he also found many below ground level during an excavation financed and directed by the British Association for the Advancement of Science. In addition to eoliths, Harrison found at various places on the Kent Plateau more advanced stone tools (paleoliths) of similar Pliocene antiquity.

In the early part of the twentieth century, J. Reid Moir, a fellow of the Royal Anthropological Institute and president of the Prehistoric Society of East Anglia, found eoliths (and more advanced stone tools) in England's Red Crag formation. The tools were about 2.0 - 2.5 million years old. Some of Moir's tools were discovered in the detritus beds beneath the Red Crag and could be anywhere from 2.5 to 55 million years old.

Moir's finds won support from one of the most vocal critics of eoliths, Henri Breuil, then regarded as one of the world's preeminent authorities on stone tools. Another supporter was paleontologist Henry Fairfield Osborn, of the American Museum of Natural History in New York. And in 1923, an international commission of scientists journeyed to England to investigate Moir's principal discoveries and pronounced them genuine.

But in 1939, A. S. Barnes published an influential paper. In which he analyzed the eoliths found by Molr and others in terms of the angle of flaking observed on them. Barnes claimed his method could distinguish human flaking from flaking by natural causes. On this basis, he dismissed all the eoliths he studied, Including Moiras. as the product of natural forces. Since then. scientists have used Barnes's method to deny the human manufacture of other stone tool industries. But In recent years. authorities on stone tools such as George F. Carter, Leland W. Patterson, and A. L. Bryan have disputed Barnes's methodology and Its blanket application. This suggests the need for a reexamination of the European eoliths.

Significantly, early stone tools from Africa, such as those from the lower levels of Olduvai Gorge. appear Identical to the rejected European eoliths. Yet they are accepted by the scientific community without question. This is probably because they fall within, and help support, the conventional spatio-temporal framework of human evolution.

But other Eolithic Industries of unexpected antiquity continue to encounter strong opposition. For example, In the 1950s, Louis Leakey found stone tools over 200,000 years old at Calico in southern California. According to standard views, humans did not enter the subarctic regions of the New World until about 12,000 years ago. Mainstream scientists responded to Calico with predictable claims that the objects found there were natural products or that they were not really 200,000 years old. But there Is sufficient reason to conclude that the Calico finds are genuinely old human artifacts. Although most of the Calico implements are crude. some, including a beaked graver, are more advanced.

In Chapter 4. we discuss a category of implements that we call crude paleoliths. In the case of eoliths. chipping Is confined to the working edge of a naturally broken piece of stone. But the makers of the crude paleoliths deliberately struck flakes from stone cores and then shaped them into more recognizable types of tools. In some cases, the cores themselves were shaped Into tools. As we have seen, crude paleoliths also turn up along with eoliths. But at the sites discussed In Chapter 4, the paleoliths are more dominant In the assemblages.

In the category of crude paleoliths, we Include Miocene tools (5 - 25 million years old) found In the late nineteenth century by Carlos Ribeiro, head of the Geological Survey of Portugal. At an International conference of archeologists and anthropologists held In Lisbon, a committee of scientists Investigated one of the sites where Ribeiro had found Implements. One of the scientists found a stone tool even more advanced than the better of Ribeiro's specimens. Comparable to accepted Late Pleistocene tools of the Mousterian type, It was firmly embedded In a Miocene conglomerate, In circumstances confirming Its Miocene antiquity.

Crude paleoliths were also found In Miocene formations at Thenay, France. S. Laing, an English science writer. noted: "On the whole, the evidence for these Miocene Implements seems to be very conclusive, and the objections to have hardly any other ground than the reluctance to admit the great antiquity of man,"

Scientists also found crude paleoliths of Miocene age at Aurillac, France. And at Boncelles, Belgium, A. Rutot uncovered an extensive collection of paleoliths of Oligocene age (25 to 38 million years old).

In Chapter 5, we examine very advanced stone Implements found in unexpectedly old geological contexts. Whereas the Implements discussed in Chapters

2 and 4 could conceivably be the work of human precursors such as *Homo erectus* or *Homo habilis*, given current estimates of their capabilities, the Implements of Chapter 5 are unquestionably the work of anatomically modern humans.

Florentino Ameghino, a respected Argentine paleontologist, found stone tools, signs of fire, broken mammal bones, and a human vertebra in a Pliocene formation at Monte Hermoso, Argentina. Ameghino made numerous similar discoveries in Argentina, attracting the attention of scientists around the world. Despite Ameghino's unique theories about a South American origin for the hominids, his actual discoveries are still worth considering.

In 1912, Ales Hrdlicka, of the Smithsonian Institution, published a lengthy, but not very reasonable, attack on Ameghino's work. Hrdlicka asserted that all of Ameghino's finds were from recent Indian settlements.

In response, Carlos Ameghino, brother of Florentino Ameghino, carried out new investigations at Miramar, on the Argentine coast south of Buenos Aires. There he found a series of stone Implements, including bolas, and signs of fire. A commission of geologists confirmed the Implements' position in the Chapadmalalan formation, which modern geologists say is 3-5 million years old. Carlos Ameghino also found at Miramar a stone arrowhead firmly embedded in the femur of a Pliocene species of *Toxodon*, an extinct South American mammal.

Ethnographer Eric Boman disputed Carlos Ameghino's discoveries but also unintentionally helped confirm them. In 1920, Carlos Ameghino's collector, Lorenzo Parodi, found a stone Implement in the Pliocene seaside barranca (cliff) at Miramar and left it in place. Boman was one of several scientists invited by Ameghino to witness the implement's extraction. After the implement (a bola stone) was photographed and removed, another discovery was made. "At my direction," wrote Boman, "Parodi continued to attack the barranca with a pick at the same point where the bola stone was discovered, when suddenly and unexpectedly, there appeared a second stone ball. ... It is more like a grinding stone than a bola." Boman found yet another implement 200 yards away. Confounded, Boman could only hint in his written report that the Implements had been planted by Parodi. While this might conceivably have been true of the first Implement, it is hard to explain the other two in this way. In any case, Boman produced no evidence whatsoever that Parodi, a longtime employee of the Buenos Aires Museum of Natural History, had ever behaved fraudulently.

The kinds of implements found by Carlos Ameghino at Miramar (arrowheads and bolas) are usually considered the work of *Homo sapiens sapiens*. Taken at face value, the Miramar finds therefore

demonstrate the presence of anatomically modern humans in South America over 3 million years ago. Interestingly enough, in 1921 M. A. Vignati discovered in the Late Pliocene Chapadmalalan formation at Miramar a fully human fossil jaw fragment.

In the early 1950s, Thomas E. Lee of the National Museum of Canada found advanced stone tools in glacial deposits at Shégúandah, on Manitoulin Island in northern Lake Huron. Geologist John Sanford of Wayne State University argued that the oldest Shégúandah tools were at least 65,000 years old and might be as much as 125,000 years old. For those adhering to standard views on North American prehistory, such ages were unacceptable.

Thomas E. Lee complained: "The site's discoverer [Lee] was hounded from his Civil Service position into prolonged unemployment; publication outlets were cut off; the evidence was misrepresented by several prominent authors . . . : the tons of artifacts vanished into storage bins of the National Museum of Canada; for refusing to fire the discoverer, the

Director of the National Museum, who had proposed having a monograph on the site published, was himself fired and driven into exile; official positions of prestige and power were exercised in an effort to gain control over just six Shégúandah specimens that had not gone under cover; and the site has been turned into a tourist resort. . . Shégúandah would have forced embarrassing admissions that the Brahmins did not know everything. It would have forced the rewriting of almost every book in the business. It had to be killed. It was killed."

The treatment received by Lee is not an isolated case. In the 1960s, anthropologists uncovered advanced stone tools at Hueyatlaco, Mexico. Geologist Virginia Steen-McIntyre and other members of a U.S. Geological Survey team obtained an age of about 250,000 years for the site's implement-bearing layers. This challenged not only standard views of New World anthropology but also the whole standard picture of human origins. Humans capable of making the kind of tools found at Hueyatlaco are not thought to have come into existence until around 100,000 years ago in Africa.

Virginia Steen-McIntyre experienced difficulty in getting her dating study on Hueyatlaco published. "The problem as I see it is much bigger than Hueyatlaco," she wrote to Estella Leopold, associate editor of *Quaternary Research*. "It concerns the manipulation of scientific thought through the suppression of 'Enigmatic Data,' data that challenges the prevailing mode of thinking. Hueyatlaco certainly does that! Not being an anthropologist, I didn't realize the full significance of our dates back in 1973, nor how deeply woven into our thought the current theory of human evolution has become. Our work at Hueyatlaco has been rejected by most archaeologists because it contradicts that theory, period."

This pattern of data suppression has a long history. In 1880, J. D. Whitney, the state geologist of California, published a lengthy review of advanced stone tools found in California gold mines. The implements, including spear points and stone mortars and pestles, were found deep in mine shafts, underneath thick, undisturbed layers of lava, in formations that geologists now say are from 9 million to over 55 million years old. W. H. Holmes of the Smithsonian Institution, one of the

most vocal nineteenth-century critics of the California finds, wrote; "Perhaps if Professor Whitney had fully appreciated the story of human evolution as it is understood today, he would have hesitated to announce the conclusions formulated [that humans existed in very ancient times in North America], notwithstanding the imposing array of testimony with which he was confronted." In other words, if the facts do not agree with the favored theory, then such facts, even an imposing array of them, must be discarded.

In Chapter 6, we review discoveries of anomalously old skeletal remains of the anatomically modern human type. Perhaps the most interesting case is that of Castenedolo, Italy, where in the 1880s, G. Ragazzoni, a geologist, found fossil bones of several *Homo sapiens sapiens* individuals in layers of Pliocene sediment 3 to 4 million years old. Critics typically respond that the bones must have been placed into these Pliocene layers fairly recently by human burial. But Ragazzoni was alert to this possibility and carefully inspected the overlying layers. He found them undisturbed, with absolutely no sign of burial.

Modern scientists have used radiometric and chemical tests to attach recent ages to the Castenedolo bones and other anomalously old human skeletal remains. But, as we show in Appendix 1, these tests can be quite unreliable. The carbon 14 test is especially unreliable when applied to bones (such as the Castenedolo bones) that have lain in museums for decades. Under these circumstances, bones are exposed to contamination that could cause the carbon 14 test to yield abnormally young dates. Rigorous purification techniques are required to remove such contamination. Scientists did not employ these techniques in the 1969 carbon 14 testing of some of the Castenedolo bones, which yielded an age of less than a thousand years.

Although the carbon 14 date for the Castenedolo material is suspect, it must still be considered as relevant evidence. But it should be weighed along with the other evidence, including the original stratigraphic observations of Ragazzoni, a professional geologist. In this case, the stratigraphic evidence appears to be more conclusive.

Opposition, on theoretical grounds, to a human presence in the Pliocene is not a new phenomenon. Speaking of the Castenedolo finds and others of similar antiquity, the Italian scientist G. Sergi wrote in 1884; "By means of a despotic scientific prejudice, call it what you will, every discovery of human remains in the Pliocene has been discredited."

A good example of such prejudice is provided by R. A. S. Macalister, who in 1921 wrote about the Castenedolo finds in a textbook on archeology: "There must be something wrong somewhere." Noting that the Castenedolo bones were anatomically modern, Macalister concluded: "If they really belonged to the stratum in which they were found, this would imply an extraordinarily long standstill for evolution. It is much more likely that there is something amiss with the observations." He further stated: "The acceptance of a Pliocene date for the Castenedolo skeletons would create so many insoluble problems that we can hardly hesitate in choosing between the alternatives of adopting or rejecting their authenticity." This supports the primary point we are trying to make in *Forbidden Archeology*, namely, that there exists in the scientific

community a knowledge filter that screens out unwelcome evidence. This process of knowledge filtration has been going on for well over a century and continues right up to the present day.

Our discussion of anomalously old human skeletal remains brings us to the end of Part I. our catalog of controversial evidence. In Part II of *Forbidden Archeology*, we survey the body of accepted evidence that is generally used to support the now-dominant ideas about human evolution.

Chapter 7 focuses on the discovery of *Pithecanthropus erectus* by Eugene Dubois in Java during the last decade of the nineteenth century. Historically, the Java man discovery marks a turning point. Until then, there was no clear picture of human evolution to be upheld and defended. Therefore, a good number of scientists, most of them evolutionists, were actively considering a substantial body of evidence (cataloged In Part I ) Indicating that anatomically modern humans existed In the Pliocene and earlier. With the discovery of Java man, now classified as *Homo erectus*, the long-awaited missing link turned up In the Middle Pleistocene. As the Java man find won acceptance among evolutionists, the body of evidence for a human presence In more ancient times gradually slid Into disrepute.

This evidence was not conclusively Invalidated. Instead, at a certain point, scientists stopped talking and writing about it. It was incompatible with the Idea that apelike Java man was a genuine human ancestor.

As an example of how the Java man discovery was used to suppress evidence for a human presence In the Pliocene and earlier, the following statement made by W. H. Holmes about the California finds reported by J D. Whitney Is instructive. After asserting that Whitney's evidence "stands absolutely alone," Holmes complained that "it implies a human race older by at least one-half than *Pithecanthropus erectus*. which may be regarded as an incipient form of human creature only.'" Therefore, despite the good quality of Whitney's evidence, It had to be dismissed.

Interestingly enough. modern researchers have reinterpreted the original Java *Homo erectus* fossils. The famous bones reported by Dubois were a skullcap and femur. Although the two bones were found over 45 feet apart, in a deposit filled with bones of many other species. Dubois said they belonged to the same individual. But in 1973, M. H. Day and T. I. Molleson detennined that the femur found by Dubois is different from other *Homo erectus* femurs and is in fact Indistinguishable from anatomically modern human femurs. This caused Day and Molleson to propose that the femur was not connected with the Java man skull.

As far as we can see, this means that we now have an anatomically modern human femur and a *Homo erectus* skull in a Middle Pleistocene stratum that Is considered to be 800,000 years old. This provides further evidence that anatomically modern humans coexisted with more apelike creatures In unexpectedly remote times. According to standard views, anatomically modern humans arose Just 100,000 years ago in Africa. Of course, one can always propose that the anatomically modern human femur somehow got buried quite recently Into the Middle Pleistocene beds at Tiinil. But the same could also be said of the skull.

In Chapter 7, we also consider the many Java *Homo erectus* discoveries reported by G. H. R von Koenigswald and other researchers. Almost all of these bones were surface finds. the true age of which is doubtful. Nevertheless, scientists have assigned them Middle and Early Pleistocene dates obtained by the potassium-argon method. The potassium-argon method is used to date layers of volcanic material, not bones. Because the Java *Homo erectus* fossils were found on the surface and not below the intact volcanic layers, it is misleading to assign them potassium-argon dates obtained from the volcanic layers.

The infamous Piltdown hoax is the subject of Chapter 8. Early in this century, Charles Dawson, an amateur collector, found pieces of a human skull near Piltdown. Subsequently, scientists such as Sir Arthur Smith Woodward of the British Museum and Pierre Teilhard de Chardin participated with Dawson in excavations that uncovered an apelike jaw, along with several mammalian fossils of appropriate antiquity. Dawson and Woodward, believing the combination of humanlike skull and apelike jaw represented a human ancestor from the Early Pleistocene or Late Pliocene, announced their discovery to the scientific world. For the next four decades, Piltdown man was accepted as a genuine discovery and was integrated into the human evolutionary lineage.

In the 1950s, J. S. Weiner, K. P. Oakley, and other British scientists exposed Piltdown man as an exceedingly clever hoax, carried out by someone with great scientific expertise. Some blamed Dawson or Teilhard de Chardin, but others have accused Sir Arthur Smith Woodward of the British Museum, Sir Arthur Keith of the Hunterian Museum of the Royal College of Surgeons, William Sollas of the geology department at Cambridge, and Sir Grafton Eliot Smith, a famous anatomist.

J. S. Weiner himself noted: "Behind it all we sense, therefore, a strong and impelling motive. . . . There could have been a mad desire to assist the doctrine of human evolution by furnishing the 'requisite' 'missing link.' . . . Piltdown might have offered irresistible attraction to some fanatical biologist."

Piltdown is significant in that it shows that there are instances of deliberate fraud in paleoanthropology, in addition to the general process of knowledge filtration.

Finally, there is substantial, though not incontrovertible, evidence that the Piltdown skull, at least, was a genuine fossil. The Piltdown gravels in which it was found are now thought to be 75,000 to 125,000 years old. An anatomically modern human skull of this age in England would be considered anomalous.

Chapter 9 takes us to China, where in 1929 Davidson Black reported the discovery of Peking man fossils at Zhoukoudian (formerly Choukoutien). Now classified as *Homo erectus*, the Peking man specimens were lost to science during the Second World War. Traditionally, Peking man has been depicted as a cave dweller who had mastered the arts of stone tool manufacturing, hunting, and building fires. But a certain number of influential researchers regarded this view as mistaken. They saw Peking man as the prey of a more advanced hominid, whose skeletal remains have not yet been discovered.



In 1983, Wu Rukang and Lin Shenglong published an article in Scientific American purporting to show an evolutionary increase in brain size during the 230,000 years of the Homo erectus occupation of the Zhoukoudian cave. But we show that this proposal was based on a misleading statistical presentation of the cranial evidence.

In addition to the famous Peking man discoveries, many more hominid finds have been made in China. These include, say Chinese workers, australopithecines, various grades of Homo erectus, Neanderthals, early Homo sapiens, and anatomically modern Homo sapiens. The dating of these hominids is problematic. They occur at sites along with fossils of mammals broadly characteristic of the Pleistocene. In reading various reports, we noticed that scientists routinely used the morphology of the hominid remains to date these sites more precisely.

For example, at Tongzi, South China, Homo sapiens fossils were found along with mammalian fossils. Qiu Zhonglang said: "The fauna suggests a Middle-Upper Pleistocene range, but the archeological [i.e., human] evidence is consistent with an Upper Pleistocene age." Qiu, using what we call morphological dating, therefore assigned the site, and hence the human fossils, to the Upper Pleistocene. A more reasonable conclusion would be that the Homo sapiens fossils could be as old as the Middle Pleistocene. Indeed, our examination of the Tongzi faunal evidence shows mammalian species that became extinct at the end of the Middle Pleistocene. This indicates that the Tongzi site, and the Homo sapiens fossils, are at least

100,000 years old. Additional faunal evidence suggests a maximum age of about 600,000 years.

The practice of morphological dating substantially distorts the hominid fossil record. In effect, scientists simply arrange the hominid fossils according to a favored evolutionary sequence, although the accompanying faunal evidence does not dictate this. If one considers the true probable date ranges for the Chinese hominids, one finds that various grades of Homo erectus and various grades of early Homo sapiens (including Neanderthals) may have coexisted with anatomically modern Homo sapiens in the middle Middle Pleistocene, during the time of the Zhoukoudian Homo erectus occupation.

In Chapter 10, we consider the possible coexistence of primitive hominids and anatomically modern humans not only in the distant past but in the present. Over the past century, scientists have accumulated evidence suggesting that humanlike creatures resembling Gigantopithecus, Australopithecus, Homo erectus, and the Neanderthals are living in various wilderness areas of the world. In North America, these creatures are known as Sasquatch. In Central Asia, they are called Almas. In Africa, China, Southeast Asia, Central America, and South America, they are known by other names. Some researchers use the general term "wildmen" to include them all. Scientists and physicians have reported seeing live wildmen, dead wildmen, and footprints. They have also catalogued thousands of reports from ordinary people who have seen wildmen, as well as similar reports from historical records.

Myra Shackley, a British anthropologist, wrote to us: "Opinions vary, but I guess the commonest would be that there is indeed sufficient evidence to suggest at least the possibility of the existence

of various unclassified manlike creatures, but that in the present state of our knowledge it is impossible to comment on their significance in any more detail. The position is further complicated by misquotes, hoaxing, and lunatic fringe activities, but a surprising number of hard core anthropologists seem to be of the opinion that the matter is very worthwhile investigating."

Chapter 11 takes us to Africa. We describe in detail the cases mentioned in the first part of this introduction (Reck's skeleton, the Laetoli footprints, etc.). These provide evidence for anatomically modern humans in the Early Pleistocene and Late Pliocene.

We also examine the status of *Australopithecus*. Most anthropologists say *Australopithecus* was a human ancestor with an apelike head, a humanlike body, and a humanlike bipedal stance and gait. But other researchers make a convincing case for a radically different view of *Australopithecus*. Physical anthropologist C. E. Oxnard wrote in his book *Uniqueness and Diversity in Human Evolution* (1975): "Pending further evidence we are left with the vision of intermediately sized animals, at home in the trees, capable of climbing, performing degrees of acrobatics, and perhaps of arm suspension." In a 1975 article in *Nature*, Oxnard found the australopithecines to be anatomically similar to orangutans and said "it is rather unlikely that any of the australopithecines ... can have any direct phylogenetic link with the genus *Homo*."

Oxnard's view is not new. Earlier in this century, when the first australopithecines were discovered, many anthropologists, such as Sir Arthur Keith, declined to characterize them as human ancestors. But they were later overruled. In his book *The Order of Man* (1984), Oxnard noted: "In the uproar, at the time, as to whether or not these creatures were near ape or human, the opinion that they were human won the day. This may well have resulted not only in the defeat of the contrary opinion but also the burying of that part of the evidence upon which the contrary opinion was based. If this is so, it should be possible to unearth this other part of the evidence," And that. In a more general way, is what we have done in *Forbidden Archeology*, we have unearthed buried evidence, evidence which supports a view of human origins and antiquity quite different from that currently held.

In Appendix 1, we review chemical and radiometric dating techniques and their application to human fossil remains, including some of those discussed in Chapter 6. In Appendix 2, we provide a limited selection of evidence for ancient humans displaying a level of culture beyond that indicated by the stone tools discussed in Chapters 3-5. And in Appendix 3, we provide a table listing almost all of the discoveries contained in *Forbidden Archeology*,

Some might question why we would put together a book like *Forbidden Archeology*, unless we had some underlying purpose. Indeed, there is some underlying purpose.

Richard Thompson and I are members of the Bhaktivedanta Institute, a branch of the International Society for Krishna Consciousness that studies the relationship between modern science and the world view expressed in the Vedic literature. This Institute was founded by our spiritual master, His Divine Grace A. C. Bhaktivedanta Swami Prabhupada, who encouraged us to critically examine the prevailing account of human origins and the methods by which it was established. From the

Vedic literature, we derive the Idea that the human race is of great antiquity. To conduct systematic research into the existing scientific literature on human antiquity, we expressed the Vedic Idea in the form of a theory that various humanlike and apelike beings have coexisted for a long time.

That our theoretical outlook is derived from the Vedic literature should not disqualify it. Theory selection can come from many sources—a private inspiration, previous theories, a suggestion from a friend, a movie, and so on. What really matters is not a theory's source but its ability to account for observations.

Our research program led to results we did not anticipate, and hence a book much larger than originally envisioned. Because of this, we have not been able to develop in this volume our Ideas about an alternative to current theories of human origins. We are therefore planning a second volume relating our extensive research results in this area to our Vedic source material.

Given their underlying purpose, *Forbidden Archeology* and its forthcoming companion volume may therefore be of interest to cultural and cognitive anthropologists, scholars of religion, and others concerned with the interactions of cultures in time and space.

At this point, I would like to say something about my collaboration with Richard Thompson. Richard is a scientist by training, a mathematician who has published refereed articles and books in the fields of mathematical biology, remote sensing from satellites, geology, and physics. I am not a scientist by training. Since 1977, I have been a writer and editor for books and magazines published by the Bhaktivedanta Book Trust.

In 1984, Richard asked his assistant Stephen Bernath to begin collecting material on human origins and antiquity. In 1986, Richard asked me to take that material and organize it into a book.

As I reviewed the material provided to me by Stephen, I was struck by the very small number of reports from 1859, when Darwin published *The Origin of Species*, until 1894, when Dubois published his report on Java man. Curious about this, I asked Stephen to obtain some anthropology books from the late nineteenth and early twentieth centuries. In these books, including an early edition of Boule's *Fossil Men*, I found highly negative reviews of numerous reports from the period in question. By tracing out footnotes, we dug up a few samples of these reports. Most of them, by nineteenth-century scientists, described incised bones, stone tools, and anatomically modern skeletal remains encountered in unexpectedly old geological contexts. The reports were of high quality, answering many possible objections. This encouraged me to make a more systematic search. Digging up this buried literary evidence required another three years. Stephen Bernath and I obtained rare conference volumes and journals from around the world, and together we translated the material into English. The results of this labor provided the basis for Chapters 2-6 in *Forbidden Archeology*.

After I reviewed the material Stephen gave me about the Peking man discoveries, I decided we should also look at recent hominid finds in China. While going through dozens of technical books

and papers, I noticed the phenomenon of morphological dating. And when I reviewed our African material, I encountered hints of the dissenting view regarding *Australopithecus*. My curiosity about these two areas also led to a fruitful extension of our original research program.

Writing the manuscript from the assembled material took another couple of years. Throughout the entire period of research and writing, I had almost daily discussions with Richard about the significance of the material and how best to present it. Richard himself contributed most of Appendix I, the discussion of the uranium series dating of the Hueyatenco tools in Chapter 5, and the discussion of epistemological considerations in Chapter 1. The remainder of the book was written by me, although I relied heavily on research reports supplied by Stephen Bernath for Chapter 7 and the first part of Chapter 9, as well as Appendix 2. Stephen obtained much of the material in Appendix 2 from Ron Calais, who kindly sent us many

Xeroxes of original reports from his archives.

In this second printing of the first edition of *Forbidden Archeology*, we have corrected several small errors in the original text, mostly typographical. The account of a wildman sighting by Anthony B. Wooldridge, originally included in Chapter 10, has been deleted because we have since learned that the author has retracted his statements.

Richard and I are grateful to our Bhaktivedanta Institute colleagues and the other reviewers who read all or part of the manuscript of *Forbidden Archeology*. We have incorporated many, but not all, of their suggestions. Full responsibility for the content and manner of presentation lies with us.

Virginia Steen-McIntyre was kind enough to supply us with her correspondence on the dating of the Hueyatenco, Mexico, site. We also had useful discussions about stone tools with Ruth D. Simpson of the San Bernardino County Museum and about shark teeth marks on bone with Thomas A. Demere of the San Diego Natural History Museum.

I am indebted to my friend Pierce Julius Flynn for the continuing interest he has displayed in the writing and publication of *Forbidden Archeology*. It is through him that I have learned much of what I know about current developments in the social sciences, particularly semiotics, the sociology of knowledge, and postmodern anthropology.

This book could not have been completed without the varied services of Christopher Beetle, a computer science graduate of Brown University, who came to the Bhaktivedanta Institute in San Diego in 1988. He typeset almost all of the book, going through several revisions. He also made most of the tables, processed most of the illustrations, and served as a proofreader. He made many helpful suggestions on the text and illustrations, and he also helped arrange the printing.

For overseeing the design and layout, Richard and I thank Robert Wintennute. The illustrations opposite the first page of the Introduction and in Figure 11.11 are the much-appreciated work of Miles Triplett. The cover painting is by Hans Olson. David Smith, Sigalit Binyaminy, Susan Fritz, Barbara Cantatore, and Michael Best also helped in the production of this book.

Richard and I would especially like to thank the International trustees of the Bhaktivedanta Book Trust, past and present, for their generous support for the research, writing, and publication of this book. Michael Crabtree also contributed toward the printing cost of this book.

Finally, we encourage readers to bring to our attention any additional evidence that may be of interest, especially for Inclusion in future editions of this book. We are also available for Interviews and speaking engagements.

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Michael A. Cremo Alachua, Florida April 24. 1995

## Part I

### Anomalous Evidence

#### The Song of the Red Lion

One evening in 1871. an association of learned British gentlemen. the Red lions, gathered in Edinburgh. Scotland. to feed happily together and entertain each other with humorous songs and speeches. Lord Neaves. known well for his wily lyrics, stood up before the assembled lions and sang twelve stanzas he had composed on "The origin of species a la Darwin.'" Among them:

An Ape with a pliable thumb and big brain,

When the gift of gab he had managed to gain,

As Lord of Creation established his reign Which Nobody can Deny!

His listeners responded. as customary among the Red lions, by gently roaring and wagging their coattails (Wallace 1905, p. 48).

#### 1.1 Darwin Hesitates

Just a dozen years after Charles Darwin published *The Origin of Species* in 1859, growing numbers of scientists and other educated persons considered it impossible, indeed laughable, to suppose that humans were anything other than the modified descendants of an ancestral line of apelike creatures, In *The Origin of Species* itself. Darwin touched but briefly on the question of human beginnings, noting in the final pages only that "Light will be thrown on the origin of man and his history," Yet despite Darwin's caution, it was clear that he did not see humanity as an exception to his theory that one species evolves from another.

Other scientists were not as hesitant as Darwin to directly apply evolutionary theory to the origin of the human species, For these scientists, Darwinism helped explain the remarkable similarity between humans and apes, Even before Darwin published *The Origin of Species*, Thomas Huxley had been investigating anatomical similarities between apes and humans, Huxley clashed with Richard Owen, who insisted that human brains had a unique feature—the hippocampus major. At a meeting of the British Association for the Advancement of science in 1860, Huxley presented evidence showing that brains of apes also had the hippocampus major, thus nullifying a potential objection to the idea that humans had evolved from apelike ancestors, Exuding his usual self-confidence, Huxley (Wendt 1972, p, 71) had written his wife before the British Association meeting: "By next Friday evening they will all be convinced that they are monkeys!"

Huxley did not limit himself to convincing scientists of this proposition, He delivered to working men a series of lectures on the evolutionary connection between humans and lower animals, and in 1863 he published *Man's Place in Nature*, in which he summarized in popular form his arguments for human descent from an apelike creature by the mechanism of Darwinian evolution, In his book, Huxley presented detailed evidence showing the similarity of the human anatomy to that of the chimpanzees and gorillas, The book, intended for general readership, inspired violent criticism but sold well, Scientists continue to use the similarity between humans and apes as an argument in favor of the evolution of humans from ape like ancestors,

Scientists have extended the argument to the molecular level, and have presented evidence showing that there is 99 percent agreement between the DNA sequences of human genes and the corresponding genes of chimpanzees, This certainly suggests a close relationship between humans and chimpanzees, and on a broader scale the shared biochemical mechanisms of living cells indicate a relationship between all living organisms, However, the mere existence of patterns of similarity does not tell us what this relationship is. From an a prior standpoint. it could be a relationship of descent by Darwinian evolution, or it could be something quite different. To actually show evolutionary descent, it is necessary to find physical evidence of transforming sequences of ancestors.

In a companion volume to this book, we will fully discuss the argument that the genealogical tree of human descent can be traced out using biomolecular studies involving mitochondrial DNA and other genetic material. For now. we shall simply point out that interpretation of patterns of molecular similarity in terms of genealogical trees presupposes (rather than proves) that the patterns came about by evolutionary processes. In addition, the assignment of ages to such

patterns of relationships depends on archeological and paleoanthropological studies of ancient human or near human populations. Thus, in the end, all attempts to show the evolution of species (the human species in particular) must rely on the interpretation of fossils and other remains found in the earth's strata.

By the time Darwin published *The Origin of Species* in 1859, some key finds relevant to human origins had already been made. About 15 years previously, Edouard Lartet had found in Miocene strata at Sansan in southern France the first fossils of *Pliopithecus*, an extinct primate thought to be ancestral to the modern gibbons. About this discovery Lartet wrote in 1845: "This corner of ground once supported a population of mammals of much higher degree than those here today. ... Here are represented various degrees in the scale of animal life, up to and including the Ape. A higher type, that of the human kind, has not been found here; but we must not hastily conclude from its absence from these ancient formations that it did not exist" (Boule and Vallois 1957, pp. 17-18). Lartet was hinting that human beings might have existed in Miocene times, over 5 million years ago, an idea that would not win any support from today's scientists.

In 1856, Lartet reported on *Dryopithecus*, a fossil ape discovered by Alfred Fontan near Sansan. This Miocene ape is thought to be anatomically related to the modern chimpanzees and gorillas. Although *Pliopithecus* and *Dryopithecus* provided Darwinists with possible distant ancestors for humans and modern apes, there were no fossils of intermediate beings connecting humans with these Miocene primates. However, in the same year Lartet reported on *Dryopithecus*, the first evidence that intermediate prehuman forms may have existed was found in the Neander valley in Germany.

## 1.2 The Neanderthals

In the latter part of the seventeenth century, a minor German religious poet and composer named Joachim Neumann sometimes wandered through the Dussel River valley in solitary communion with nature. He used the pseudonym Neander, and after his death the local people called the valley the Neanderthal. Two centuries later, others came to the pleasant little valley of the Dussel not for peace of mind but to quarry limestone for the Prussian construction industry. One day in August of 1856, while excavating the Feldhofer cave high on a steep slope of the valley, some workmen discovered human fossils and gave them to Herr Beckershoff. Beckershoff later dispatched a skullcap and some other large bones to J. Carl Fuhlrott, a local schoolteacher with a well-known interest in natural history. Recognizing the fossils as possible evidence of humanity's great antiquity, Fuhlrott in turn gave them to Herman Schaffhausen, a professor of anatomy at the University of Bonn.

At this time, most of the scientists considering the question of human antiquity believed that Europe had once been inhabited by a roundheaded primitive race who used tools of stone and bronze. This race had later been replaced by an invading longheaded race who knew how to use iron. The two races were not, however, regarded as being linked by evolution. In 1857, Professor Schaffhausen delivered reports to scientific gatherings in Germany, calling the newly discovered Neanderthal man a representative of a "barbarous aboriginal race," perhaps descended from the

wildmen of northwestern Europe mentioned in the works of various Roman authors such as Virgil and Ovid. Schaffhausen called special attention to the Neanderthal skull's primitive features—its thick bone structure and its pronounced brow ridges—as evidence of its antiquity and difference from the modern racial type. Others suggested it was simply the skull of a modern man, heavily deformed by disease. And there the matter rested until 1859, when Darwin published *The Origin of Species*, setting off intense speculation about humanity's possible descent from more primitive apelike creatures.

The Neanderthal discovery was then no longer a topic for discussion only among the members of the Natural History Society of the Prussian Rhineland and Westphalia. The heavyweights of European science moved in to pass judgement. Charles Lyell, then recognized as the world's preeminent geologist, came to Germany and personally investigated both the fossils and the cave in which they had been found. He felt nothing conclusive could be deduced from the Neanderthal skeleton. For one thing it was "too isolated and exceptional" (Lyell 1863, p. 375). How could generalizations about human prehistory be drawn from just one set of bones which happened to have some "abnormal and ape-like" features? Lyell also felt that its age was "too uncertain." The unstratified cave deposits in which it had been found could not be assigned a place in the sequence of geological periods. Accompanying animal fossils might have helped establish the age of the Neanderthal man, but none had been found.

Many scientists, especially those opposed to evolutionary doctrines, thought the skeleton was that of a pathologically deformed individual of the recent era. The German anatomist Rudolf Virchow, for example, believed the crude features of the Neanderthal specimen could be explained by deformities resulting from rickets and arthritis. Thirty years after first expressing this opinion in 1857, Virchow still held it, and also continued to dismiss the idea that the Neanderthal bones represented a stage in human evolution from lower species. "The idea that men arose from animals," said Virchow, "is entirely unacceptable in my view, for if such transitional men had lived there would be evidence of it, and such evidence does not exist. The creature preliminary to man has just not been found" (Wendt 1972, pp. 57-58).

A British scientist argued that the "skull belonged to some poor idiotic hennit whose remains were found in the cave where he died" (Goodman 1982, p. 75). Dr. F. Mayer, an anatomist at Bonn University suggested, like Virchow, that the Neanderthal man's bent leg bones had been caused by childhood rickets, or perhaps many years of horse riding. In 1814, Cossack cavalry had moved through the area in pursuit of Napoleon's army. Was the Neanderthal man a wounded Cossack who had crawled into the cave and died? Mayer saw this as a distinct possibility. But Thomas Huxley, writing in *Natural History Review* (1864), asked how a dying soldier got in a cave 60 feet up a steep valley wall and buried himself. And where was his uniform?

An old skull dug up at Forbe's Quarry, during the building of fortifications at Gibraltar in 1848, entered the discussion. On investigation, the fossil skull had turned out to be quite similar to the Feldhofer cave specimen, prompting George Busk, professor of anatomy at the Royal College of Surgeons, to write in 1863: "the Gibraltar skull adds immensely to the scientific value of the



Neanderthal specimen, showing that the latter does not represent. . a mere individual peculiarity, but that it may have been characteristic of a race extending from the Rhine to the Pillars of Hercules. . . . Even Professor Mayer will hardly suppose a rickety Cossack engaged in the campaign of 1814 had crept into a sealed fissure in the Rock of Gibraltar'" (Goodman 1982, p. 77).

In 1865, Hugh Falconer said the Gibraltar skull represented "a very low type of humanity—very low and savage, and of extreme antiquity—but still a man and not halfway between a man and a monkey and certainly not the missing link'" (Millar

1972, p. 62). In similar fashion, Huxley concluded, after examining the detailed drawings of the Neanderthal skull sent to him by Lyell, that the Neanderthals were not the missing link sought by scientists. Despite the skull's somewhat primitive features and its apparent great age, it was in Huxley's opinion quite close to the modern type, close enough to be classified as simply a variation. "In no sense," he said, "can the Neanderthal bones be regarded as the remains of a human being Intermediate between men and Apes" (Huxley 1911. p. 205). Most modern scientists agree with Huxley's analysis and see the Neanderthals as a recent offshoot from the main line of human evolution. The Neanderthals are sometimes designated *Homo sapiens neanderthalensis*, indicating a close relationship with the modern human type.

Huxley (1911, pp. 207-208) then went on to ask, "Where then, must we look for primaeval man? Was the oldest *Homo sapiens* Pliocene or Miocene, or yet more ancient? In still older strata do the fossilized bones of an ape more anthropoid, or a man more pithecoïd. than any yet known await the researches of some unborn paleontologist? Time will show."

### 1.3 Haeckel and Darwinism

Possible intermediate forms between humans and apes were of great concern to the German anatomist Ernst Haeckel. Haeckel, whose specialty was embryology, was an avid advocate of Darwin's theory of evolution by natural selection. He was also famous for his own theory that ontogeny, the step-by-step growth of an animal (or human) embryo, faithfully represents the creature's phylogeny, or evolutionary development over millions of years from a simple, one-celled organism. However, this theory, which is summed up by the slogan "ontogeny recapitulates phylogeny.'" has long been rejected by twentieth-century scientists.

Haeckel had illustrated his theory with drawings of embryos of different kinds of animals. Unfortunately, some of his drawings turned out to be fakes, and he was tried before the Court of Jena University on charges of fraud. In his defense he declared: "A small percent of my embryonic drawings are forgeries: those namely, for which the observed material is so incomplete or insufficient as to compel us to fill in and reconstruct the missing links by hypothesis and comparative synthesis. I should feel utterly condemned . . . were it not that hundreds of the best observers and biologists lie under the same charge" (Meldau 1964, p. 217). If Haeckel's sweeping accusation is correct, this may have important bearing on the mode of anatomical reconstruction employed for the many "missing links'" we will discuss in this book.

Haeckel's enthusiasm for Darwinism was boundless, and he showed no hesitation in proclaiming the essence of the theory, the survival of the fittest, as the foundation of his whole view of reality. An early advocate of social Darwinism, he said: "A grim and ceaseless struggle for life is the real mainspring of the purposeless drama of the world's history. We can only see a 'moral order' and 'design' in it when we ignore the triumph of immoral force and the aimless features of the organism. Might goes before right as long as the organism exists'" (Haeckel 1905, p. 88).

In *Descent of Man*, Darwin himself (1871, p. 501) wrote: "With savages, the weak in body and mind are soon eliminated and those that survive commonly exhibit a vigorous state of health. We civilized men, on the other hand, do our utmost to check the process of elimination; we build asylums for the imbecile, the maimed, and the sick; we institute poor-laws; and our medical men exert their utmost skill to save the life of every one to the last moment. . . . Thus the weak members of civilized societies propagate their kind. No one who has attended to the breeding of domestic animals will doubt that this must be highly injurious to the race of man. . . . Hardly anyone is so ignorant as to allow his worst animals to breed." Modern supporters of Darwin's theory routinely downplay such unsettling statements.

Haeckel was one of the first to compose the familiar phylogenetic tree, showing different groups of living beings related to each other like branches and limbs coming from a central trunk. At the top of Haeckel's tree is found *Homo sapiens*. His immediate predecessor was *Homo sapiens*, "true but ignorant man." And before him came *Pithecanthropus alalus*, the "apeman without speech"—the missing link. Haeckel scored another first by commissioning a highly realistic painting of *Pithecanthropus alalus*, thus starting the longstanding tradition of presenting hypothetical human ancestors to the general public through the medium of lifelike pictures and statues.

Haeckel published his view of human evolution in 1866, in *General Morphology of Organisms*, and in 1868, in *Natural History of Creation*. These books appeared several years before Darwin came out with *Descent of Man*, in which Darwin acknowledged Haeckel's work. Haeckel believed humans had arisen from a primate ancestor in South Asia or Africa: "considering the extraordinary resemblance between the lowest woolly-haired men and the highest man-like apes ... it requires but a slight imagination to conceive an intermediate form connecting the two" (Spencer 1984, p. 9).

#### 1.4 The search Begins

In his book *The Antiquity of Man*, first published in 1863, Charles Lyell, like Huxley and Haeckel, expressed the belief that fossils of a creature intermediate between the apes and humans would someday be found. The most likely places were "the countries of the anthropomorphous apes ... the tropical regions of Africa and the Islands of Borneo and Sumatra" (Lyell 1863, p. 498).

Of course, it should be kept in mind that the missing link was not expected to connect modern humans with modern monkeys, but instead with the fossil apes. The first human ancestor, it was thought, must have branched off from the Old World monkeys sometime before the Miocene period. As Darwin himself stated (1871, p. 520): "We are far from knowing how long ago it was

when man first diverged from the Catarrhine [Old World monkey] stock; but It may have occurred at an epoch as remote as the Eocene period: for that higher apes had diverged from the lower apes as early as the upper Miocene period Is shown by the existence of *Dipithecus*"

*Dryopithecus* is still recognized as an early precursor of the anthropoid or humanlike apes, which include gorillas, chimpanzees, gibbons, and orangutans. As previously noted, *Dryopithecus* was discovered by Alfred Fontan, near Sansan in the Pyrenees region of southern France. In 1856, the find was reported to the scientific world by Edouard Lartet. who also gave it its name, which means "forest ape." in 1868, Louis Lartet. the son of Edouard Lartet, reported on /*Asiatic* of the earliest fully modern humans, discovered near Cro-Magnon in southwestern France. Recently. Cro-Magnon man has been assigned a date of 30,000-40,000 years. At the time, no fossils intermediate between *Dryopithecus* and Cro-Magnon man, except the Neanderthal man bones from Germany and Gibraltar, had been found (or so it appears from today's accounts).

In general. Lyell wanted to see the presence of anatomically modern humans pushed far back in time—but not too far. There were limits: "we cannot expect to meet with human bones in the Miocene formations, where all the species and nearly all the genera of mammalia belong to types widely differing from those now living: and had some other rational being, representing man. then flourished. some signs of his existence could hardly have escaped unnoticed. In the shape of Implements of stone or metal" (Lyell 1863, p. 399).

This idea links the origin of humans directly with the succession in time of mammalian species. and It would be seen today as implicitly evolutionary. However. Lyell (1863. p. 499) proposed withholding final judgement regarding human evolution until a great many fossils confirming modern humanity's link with *Dryopithecus* were discovered: "At some future day, when many hundred species of extinct quadrumana [primates] may have been brought to light, the naturalist may speculate with advantage on this subject."

Still. Lyell clearly felt we should not let the lack of such evidence prejudice us against the idea of evolution. "The opponents of the theory of transmutation sometimes argue," he wrote, "that, if there had been a passage by variation from the lower Primates to Man. the geologist ought ere to this have detected some fossil remains of the intermediate links of the chain" (Lyell 1863. p. 435). But Lyell went on to suggest that "what we have said respecting the absence of gradational forms between the recent and Pliocene mammalia . . . may serve to show the weakness in the present state of science of any argument based on such negative evidence, especially in the case of man. since we have not yet searched those pages of the great book of nature, in which alone we have any right to expect to find records of the missing links alluded to" (1863. pp. 435-436). He believed the proper paleontological pages were to be found in Africa and the East Indies. It is there, he felt that "the discovery, in a fossil state, of extinct forms allied to the human. could be looked for" (Lyell 1863. p. 498).

Lyell's approach was reasonable, since he advocated withholding judgement until enough evidence was gathered. However. while rejecting arguments based on a lack of evidence. he was perhaps implicitly assuming that the discovery of semihuman forms would confirm modern humanity's

descent from those forms. This is an error (and a perennial one), for the presence of a semihuman form does not preclude the contemporary or prior existence of fully human forms.

### 1.5 Darwin speaks

We have now seen that Huxley, Haeckel, and Lyell all wrote major works dealing with the question of human origins and that they did so before Darwin, who had deliberately held back from treating the question in *The Origin of Species*. Finally, in 1871 Darwin came out with his own book, *Descent of Man*. explaining his delay, Darwin (1871, p. 389) wrote: "during many years I collected notes on the origin or descent of man, without any intention of publishing on the subject, but rather with the determination not to publish, as I thought that I should thus only add to the prejudices against my views. It seemed to me sufficient to indicate, in the first edition of my 'Origin of Species,' that by this work 'light would be thrown on the origin of man and his history:' and this implies that man must be included with other organic beings. In any general conclusion respecting his manner of appearance on this earth."

In *Descent of Man*, Darwin was remarkably explicit in denying any special status for the human species. "We thus learn that man is descended from a hairy, tailed quadruped, probably arboreal. In its habits, and an inhabitant of the Old World.

The higher mammals are probably derived from an ancient marsupial animal, and this through a long series of diversified forms, from some amphibianlike creature, and this again from some fish-like animal. In the dim obscurity of the past we can see that the early progenitor of all the vertebrata must have been an aquatic animal. . . . More like the larvae of the existing marine Ascidians than any other known form" (Darwin 1871, p. 911). It was a bold statement, yet one lacking the most convincing kind of proof—fossils of species transitional between the ancient dryopithecine apes and modern humans.

The absence of evidence of possible transitional forms may not provide a proper disproof of evolution, but one can argue that such forms are required in order to positively prove the theory. Yet aside from the Neanderthal skulls and a few other little-reported finds of modern morphology, there were no discoveries of hominid fossil remains. This fact soon became ammunition to those who were revolted by Darwin's suggestion that humans had apelike ancestors. Where, they asked, were the fossils to prove it?

### 1.6 The Incompleteness of the Fossil Record

Darwin himself (1871, p. 521) felt forced to reply and sought to defend himself by appealing to the Imperfection of the fossil record: "With respect to the absence of fossil remains, serving to connect man with his ape-like progenitors, no one will lay much stress on this fact who reads Sir C. Lyell's discussion (*Elements of Geology* 1865, pp. 583-585 and *Antiquity of Man* 1863, p. 145), where he shows that in all the vertebrate classes the discovery of fossil remains has been a very slow and fortuitous process. nor should it be forgotten that those regions which are the most

likely to afford remains connecting man with some extinct ape-like creatures, have as yet not been searched by geologists."

Lyell (1863, p. 146) had argued that it was not "part of the plan of nature to store up enduring records of a large number of Individual plants and animals which have lived." Rather nature tends to regularly clear her files, employing "the heat and moisture of the sun and atmosphere, the dissolving power of carbonic and other acids, the grinding teeth and gastric juices of quadrupeds, birds, reptiles, and fish, and the agency of many other Invertebrata" (Lyell 1863, p. 146). Lyell also pointed out that researchers who had attempted to dredge human fossils from the sediments on the sea bottom had also been unsuccessful. He cited the attempt of the team of MacAndrew and Forbes who "failed utterly in drawing up from the deep a single human bone" and found no human artifacts "on a coast line of several hundred miles in extent, where they approached within less than half a mile of a land peopled by millions of human beings" (Lyell 1863, pp. 146-147).

To the present day, the drastic incompleteness of the fossil record has remained a critical factor in paleontology. Most popular presentations of evolution give the idea that the layers of sedimentary rock offer a complete and incontrovertible record of the progressive development of life on earth. But geologists who have studied the matter have come up with some astounding findings. For example, Tjeerd H. van Andel looked at a series of sandstone and shale deposits in Wyoming, parts of which apparently were submerged in a body of water resembling our present Gulf of Mexico. The rates at which sediment is deposited in the Gulf of Mexico are known. Applying these rates to the Wyoming strata, van Andel calculated they could have been deposited in 100,000 years. Yet geologists and paleontologists agreed that the series spans a time of 6 million years. That means that 5.9 million years of strata are missing. Van Andel (1981, p. 397) stated: "We may repeat the experiment elsewhere: Invariably we find that the rock record requires only a small fraction, usually 1 to 10 percent, of the available time. . . . Thus it appears that the geological record is exceedingly incomplete."

What about the sea bottom? Shouldn't the lack of erosional forces present on continental land masses result in a more complete record there? Van Andel (1981, p. 397) answered: "This turned out to be far from true. In the south Atlantic, for example, barely half of the history of the last 125 Myr is recorded in the sediment. It is no better in other oceans and surely worse for shallow marine and continental environments."

This has definite implications regarding the fossil record. Van Andel (1981, p. 398) warned that "key elements of the evolutionary record may be forever out of reach." J. Wyatt Durham, a past president of the Paleontological Society, pointed out that according to theory, about 4.1 million fossilizable marine species have existed since the Cambrian period some 600 million years ago. Yet only 93,000 fossil species have been catalogued. Durham (1967, p. 564) concluded: "Thus conservatively we now know about one out of every 44 species of invertebrates with hard parts that has existed in the marine environment since the beginning of the Cambrian. I think this ratio is unrealistically conservative; probably one out of every 100 is closer to reality."

When we turn from marine organisms to the totality of living organisms, the situation only gets worse. David m. Raup, curator of Chicago's Field museum, and Steven Stanley, a paleontologist at Johns Hopkins University, estimated that 982 million species have existed during the earth's history, compared with the 130,000 known fossil species. They concluded that "only about .013 of one percent of the species that have lived during this 600 million year period have been recognized in the fossil record" (Raup and Stanley 1971, p. 11).

What does this have to do with human evolution? The standard idea is that the fossil record reveals a basic history, true in outline even though not known in every detail. But this might not at all be the case. Can we really say with complete certainty that humans of the modern type did not exist in distant bygone ages? Consider Van Andel's point that out of 6 million years, only 100,000 may be represented by surviving strata. In the unrecorded 5.9 million years there is time for even advanced civilizations to have come and gone leaving hardly a trace.

Darwin's appeal to the incompleteness of the fossil record served to explain the absence of evidence supporting his theory. It was, nevertheless, basically a weak argument. Admittedly, many key events in the history of life probably have gone unrecorded in the surviving strata of the earth. But although these unrecorded events might support the theory of human evolution, they might radically contradict it.

Today, however, almost without exception, modern paleoanthropologists believe that they have fulfilled the expectations of Darwin, Huxley, and Haeckel by positive discoveries of fossil human ancestors in Africa, Asia, and elsewhere. We will now give a brief summary of these discoveries, placing them within the framework of the history of life on the earth as reconstructed by paleontologists. In this summary, we shall introduce the standard system of geological dates and time divisions that we <sup>11</sup> use throughout the book.

## 1.7 The Geological Timetable

The story of life on earth now accepted by paleontologists can be outlined as follows. About 4.6 billion years ago the earth came into being as part of the formation of the solar system. The earliest evidences of life are fossils reputed to be of single-celled organisms. These date to 35 billion years ago. It is said that only single-celled organisms inhabited the earth until about 630 million years ago, when simple multicellular creatures first make their appearance in the fossil record.

Then, some 590 million years ago, there was an explosive proliferation of invertebrate marine life forms, such as trilobites. This marks the beginning of the Paleozoic era and its first subdivision, the Cambrian period. The first fish are often said to have appeared in the Ordovician period, beginning 505 million years ago, but Cambrian fish have now been reported. In the Silurian period, beginning some 438 million years ago, the first land plants entered the fossil record. We note, however, that spores and pollen from such plants have been reported from Cambrian and even Precambrian marine strata (Oacob et al, 1953, stain forth 1966, McDougall et al, 1963, Snelling 1963). In the Devonian period, which began 408 million years ago, the first amphibians came on the scene,

followed by early reptiles in the carboniferous period, the beginning of which is set at about 360 million years ago. Next is the Pennsylvanian period, which began some 286 million years ago and marks the end of the Paleozoic era.

The next period is the Triassic, which began some 248 million years ago and is marked by the appearance of the first mammals. In the succeeding Jurassic period, which extends from 213 million years to 144 million years ago, paleontologists note the appearance of the first birds. The Jurassic and Triassic periods, along with the following Cretaceous period, are famous as the Age of the dinosaurs and are known collectively as the Mesozoic era. At the end of the Cretaceous period, some 65 million years ago, the dinosaurs mysteriously died out.

Then comes the Cenozoic era. The name Cenozoic is made of two Greek words meaning “recent” and “life.” The Cenozoic is divided into seven periods: Paleocene, Eocene, Oligocene, Miocene, Pliocene, Pleistocene, and finally the Holocene or most recent period, dating back 10,000-12,000 years. The dates for these periods, and the periods comprising the Paleozoic and Mesozoic eras, are given in Table 1.1. These dates are taken from *A Geologic Time Scale*, a recent text on radiometric dating (Harland et al, 1982).

The geological time divisions were largely formulated in the nineteenth century, on the basis of stratigraphic considerations.

Initially, there was no way to assign quantitative dates to these divisions, and thus geologists referred to them qualitatively—a particular period was simply said to be earlier or later than another. In the twentieth century, scientists began to assign quantitative dates by means of radiometric methods, and they have continued to revise these dates periodically up to the present time. Thus today many roughly equivalent systems of dates are used by different geologists and paleontologists,

In general, we will use the dates in Table 1.1 throughout this book. When authors from the nineteenth century or early twentieth century assign a fossil to, say, the Miocene period, we will state that the fossil is from 5 to 25 million years old. The author in question may have had no quantitative estimate of the age of his fossil, or he may have had an estimate quite different from 5 to 25 million years. However, if the modern dates from Table 1.1 are correct for the Miocene, and the early author correctly assigned his fossil to the Miocene on the basis of stratigraphy, then it is valid for us to use the modern dates. We will do this since it helps us compare the old discovery with modern discoveries, which are generally given quantitative radiometric dates,

In some cases, the geological periods assigned to certain strata in the nineteenth century have been revised by modern geologists. For example, some Miocene strata have been reassigned to the Pliocene period. In general, whenever strata in a given locality have been identified, we have tried to look up the periods assigned to them in current geological literature. We have then given dates to these strata on the basis of the modern period assignments,

However, this method is often inadequate for assigning dates to nineteenth century Pliocene and Pleistocene sites, In recent years, dates ranging from 2,7 to 15,0 million years have been assigned to the start of the Pliocene, with many vertebrate paleontologists favoring 10-12 million years, Other scientists have used the potassium-argon method to assign a date of

4,5- 6,0 million years to the start of the Pliocene, and In Table 1.1 this date is listed as 5 million years (Berggren and Van Couvering 1974),

TABLE 1.1 (continued) Eras and Periods

Era

Period

Start of Year

Neozoic

Holocene

.0

Pleistocene

■\*

Pliocene

■i

Miocene

2f

Oligocene

35



Eocene

5f

Paleocene

6f

OZOIC

Cre taceou s

14 -

Jiuawc

213

Iriassi c

245

leozoic

Pemiian

28C

Carbomferou s

-'6-

Devonian

405

The Pliocene-Pleistocene boundary is defined as the base of the Calabrian, a marine stratigraphic subdivision from Italy, and this is now thought to be approximately 1.8 million years old. However, for this book the terrestrial mammalian fauna associated with the Pliocene and Pleistocene are of primary importance, since evidence pertaining to ancient human beings is typically dated on the basis of associated mammalian bones. A key faunal subdivision associated with the Pliocene and Pleistocene is the Villafranchian, which is divided into early, middle, and late sections, with dates ranging from 3.5 - 4.0 million years to 1.0 -1.3 million years. Since many vertebrate paleontologists assigned the Villafranchian entirely to the Pleistocene, the starting date of the Pleistocene was sometimes given as 3.5 - 4.0 million years. At present, however, the Villafranchian is divided between the Pleistocene and Pliocene, and the basal Calabrian date of 1.8-2.0 million years is assigned to the beginning of the Pleistocene (Berggren and Van Couvering 1974).

As a result, the best way to arrive at a quantitative date for a nineteenth century site with Villafranchian (or later) fauna is to refer to modern estimates for the age of that site in years, and we have tried to do this as much as possible.

For sites with pre-Villafranchian fauna, the period will be Early Pliocene or earlier, and it is adequate for the purposes of this book to arrive at a date using Table 1.1 and the period presently assigned to the site.

In this book, we will take the modern system for granted, accepting it, for the sake of argument, as a fixed reference frame to use in studying the history of ancient humans and near humans. However, it is clear on closer examination that this reference frame is by no means fixed, and it may be that further study will reveal as much ambiguity in the evidence for its different time divisions and fossil markers as we have found in the evidence for ancient humans.

Certainly, experts in geology have sometimes expressed dissatisfaction with the established geological time divisions. For example, Edmund M. Spieker (1956, p. 1803) made the following remarks in a lecture delivered to the American Association of Petroleum Geologists: "I wonder how many of us realize that the time scale was frozen in its present form by 1840. . . . How much world geology was known in 1840? A bit of Western Europe, none too well, and a lesser fringe of eastern North America. All of Asia, Africa, South America, and most of North America were virtually unknown. How dared the pioneers assume that their scale would fit the rocks in these vast areas, by far most of the world? Only in dogmatic assumption. . . . And in many parts of the world, notably India and South America, it does not fit. But even there it was applied! The founding fathers went forth across the earth and in Procrustean fashion made it fit the sections they found, even in places where the actual evidence literally proclaimed denial. So flexible and accommodating are the 'facts' of geology."

## 1.8 The Appearance of the Hominids

The first apelike beings appeared in the Oligocene period, which began about 38 million years ago. The first apes thought to be on the line to humans appeared in the Miocene, which extends from 5

to 25 million years ago. These include the dryopithecine ape *Proconsul africanus* and *Ramapithecus*, which is now thought to be an ancestor of the orangutan.

Then came the Pliocene period. During the Pliocene, the first hominids, or erect-walking humanlike primates, are said to appear in the fossil record. The term hominid should be distinguished from hominoid, which designates the taxonomic superfamily including apes and humans. The earliest known hominid is *Australopithecus*, the "southern ape," and is dated back as far as 4 million years, in the Pliocene.

This near human, say scientists, stood between 4 and 5 feet tall and had a cranial capacity of between 300 and 600 cubic centimeters (cc). From the neck down, *Australopithecus* is said to have been very similar to modern humans, whereas the head displayed some apelike and some human features.

One branch of *Australopithecus*, known as the "gracile" or lighter branch, is thought to have given rise to *Homo habilis* around 2 million years ago, at the beginning of the Pleistocene period. *Homo habilis* appears similar to *Australopithecus*

except that his cranial capacity is said to have been larger, between 600 and 750 cc.

*Homo habilis* is thought to have given rise to *Homo erectus* (the species that includes Java man and Peking man) around 1.5 million years ago. *Homo erectus* is said to have stood between 5 and 6 feet tall and had a cranial capacity varying between 700 and 1,300 cc. Most paleoanthropologists now believe that from the neck down, *Homo erectus* was, like *Australopithecus* and *Homo habilis*, almost the same as modern humans. The forehead, however, still sloped back from behind massive brow ridges. The jaws and teeth were large, and the lower jaw lacked a chin. It is believed that *Homo*

*erectus* lived in Africa, Asia, and Europe until about 200,000 years ago.

Paleoanthropologists believe that anatomically modern humans (*Homo sapiens*) emerged gradually from *Homo erectus*. Somewhere around 300,000 or 400,000 years ago the first early *Homo sapiens* or archaic *Homo sapiens* are said to have appeared. They are described as having a cranial capacity almost as large as that of modern humans, yet still manifesting to a lesser degree some of the characteristics of *Homo erectus*, such as the thick skull, receding forehead, and large brow ridges. Examples of this category are the finds from Swanscombe in England, Steinheim in Germany, and Fontchevade and Arago in France. Because these skulls also possess, to some degree, Neanderthal characteristics (Gowlett 1984, p. 85; Brauer 1984, p. 328; Stringer et al. 1984, p. 90), they are also classified as pre-Neanderthal types. Most authorities now postulate that both anatomically modern humans and the classic Western European Neanderthals evolved from the pre-Neanderthal or early *Homo sapiens* types of hominids (Spencer 1984, pp. 1-49).

In the early part of the twentieth century, some scientists advocated the view that the Neanderthals of the last glacial period, known as the classic Western European Neanderthals, were the direct ancestors of modern human beings. They had brains larger than those of *Homo*

sapiens sapiens. Their faces and jaws were much larger, and their foreheads were lower, sloping back from behind large brow ridges. Neanderthal remains are found in Pleistocene deposits ranging from 30,000 to 150,000 years old. However, the discovery of early Homo sapiens in deposits far older than 150,000 years effectively removed the classic Western European Neanderthals from the direct line of descent leading from Homo erectus to modern humans.

The type of human known as Cro-Magnon appeared in Europe approximately 30,000 years ago (Gowlett 1984, p. 118), and they were anatomically modern. Scientists used to say that anatomically modern Homo sapiens sapiens first appeared around 40,000 years ago, but now many authorities, in light of the Border Cave discoveries in South Africa, say that they appeared around 100,000 years ago (Rightmire 1984, pp. 320-321).

The cranial capacity of modern humans varies from 1,000 cc to 2,000 cc, the average being around 1,350 cc. As can be readily observed today among modern humans, there is no correlation between brain size and intelligence. There are highly intelligent people with 1,000 cc brains and morons with 2,000 cc brains. Exactly where, when, or how Australopithecus gave rise to Homo habilis,

or Homo habilis gave rise to Homo erectus, or Homo erectus gave rise to modern humans is not explained in present accounts of human origins. However, one thing paleoanthropologists do say is that only anatomically modern humans came to the New World. The earlier stages of evolution, from Australopithecus on up, are all said to have taken place in the Old World. The first arrival of human beings in the New World is generally said to have occurred some 12,000 years ago, with some scientists willing to grant a Late Pleistocene date of 25,000 years.

Even today there are many gaps in the presumed record of human descent. For example, there is an almost total absence of fossils linking the Miocene apes with the Pliocene ancestors of modern apes and ancestral humans, especially within the span of time between 4 and 8 million years ago.

Perhaps it is true that fossils will someday be found that fill in the gaps. Yet, and this is extremely important, there is no reason to suppose that the fossils that turn up will be supportive of evolutionary theory. What if, for example, fossils of anatomically modern humans turned up in strata older than those in which the dryopithecine apes were found? Even if anatomically modern humans were found to have lived contemporaneously with Dryopithecus (or even a million years ago,

3 million years after the late Miocene disappearance of Dryopithecus), that would be enough to throw the current accounts of the origin of humankind completely out the window.

In fact, such evidence has already been found, but it has since been suppressed or conveniently forgotten. Much of it came to light immediately after Darwin published *The Origin of Species*, before which there had been no notable finds except Neanderthal man. In the first years of Darwinism, there was no clearly established story of human descent to be defended, and professional scientists made and reported many discoveries that now would never make it into the

pages of any Journal more academically respectable than the National Enquirer. Most of these fossils and artifacts were unearthed before the discovery by Eugene Dubois of Java man, the first protohuman hominid between Dryopithecus and modern humans.

Java man was found in Middle Pleistocene deposits generally given an age of 800,000 years. The discovery became a benchmark. Henceforth, scientists would not expect to find fossils or artifacts of anatomically modern humans in deposits of equal or greater age. If they did, they (or someone wiser) concluded that this was impossible and found some way to discredit the find as a mistake, an illusion, or a hoax. Before Java man, however, reputable nineteenth-century scientists found a number of examples of anatomically modern human skeletal remains in very ancient strata. And they also found large numbers of stone tools of various types, as well as animal bones bearing signs of human action.

### 1.9 Some Principles of Epistemology

Before beginning our survey of rejected and accepted paleoanthropological evidence, we shall outline a few epistemological rules that we have tried to follow. Epistemology is defined in Webster's New World Dictionary (1978) as "the study or theory of the origin, nature, methods, and limits of knowledge." When engaged in the study of scientific evidence, it is important to keep the "nature, methods, and limits of knowledge" in mind; otherwise one is prone to fall into a number of illusions.

One important illusion, sometimes called the illusion of "misplaced concreteness," is that a scientific study deals directly with facts, and that scientific arguments appealing to the facts can prove statements about reality. For example, one might suppose that an argument involving facts in the form of fossil bones can prove that anatomically modern humans really did arise in Africa 100,000 years ago. Thinking this, one might strongly argue, on the basis of certain facts, that the statement "anatomically modern humans arose in Africa 100,000 years ago" represents the truth. If the facts are part of reality, and the arguments are sound, then surely the conclusion must be true. Or, at least, granting our human fallibility, we can be reasonably confident that it is true.

The problem here is that in the field of paleoanthropology the facts being considered are not directly part of reality. Indeed, if a "fact" is examined closely it is found to resolve into (1) arguments based on further "facts," or (2) claims that someone has witnessed something at a particular time and place. Thus "facts" turn out to be networks of arguments and observational claims.

To some extent, this is true of the facts discussed in any field of science. But the facts of paleoanthropology have certain key limitations that should be pointed out. First, the observations that go into paleoanthropological facts tend to involve rare discoveries that cannot be duplicated at will. For example, some scientists in this field have built great reputations on the basis of a few famous discoveries, and others, the vast majority, have spent their whole careers without making a single significant find.

Second, once a discovery is made, key elements of the evidence are destroyed, and knowledge of these elements depends solely on the testimony of the discoverers. For example, one of the most important aspects of a fossil is its stratigraphic position. However, once the fossil is removed from the earth, the direct evidence indicating its position is destroyed, and we simply have to depend on the excavator's testimony as to where he or she found it. Of course, one may argue that chemical or other features of the fossil may indicate its place of origin. This is true in some cases but not in others. And in making such judgments, we also have to depend on reports concerning the chemical and other physical properties of the strata in which the fossil was allegedly found.

Persons making important discoveries sometimes cannot find their way back to the sites of those discoveries. After a few years, the sites are almost inevitably destroyed, perhaps by erosion, by complete paleoanthropological excavation, or by commercial developments (involving quarrying, building construction, and so forth). Even modern excavations involving meticulous recording of details destroy the very evidence they are recording, and leave one with nothing but written testimony to back up many key assertions. And many important discoveries, even today, involve very scanty recording of key details.

Thus a person desiring to verify paleoanthropological reports<sup>11</sup> find it very difficult to gain access to the "real facts," even if he or she is able to travel to the site of a discovery. And, of course, limitations of time and money make it impossible to personally examine more than a small percentage of the totality of important paleoanthropological sites.

A third problem is that the facts of paleoanthropology are seldom (if ever) simple. A scientist may testify that "the fossils were clearly weathering out of a certain Early Pleistocene layer." But this apparently simple statement may depend on many observations and arguments involving geological faulting, the possibility of slumping, the presence or absence of a layer of hillwash, the presence of a refilled gully, and so on. If one consults the testimony of another person present at the site, one may find that he or she discusses many important details not mentioned by the first witness.

Different observers sometimes contradict one another, and their senses and memories are imperfect. Thus, an observer at a given site may see certain things, but miss other important things. Some of these things might be seen by other observers, but this could turn out to be impossible because the site has become inaccessible.

Then there is the problem of cheating. This can occur on the level of systematic fraud, as in the Piltdown case. As we shall see, to get to the bottom of this kind of cheating one requires the investigative abilities of a super Sherlock Holmes plus all the facilities of a modern forensic laboratory. Unfortunately, there are always strong motives for deliberate or unconscious fraud, since fame and glory await the person who succeeds in finding a human ancestor.

Cheating can also occur on the level of simply omitting to report observations that do not agree with one's desired conclusions. As we will see in the course of this book, investigators have sometimes admitted that they have observed artifacts in certain strata, but never reported this

because they did not believe the artifacts could possibly be of that age. It is very difficult to avoid this, because our senses are imperfect, and if we see something that seems impossible, then it is natural to suppose that we may be mistaken. Indeed, this may very well be the case. Thus, cheating by omitting to mention important observations can have an important effect on paleoanthropological conclusions, but it cannot be eliminated. It is simply a limitation of human nature that, unfortunately, can have a considerably deleterious impact on the empirical process.

The drawbacks of paleoanthropological facts are not limited to excavations of objects. Similar drawbacks are also found in modern chemical or radiometric dating studies. For example, a carbon 14 date might seem to involve a straightforward procedure that reliably yields a number—the age of an object. But actual dating studies often turn out to involve complex considerations regarding the identity of samples, and their history and possible contamination. They may involve the rejection of some preliminary calculated dates and the acceptance of others on the basis of complex arguments that are seldom explicitly published. Here also the facts can be complex, incomplete, and largely inaccessible.

The conclusion we draw from these limitations of paleoanthropological facts is that in this field of study we are largely limited to the comparative study of reports. Although ‘hard evidence’ does exist in the form of fossils and artifacts in museums, most of the key evidence that gives importance to these objects exists only in written form.

Since the information conveyed by paleoanthropological reports tends to be incomplete, and since even the simplest paleoanthropological facts tend to involve complex, unresolvable issues, it is difficult to arrive at solid conclusions about reality in this field. What then can we do? We suggest that one important thing we can do is compare the quality of different reports. Although we do not have access to the real facts, we can directly study different reports and objectively compare them.

A collection of reports dealing with certain discoveries can be evaluated on the basis of the thoroughness of the reported investigation and the logic and consistency of the arguments presented. One can consider whether or not various skeptical counterarguments to a given theory have been raised and answered. Since reported observations must always be taken on faith in some respect, one can also inquire into the qualifications of the observers.

We propose that if two collections of reports appear to be equally reliable on the basis of these criteria, then they should be treated equally. Both sets might be accepted, both might be rejected, or both might be regarded as having an uncertain status. It would be wrong, however, to accept one set of reports while rejecting the other, and it would be especially wrong to accept one set as proof of a given theory while suppressing the other set, and thus rendering it inaccessible to future students.

We apply this approach to two particular sets of reports. The first set consists of reports of anomalously old artifacts and human skeletal remains, most of which were discovered in the late nineteenth and early twentieth centuries. These reports are discussed in Part I of this book. The

second set consists of reports of artifacts and skeletal remains that are accepted as evidence in support of current theories of human evolution. These reports range in date from the late nineteenth century (the Pithecanthropus of Dubois) to the 1980s, and they are discussed in Part II. Due to the natural interconnections between different discoveries, some anomalous discoveries are also discussed in Part II.

Our thesis is that in spite of the various advances in paleoanthropological science in the twentieth century there is an essential equivalence in quality between these two sets of reports. We therefore suggest that it is not appropriate to accept one set and reject the other. This has serious implications for the modern theory of human evolution. If we reject the first set of reports (the anomalies) and, to be consistent, also reject the second set (evidence currently accepted), then the theory of human evolution is deprived of a good part of its observational foundation. But if we accept the first set of reports, then we must accept the existence of intelligent, toolmaking beings in geological periods as remote as the Miocene, or even the Eocene. If we accept the skeletal evidence presented in these reports, we must go further and accept the existence of anatomically modern human beings in these remote periods. This not only contradicts the modern theory of human evolution, but it also casts grave doubt on our whole picture of the evolution of mammalian life in the Cenozoic era.

In general, if A contradicts B it is not necessary to prove that A is right in order to prove that B is wrong. To discredit B, all that is required is to show that A and B are both equally well supported by arguments and evidence. Then they cancel each other out. That is the case with our two sets of reports.

In making this study, there are a number of basic features of modern geology and paleontology that we are accepting as a fixed reference framework. These are the system of geological time divisions, the modern radiometric dates for these divisions, the succession of faunal types in successive time divisions of the Cenozoic era, and the basic principles of stratigraphy.

It might be argued that if we are going to advocate a conclusion as radical as the one we just mentioned, then we might as well challenge these items as well. After all, if scientists can be completely wrong about the geological time range of human beings, why should we expect them to be right about the time ranges of various mammals?

The answer to this objection is that the various elements in our fixed reference frame may well be in need of reevaluation. However, in this study it would be impractical to delve into these matters in sufficient detail to demonstrate the specific defects that may exist in this geological and paleontological framework. Given the total body of available paleoanthropological evidence, we can only conclude that something must be seriously wrong with our current scientific picture of human evolution.

The point could be made that even if human beings existed in much earlier periods than is currently believed possible, this still does not contradict the theory of evolution. The evolution of humans could simply have taken place at earlier times. Our answer is that the material we are



presenting can be interpreted in that way, and indeed it was so interpreted by most of the scientists who originally presented it. In fact, no matter what evidence is presented for the existence of human beings at a particular date, it is always possible to suppose that they evolved from lower forms at an earlier time.

It can also be said, however, that if the empirical basis for the current view of human evolution proves faulty, then the credibility of evolutionary theory in general is brought into question. After all, if the imposing empirical edifice of evolution from *Australopithecus* to *Homo sapiens* is just a house of cards, then how quick should one be to accept another elaborate evolutionary scheme?

#### 1.10 Theories and Anomalous Evidence

We have spoken of "anomalous evidence" and "evidence accepted in support of modern theory." In general, a piece of evidence is anomalous only in relation to a particular theory. If one could look at the world without any theoretical presuppositions (conscious or unconscious), one would see nothing anomalous. Unfortunately, one would probably experience little but a welter of meaningless sense perceptions, since it is through theoretical understanding that we give meaning to what we perceive.

In this connection a famous remark by Einstein is worth considering: "It may be heuristically useful to keep in mind what one has observed. But on principle it is quite wrong to try grounding a theory on observable quantities alone. In reality the opposite happens. It is the theory which determines what we can observe" (Brush 1974, p. 1167).

If Einstein is right, then as theories change, observations should also change. And this is indeed what we find in paleoanthropology. As we shall see, large amounts of paleoanthropological evidence were amassed in the late nineteenth and early twentieth centuries in support of a theory that humans or near humans were living in the Pliocene, Miocene, or earlier periods. This evidence was not regarded as anomalous by the scientists who introduced it, since they were contemplating theories of human origins (mainly along the lines of Darwinian evolution) that were compatible with this evidence. Then, with the development of the modern theory that humans like ourselves evolved in the Pleistocene, this evidence became highly unacceptable, and it vanished from sight.

One prominent feature in the treatment of anomalous evidence is what we could call the double standard. All paleoanthropological evidence tends to be complex and uncertain. Practically any evidence in this field can be challenged, for if nothing else, one can always raise charges of fraud. What happens in practice is that evidence agreeing with a prevailing theory tends to be treated very leniently. Even if it has grave defects, these tend to be overlooked. In contrast, evidence that goes against an accepted theory tends to be subjected to intense critical scrutiny, and it is expected to meet very high standards of proof.

This double standard is described in the following way by the archaeologist George Carter (1980, p. 318): "When a new idea is advanced, it necessarily challenges the previous idea. This disturbs the

holders of the previous idea and threatens their security. The normal reaction is anger. The new idea is then attacked, and support of it is required to be of a high order of certainty. The greater the departure from the previous idea, the greater the degree of certainty required, so it is said. I have never been able to accept this. It assumes that the old order was established on high orders of proof, and on examination this is seldom found to be true."

Of course, in this study the "new" ideas that we are bringing forward are actually older than the established ideas they contradict. One might say that these old ideas were properly repudiated many years ago, and it is absurd for us to resurrect them today. After all, science has advanced, and the methods we use today are far superior to those used a hundred years ago. For example, today we can date samples using nuclear physics, and the science of taphonomy has been developed to explain how materials are transformed when they are buried.

The answer to this objection is that we cannot accept a priori that the paleoanthropological studies of today are so superior in thoroughness, concept, and methodology to those of a hundred years ago. The existence of new dating methods does not rule out the validity of old stratigraphic studies. Indeed stratigraphy remains an essential tool in paleoanthropology. New methods can also create new sources of error, and some apparently new fields of study (such as taphonomy) were studied extensively in the past using different nomenclature.

The only way to really be sure of the relative value of new and old paleoanthropological reports is to undertake an actual comparative study of these reports, and that is what we attempt to do in this book. Another point, of course, is that anomalous findings are also being made today, and as we shall see, some of these involve the latest paleoanthropological techniques.

In discussing the anomalous and accepted reports in Parts I and II, we have tended to stress the merits of the anomalous reports, and we have tended to point out the deficiencies of the accepted reports. It could be argued that this indicates bias on our part. Actually, however, our objective is to show the qualitative equivalence of the two bodies of material by demonstrating that there are good reasons to accept much of the rejected material, and also good reasons to reject much of the accepted material. It should also be pointed out that we have not suppressed evidence indicating weaknesses in the anomalous findings. In fact, we extensively discuss reports that are highly critical of these findings, and give our readers the opportunity to form their own opinions.

#### 1.11 The Phenomenon of Suppression

As George Carter pointed out, some ideas or observations deviate more than others from an accepted theoretical viewpoint. If a finding is slightly anomalous, it may win acceptance after a period of controversy. If it is more anomalous, it may be studied for some time by a few scientists, while being rejected by the majority. For example, today we see that some scientists, such as Robert Jahn of Princeton University, publish parapsychological studies, while most scientists completely disregard this subject. Finally, there are some observations that so violently contradict accepted theories that they are never accepted by any scientists. These tend to be reported by scientifically uneducated people in popular books, magazines, and newspapers.

As time passes and theories change, the status of anomalous observations also changes. In some cases (as shown, for example, by the theory of continental drift), evidence once considered anomalous may later attain scientific acceptability. In other cases, evidence which was acceptable, or marginally acceptable, may become so anomalous that professional scientists will completely reject it.

This process of rejection does not usually involve careful scrutiny of the evidence by the scientists who reject it. Human time and energy are limited, and most scientists prefer to focus on positive research goals, rather than spend time scrutinizing unpopular claims. In the scientific community, the word will go out that certain findings are bogus, and this is enough to induce most scientists to avoid the rejected material.

When theories change, and a certain body of ideas and discoveries becomes unacceptable, there is generally a period of time during which prominent scientists will publish systematic attacks against the unwanted findings. On the parlance of some scientists at the British museum, these attacks are known as “demolition jobs.”)

If the attacks are successful, then after some last attempts at rebuttal by diehard supporters, scientists <sup>11</sup> realize it is not in their best interest to defend the unwanted material or be associated with it. A shroud of silence descends over the rejected evidence, and it continues to exist only in fossilized form in the moldering pages of old scientific journals. As time passes, a few dismissive mentions may be made in occasional footnotes, and then a new generation of scientists grows up, largely unaware that the earlier evidence ever existed.

This process of suppression of evidence is illustrated by many of the anomalous paleoanthropological findings discussed in this book. This evidence now tends to be extremely obscure, and it also tends to be surrounded by a neutralizing nimbus of negative reports, themselves obscure and dating from the time when the evidence was being actively rejected. Since these reports are generally quite derogatory, they may discourage those who read them from examining the rejected evidence further.

However, the negative reports generally provide many references to earlier positive reports. When these are examined in detail, it is often found that they contain a wealth of detailed information and reasoning not adequately dealt with in the later negative critiques. Thus to properly evaluate anomalous evidence, there is no alternative to examining in detail the arguments and evidence presented in the original reports. And that is what we now propose to do.

#### Incised and Broken Bones: The Dawn of Deception

Intentionally cut and broken bones of animals comprise a substantial part of the evidence for human antiquity. They came under serious study in the middle of the nineteenth century and have remained the object of extensive research and analysis up to the present.

In the decades following the publication of Darwin's *The Origin of Species*, many scientists found incised and broken bones indicating a human presence in the Pliocene, Miocene, and earlier

periods. Opponents suggested that the marks and breaks observed on the fossil bones were caused by the action of carnivores, sharks, or geological pressure. But supporters of the discoveries offered impressive counterarguments. For example, stone tools were sometimes found along with incised bones, and experiments with these implements produced marks on fresh bone exactly resembling those found on the fossils. Scientists also employed microscopes in order to distinguish the cuts on fossil bones from those that might be made by animal or shark teeth. In many instances, the marks were located in places on the bone appropriate for specific butchering operations.

Nonetheless, reports of incised and broken bones indicating a human presence in the Pliocene and earlier are absent from the currently accepted stock of evidence. This exclusion may not, however, be warranted. From the incomplete evidence now under active consideration, scientists have concluded that humans of the modern type appeared fairly recently. But in light of the evidence covered in this chapter, the soundness of that conclusion is somewhat deceptive.

## 2.1 St. Prest, France (early Pleistocene or Late Pliocene)

Just above the famous cathedral town of Chartres in northwestern France, at St. Prest, in the valley of the Eure River, there are gravel pits, where, in the early nineteenth century, workmen occasionally turned up fossils. These were first reported to the scientific world in 1848 by Monsieur de Boisvillotte, the engineer in charge of the local bridges and causeways. The numerous fossils, including many extinct animals such as *Elephas meridionalis*, *Rhinoceros tapiroideus*, *Rhinoceros etruscus*, *Hippopotamus major*, and a giant beaver called *Trogontherium cimeri*, were judged to be characteristic of the Late Pliocene (de Mortillet 1883, pp. 28-29).

A further indication of the fossils' great antiquity was the fact that the gravels in which they were found lay at an elevation of 25 to 30 meters [82 to 98 feet] above the present level of the Eure, where an ancient river once ran in a different bed. The geological reasoning is as follows. When rivers cut valleys into a plain, the most recent gravels will normally be found near the bottom of the valley. Gravels found further up on the sides of the valley were deposited earlier by the same river, or other rivers, before the valley reached its present depth. The higher the gravels, the greater their age.

In April of 1863, Monsieur J. Desnoyers, of the French National Museum, came to St. Prest to gather fossils. From the sandy gravels he recovered part of a rhinoceros tibia, upon which he noticed a series of narrow grooves, longer and deeper than could have resulted from minor fracturing or weathering. To Desnoyers, some of the grooves appeared to have been produced by a sharp knife or blade of flint. He also observed small circular marks that could well have been made by a pointed implement (de Mortillet 1883, p. 43). Later, upon examining collections of St. Prest fossils at the museums of Chartres and the School of Mines in Paris, Desnoyers recognized upon a diverse assortment of bones the same types of marks. He then reported his findings to the French Academy of Sciences, maintaining that while some of the marks could possibly be attributed to glacial action others were definitely the work of humans.

If Desnoyers concluded correctly that the marks on many of the bones had been made by flint implements, then it would appear that human beings had been present in France before the end of the Pliocene period. One might ask, "What's wrong with that?" In terms of our modern understanding of paleoanthropology, quite a bit is wrong. The presence at that time in Europe of beings using stone tools in a sophisticated manner would seem almost impossible. It is believed that at the end of the Pliocene, about 2 million years ago, the modern human species had not yet come into being. Only in Africa should one find primitive human ancestors, and these were limited to *Australopithecus* and *Homo habilis*, considered the first toolmaker.

At this point, some will inevitably question whether the nineteenth-century scientists were correct in assigning the St. Prest site to the Late Pliocene. The short answer to this question is a qualified yes,

As we mentioned in our discussion of the geological time periods in the previous chapter (Section 1.7), the dating of sites at the Pliocene-Pleistocene boundary remains a matter of intense controversy. Since the St. Prest site lies roughly in this period, one might expect various authorities to place it differently. And it turns out that this is in fact the case,

The American paleontologist Henry Fairfield Osborn (1910, p. 391) placed St. Prest in the Early Pleistocene. In times closer to our own, Claude Klein (1973, pp. 692-693) reviewed French opinion regarding the age of the St. Prest fauna. In 1927, Charles Deperet characterized St. Prest as Late Pliocene. G. Denizot placed St. Prest in the Cromerian interglacial stage of the Middle Pleistocene, a view he consistently maintained into the late 1960s. In 1950, P. Pinchemel referred St. Prest to the Late Pliocene. More recently, F. Boudier, in 1965, placed St. Prest in the Waalian temperate stage of the late Early Pleistocene, with a quantitative date of about 1 million years (Klein 1973, p. 736).

Others have arrived at different quantitative dates for St. Prest. Tage Nilsson (1983, p. 158) stated that two sites in the Central Massif region of France, Sainzelles and Le Coupet, yielded potassium-argon dates of 1.3-1.9 million years. Nilsson (1983, p. 158) then said: "St. Prest, near Chartres in northern France, is held to be closely related." Nilsson considered the three sites Late Villafranchian, or Early Pleistocene.

Let us now consider some of the species that were listed as present at St. Prest. *Elephas meridionalis* (sometimes called *Mammifhus meridionalis*) is said by modern authorities (Maglio 1973, p. 79) to have existed in Europe from about 1.2 million to 3.5 million years ago. Osborn (1910, p. 313) places *Rhinoceros (Dicerorhinus) leptorhinus* in the Plaisancian (or Piacenzian) age of the Pliocene. Osborn placed the Plaisancian age in the Early Pliocene, but Romer (1966, p. 334) places the Plaisancian in the Late Pliocene. *Rhinoceros (Dicerorhinus) elruscus*, according to Nilsson (1983, p. 475), occurs in Europe from the Villafranchian, which begins in the Late Pliocene, to the early Middle Pleistocene. But Savage and Russell (1983, p. 339) list occurrences of *Dicerorhinus elruscus* as early as the Ruscinian age of the Early Pliocene. According to Osborn (1910, p. 313), *Hippopotamus major*, a larger version of the modern hippopotamus, is found in the Late Pliocene and throughout the Pleistocene in Europe. *Hippopotamus major* is sometimes

referred to as *Hippopotamus amphibius antiquus*, This species is listed by Savage and Russell (1983, p. 351) as part of the Pliocene Villafranchian fauna of Europe. *Trogontherium cuvieri*, the giant extinct beaver, is found in Pliocene faunal lists (Savage and Russell 1983, p. 352) and persisted until the Mosbachian age of the early Middle Pleistocene (Osborn 1910, p. 403). Thus all the above species were in existence during the Pliocene period.

Add it all up, and it can be seen that a Late Pliocene date for St. Prest is not out of the question. And, as noted previously, some twentieth-century scientists (Pinchemel and Deperet) have in fact assigned St. Prest to this period. That would place toolmaking hominids in Europe at over 2 million years ago.

How recent could St. Prest possibly be? The presence of *Elephas meridionalis*, which survived in Europe until 1.2 million years ago (Maglio 1973, p. 79) would appear to impose a late Early Pleistocene limit. The potassium-argon dates of 1.3-1.9 million years for French sites having a fauna similar to that of St. Prest (Nilsson 1983, p. 158) offer another guidepost. Kurten (1968, p. 24), like Boudier (1965), assigns St. Prest to the Waalian temperate stage of the Early Pleistocene. Some authorities place the Waalian stage at about 1.1-1.2 million years (Nilsson 1983, p. 144). But Seneze, a French site tentatively attributed to the Waalian temperate stage, is estimated to be about 1.6 million years old (Nilsson 1983, p. 158). From all this, one could conclude that the St. Prest site, at the more recent end of its probable date range, might be just 1.2-1.6 million years old. Even at this date, incised bones would still be anomalous. The oldest undisputed evidence for the presence of *Homo erectus* in Europe dates back only about 700,000 years (Gowlett 1984, p. 76). Also, the oldest occurrences of *Homo erectus* in Africa have dates of about 1.5 million years.

Even in the nineteenth century, Desnoyers's discoveries of incised bones at St. Prest provoked controversy. Professor Bayle, a paleontologist at the School of Mines, responded to Desnoyers's report by claiming that it was he, with his own instruments, who had incised and otherwise marked the bones of St. Prest during the process of cleaning them. Dr. Eugene Robert accepted this explanation and communicated it to the French Academy of Sciences.

In response, Desnoyers (1863) protested that his careful scientific presentation had been attacked by means of a brief rumorlike report, submitted without any credible evidence. To his accusers, Desnoyers went on to reply, in a paper published in the proceedings of the French Academy of Sciences, that the bones of St. Prest, found in sand, did not require metal instruments in order to be cleaned. Furthermore, the grooves and other markings were visible on bones that had not needed any kind of cleaning whatsoever. Perhaps the professor of paleontology at the School of Mines, Dr. Bayle, truly had been sufficiently clumsy to have extensively damaged the valuable bones under his care. But Desnoyers did not believe anyone could say the same of the many capable and careful collectors who also had specimens of fossil bones from St. Prest bearing the exact same striations and incisions. In the words of Desnoyers (1863, p. 1201): "Let us admit, against all probability, that the memoir of the preparator and conservator of the collection is true, and all the bones of St. Prest in his possession have been subjected to the kind of alteration to which he pleads guilty. Very well. That assertion itself serves to demonstrate the action of the

hand of man on all the other bones from the same locality, which, fortunately, have been preserved in other collections, from dangerous influences. The marks on them are incontestably primitive, and are completely identical to those produced by the chisels and burins of the functionary of the School of Mines. ”

Desnoyers (1863, p. 1201) was further annoyed that persons who had never even seen the bones claimed that the impressions on them were made by the tools of the workmen in the St. Prest sand pits. He pointed out that this supposition is clearly disproved by the fact that the grooves were covered with the same magnesium deposits and dendrites found on other sections of the bone. Dendrites are crystalline mineral deposits that form branching treelike patterns. If the cuts on the fossil bones had been made by the tools of modern excavators or museum employees, the dendrites would have been scraped away. In some cases, the grooves and marks were still tightly filled with compacted sand from the deposits in which they were discovered.

Desnoyers (1863, p. 1201) suggested that doubters examine the actual specimens: "One would see that the incisions, which furrow the bones across their width and cut their edges, are frequently crossed by the longitudinal cracks resulting from dessication. These cracks were unquestionably produced after the marks made when the bone was fresh: they were produced during the course of fossilization. The distinct characteristics of these two kinds of markings are proof that the one is older than the other. ”

Recent tool marks probably would have cut through the dessication markings in recognizable fashion, erasing the lighter and shallower cracks. Desnoyers's careful analysis foreshadows the modern discipline of taphonomy, the scientific study of the changes undergone by bone and other objects in the course of entombment and fossilization.

About one of his finds, Desnoyers (1863, p. 1201) noted, "One would see on the horn of a giant deer a large incision at the base, an incision difficult to distinguish from those found on the horns of deer from caverns of later geological eras." In other words, the incision on the deer horn was placed appropriately for a human cut mark.

The prominent British geologist Charles Lyell agreed that the St. Prest gravel beds were of Pliocene age. He observed, however, that among the fauna was the large extinct beaver, *Trogontherium*, and asked how one could be certain it was not the teeth of this animal that produced the marks on the fossil bones (Lyell 1863, appendix p. 4). Gabriel de Mortillet, professor of prehistoric anthropology at the Ecole d'Anthropologie in Paris, stated in his book *Le Préhistorique* (1883, p. 45) that Lyell's supposition was inadmissible because the marks on the bones of St. Prest were not at all of the character of those of a rodent's teeth. In particular, they were too narrow to have been made by the strong and powerful incisors of *Trogontherium*.

De Mortillet had his own ideas about the cause of the marks on the fossil bones of St. Prest. Some authorities had suggested glaciers had been responsible for the markings. But de Mortillet said that glaciers had not reached that particular region of France. Modern authorities (Nilsson 1983, p. 169) agree on this point—the extreme southern limit of the North European glaciation passed

through the Netherlands and Central Germany. De Mortillet also rejected human action as the cause of the marks on the bones.

The key to understanding the marks, according to de Mortillet, could be found in the statement by Desnoyers that they appeared to have been made by a sharp blade of flint. According to de Mortillet (1883, pp. 45-46), that was true, only the flint, instead of being moved by the hand of man, had been moved by natural force—a very strong underground pressure that caused the sharp flints to slide across the bones with force sufficient to cut them. As evidence, de Mortillet cited the fact that he had observed flints from the St. Prest gravels and elsewhere that displayed on their surfaces deep scratches. At this point it should be mentioned that in *Le Préhistorique* de Mortillet rejected every single one of the many discoveries of incised bones made up to that time, almost always offering the same explanation—that the marks were caused by sharp stones moved by subterranean geological pressures.

But in the case of the St. Prest bones, Desnoyers (1863, p. 1201) responded to de Mortillet's objections, observing: "many of the incisions have been worn by later rubbing, resulting from transport or movement of the bones in the midst of the sands and gravels. The resulting markings are of an essentially different character than the original marks and striations, and offer superabundant proof of their different ages." In other words, marks from subterranean pressure may indeed be found upon the bones, but, according to Desnoyers, they can be clearly distinguished from the earlier marks attributed to human action.

So who was right, Desnoyers or de Mortillet? Some authorities believed the question could be settled if it could be demonstrated that the gravels of St. Prest contained flint tools that were definitely of human manufacture. This same demand—for the tools that made the marks—is often made today in cases of anomalous discoveries of incised bones (Section 2.3). The Abbe Bourgeois, a clergyman who had also earned a reputation as a distinguished paleontologist, carefully searched the strata at St. Prest for such evidence. By his patient research he eventually found a number of flints that he believed were genuine tools and made them the subject of a report to the Academy of Sciences in January 1867 (de Mortillet 1883, p. 46). Even this did not satisfy de Mortillet (1883, pp. 46-47), who said of the flints discovered by Bourgeois at St. Prest: "Many others that he found there, and which are now deposited in the collection of the School of Anthropology, do not have conclusive traces of human work. The slidings and pressures that resulted in striations on the surfaces of the flints have also left on their sharp edges a number of chips that greatly resemble retouching by humans. This is what deceived Bourgeois. In effect, of the flints discovered at St. Prest, many present a false appearance of having been worked."

It appears that in our attempt to answer one question, the nature of cut marks on bones, we have stumbled upon another, the question of how to recognize human workmanship on flints and other stone objects. This latter question shall be fully treated in the next chapter. For now we shall simply note that judgements about what constitutes a stone tool are a matter of considerable controversy even to this day. It is, therefore, quite definitely possible to find reasons to question de Mortillet's rejection of the flints found by Bourgeois. Certainly, the bare observation that some



of the flints collected by Bourgeois did not, In de Mortillet's opinion, show signs of human work does not change the fact that others, however few, did in fact show such signs. And the presence of stone tools at St. Prest would satisfy a key demand for the verification of intentional cuts on fossil bones found there.

The famous American paleontologist Henry Fairfield Osborn (1910, p. 399) made these interesting remarks in connection with the presence of stone tools at St. Prest: "the earliest traces of man in beds of this age [Early Pleistocene by his estimation] were the incised bones discovered by Desnoyers at St. Prest near Chartres in 1863. Doubt as to the artificial character of these incisions has been removed by the recent explorations of Laville and Rutot, which resulted in the discovery of eolithic flints, fully confirming the discoveries of the Abbe Bourgeois in these deposits in 1867."

So as far as the discoveries at St. Prest are concerned, it should now be apparent that we are dealing with paleontological problems that cannot be quickly or easily resolved. Certainly there is not sufficient reason to categorically reject these bones as evidence for a human presence in the Pliocene. This might lead one to wonder why the St. Prest fossils, and others like them, are almost never mentioned in textbooks on human evolution, except in rare cases of brief mocking footnotes of dismissal. Is it really because the evidence is clearly inadmissible?

Or is, perhaps, the omission or summary rejection more related to the fact that the potential Late Pliocene antiquity of the objects is so much at odds with the standard account of human origins? In theory, scientists proclaim themselves ready to follow the facts wherever they might lead. But in practice, the social mechanisms of the scientific community set limits beyond which its members in good standing may cross only at their peril. When eminent authorities announce their rejection of certain categories of evidence, others hesitate to mention similar evidence out of fear of ridicule. Thus anomalous evidence gradually slides from dispute into complete oblivion.

Along these lines, Armand de Quatrefages, a member of the French Academy of Sciences and a professor at the Museum of Natural History in Paris, wrote in his book *Hommes Fossiles et Hommes Sauvages* (1884, p. 90): "The objections made to the existence of humans in the Pliocene and Miocene periods seem to habitually be more related to theoretical considerations than to direct observation." De Quatrefages (1884, p. 91) further stated: "The existence of man in the Secondary epoch is not at all contrary to the principles of science, and the same is true of Tertiary man."

This is quite a shocking statement, considering that the most recent Secondary period is the Cretaceous, which ended approximately 65 million years ago. Supposedly, only very small and primitive mammals existed in the Cretaceous, dodging the last of the dinosaurs. Evidence for human beings in the Cretaceous would most certainly cast a great thundering cloud of doubt over Darwin's seemingly invincible hypothesis. But for now, our focus is on the more recent Tertiary epoch. Even if anatomically modern human beings were found to have existed in the latest Pliocene, at a mere 2 million years ago, that would still call into question the evolutionary picture of human origins.

In *Hommes Fossiles et Hommes Sauvages*, de Quatrefages gave a summary of the evidence for his assertions about humans existing in the very distant past and then stated (1884, p. 96): 'The preceding historical samples are incomplete and abbreviated. But they suffice. I believe, to make comprehensible that the conviction, agreed upon by many modern scientists of diverse disciplines, relative to the existence of Tertiary man, is not formed lightly but is the result of serious and repeated study.'

Concerning the presence of ancient man at St. Prest, de Quatrefages (1884, pp. 89-90) wrote: "Mr. Desnoyers has affirmed his existence, based on the examination of incisions manifestly intentional found on the bones of *Elephas meridionalis* and other great mammals of the same age. This discovery was greatly contested, by among others Lyell, who declared he was not able to accept that the incisions on the bones were demonstrably the work of man until he could be shown the instruments that did it. The Abbe Bourgeois responded to this desire. But 20 years later, de Mortillet, in opposing all the results of this research, simply raises objections which when made the object of attentive study turn out to have little foundation."

Elsewhere in *Hommes Fossiles et Hommes Sauvages*, de Quatrefages (1884, p. 17) succinctly reaffirmed the evidence for the presence of humans in the Pliocene at St. Prest: "The researches of Mr. Desnoyers and the Abbe Bourgeois do not leave any doubt in this regard. Mr. Desnoyers first discovered in 1863, on bones found in the gravel pits of St. Prest, near Chartres, imprints that he did not hesitate to report as being made by the action of flint implements in the hands of human beings. A little later, the Abbe Bourgeois confirmed and completed this important discovery when he found in the same place the worked flints that had made the incisions on the bones of *Elephas meridionalis*, *Rhinoceros leptorhinus*, and other animals. I have examined at leisure the bones studied by Desnoyers, as well as the scrapers, borers, lance points, and arrowheads collected by the Abbe Bourgeois. From the start, I have had little doubt, and everything has been confirming that first impression. Thus man lived on the globe at the end of the Tertiary era. And he left traces of his industry; he had at this time both arms and tools. The honor of the first recognition of this fact, so little in accord with all that was believed only a short time ago, goes incontestably to Mr. Desnoyers."

Here it should be noted that it would of course be possible to more briefly summarize and paraphrase reports such as these.

There are two reasons for not doing so. The first is that paleoanthropological evidence mainly exists in the form of reports, some primary and others secondary. Very few individuals, even experts in the field, have the opportunity to engage in firsthand inspection of the fossils themselves, scattered in collections around the world. Even if one is able to do so, one is still not able to be sure about the exact circumstances of the discovery. This is critical, because the interpretation of the significance of a fossil depends as much on the exact position in which it was found as on the fossil itself. In most cases, for all investigators except the original discoverers, the real evidence is the reports themselves, which give the details of the discovery, and we shall therefore take the trouble to include many selections from such reports, the exact wording of

which reveals much. Contemporary discussions of these original reports, both those which are positive and those which are negative, are also illuminating.

A second consideration is that the particular reports referred to in this chapter are extremely difficult to obtain. Almost no reference to them will be found in modern textbooks. Most of them come from rare nineteenth-century paleontological and anthropological books and journals, the majority in languages other than English. This being the case, translated excerpts of the original reports have been judged preferable to paraphrases and footnotes, and will serve as a unique introduction to a vast store of buried evidence.

A final consideration is that proponents of evolutionary theory often accuse authors who arrive at nonevolutionary conclusions of "quoting out of context." It therefore becomes necessary to quote at length, in order to supply the needed context.

The controversy over the St. Prest finds was noted by S. Laing, a popular British nonfiction author of the late nineteenth century, whose well-researched books on scientific subjects, intended for the general public, reached a wide audience. After discussing the site at St. Prest, Laing (1893, p. 113) stated: "In these older gravels have been found stone implements, and bones of the *Elephas meridionalis* with incisions evidently made by a flint knife worked by a human hand. This was disputed as long as possible, but Quatrefages, a very cautious and competent authority, states in his latest work, published in 1887, that it is now established beyond the possibility of doubt."

## 2.2 A Modern example: Old Crow River, Canada (Late Pleistocene)

Before moving on to further examples of nineteenth-century discoveries that challenge modern ideas about human origins, let us consider a more recent investigation of intentionally modified bones. One of the most controversial questions confronting New World paleoanthropology is determining the time at which humans entered North America. The standard view is that bands of Asian hunter-gatherers crossed over the Bering land bridge about 12,000 years ago. Some authorities are willing to extend the date to about 30,000 years ago, while an increasing minority are reporting evidence for a human presence in the Americas at far earlier dates in the Pleistocene. We shall examine this question in greater detail in coming chapters (Sections 3.8, 4.8, 5.1, 5.2, 5.4, and 5.5). For now, however, we want only to consider the fossil bones uncovered at Old Crow River in the northern Yukon territory as a contemporary example of the type of evidence dealt with in this chapter.

In the 1970s, Richard E. Morlan of the Archeological Survey of Canada and the Canadian National Museum of Man, conducted studies of modified bones from the Old Crow River sites. Morlan concluded that many bones and antlers exhibited signs of intentional human work executed before the bones had become fossilized. The bones, which had undergone river transport, were recovered from an Early Wisconsin glacial floodplain dated at 80,000 years b.p. (before present).

But R. M. Thorson and R. D. Guthrie (1984) published a taphonomic study showing that the action of river ice could have caused the alterations that suggested human work to Morlan. Thorson and

Guthrie performed experiments in which large blocks of ice containing bones frozen within them were dragged behind trucks over various surfaces, reproducing the effect of river ice scraping against rocks and gravels. In a 1986 reappraisal of his previous work, Morlan, considering the taphonomic experiments of Thorson and Guthrie, admitted "the observed effects are impressive for the hazards they might pose to recognition of artificial alterations among redeposited fossils." He went on to note: "However some critical variables probably were not simulated adequately (e.g., texture and hardness of the substrate, buoyancy of the ice block), and it is noteworthy that many of the experimental bones are more profoundly altered than those recovered from natural environments. Certainly these experiments have not shown that all the altered fossils from Old Crow Basin can be attributed to river icing and breakup" (Morlan 1986, p. 29).

Nevertheless, Morlan did in fact back away, in almost all cases, from his earlier assertions that the bones he had collected had been modified by human agency. He gave alternate explanations, such as the river ice hypothesis, but cautioned: "The alternate interpretations do not prove that humans were not present in Early Wisconsinan time, but they show that such ancient presence of people cannot be demonstrated on the basis of evidence gathered thus far" (Morlan 1986, p. 27). He went on to say: "This conclusion differs from earlier statements, but it is not necessarily a retraction of those statements. I have definitely changed my mind about some of my earlier interpretations, but in most cases I am simply trying to enlarge our conceptual framework and to stimulate further observations and discussions" (Morlan 1986, pp. 28-29).

But even though Morlan recanted his previous assertions of human work on 30 bone specimens, he believed four others still bore signs of being definite human artifacts. At Johnson Creek, near Old Crow valley, he found a "freshfractured *Bison* sp. radius" in situ. The radius is one of the long bones of the lower forelimb. "Although it is not out of the question that the bison bone was broken by carnivores," stated Morlan (1986, p. 36), "its massive size and micro-relief features indicative of dynamic fracture suggest that it was broken by man. The enclosing matrix of organic silt is suggestive of a thaw-lake deposit and yields a date of >37,000 b.p."

At another locality, Morlan found two large mammal long bones and a bison rib, all three bearing incisions. Morlan (1986, p. 36) stated about these three bones and the bison radius discussed in the previous paragraph: "The cuts and scrapes ... are indistinguishable from those made by stone tools during butchering and defleshing of an animal carcass. These four specimens comprise the most formidable barrier to a global dismissal of our supposed Early Wisconsinan archaeological record."

Morlan (1986, p. 36) then added: "While this paper was in press ... two cut bones ... were sent to Dr. Pat Shipman, Johns Hopkins University, for examination under the scanning electron microscope. The marks were examined with reference to a collection of more than 1000 documented marks on bones, and the provenience [source] of the specimens was not made known until after the marks had been identified. The surface of the large mammal long bone fragment is damaged and difficult to evaluate, but Dr. Shipman positively identified the mark on

the Bison rib as a tool mark." Morlan (1986, p. 28) noted that stone implements have been found in the Old Crow River area and in nearby uplands, but not in direct association with bones.

What this all means is that the bones of St. Prest, and others like them, cannot be so easily dismissed. Evidence of the same type is still considered important today, and the methods of analysis are almost identical to those practiced in the nineteenth century. De Quatrefages and other scientists of that era compared specimens of cut bone with bones bearing undisputed signs of human workmanship. They also performed experiments on fresh bone. Like modern students of taphonomy, they gave detailed consideration to the changes that bones would undergo during the process of entombment and fossilization. They examined bones with a microscope. It should be noted that an electron microscope is not required for such study. A modern authority, John Gowlett (1984, p. 53), said: "Under a microscope, marks made by man are distinguishable in various ways from those made by carnivores. Dr. Henry Bunn (University of California) observed through an optical microscope at low magnification that stone tools leave V-shaped cuts, which are much narrower than rodent gnawing marks."

As the Old Crow River case clearly shows, modern scientists use methods not much different from those practiced in the nineteenth century. We can just picture Thorson and Guthrie, in previous nineteenth-century incarnations, driving a horsedrawn cart, rather than a truck, and dragging behind them a big block of ice filled with bones over a rough gravel road in northern France, trying to prove the bones of St. Prest were marked by natural forces. Amusing as the image may appear, this is the type of technologically unsophisticated yet important work that still goes into resolving questions about incised bones. But as Morlan's study shows, all questions about the Old Crow bones have not been clearly decided one way or another. He changed his mind about some of his specimens, but remained convinced about others. This ambiguity and inconclusiveness is typical of the empirical approach to such evidence.

In addition to debating whether or not the cut marks on the Old Crow bones were made by stone tools or natural forces, scientists were concerned about the age of the bones. If the bones were seen as bearing signs of human work and if they were also dated to the Early Wisconsin period, that would challenge the date for the earliest entry of humans into North America. The view now dominant is that Siberian hunters crossed the Bering Strait land bridge in the latest Pleistocene and passed through an ice-free corridor into what is now the United States about 12,000 years ago. Nevertheless, as we shall see throughout this book, there is a lot of controversial, hotly debated evidence showing that human beings were present in the Americas far before 12,000 years ago. Those scientists favoring the 12,000-year date tend to believe the marks on the Old Crow bones were caused by geological action of some kind, even though the marks have in some cases been judged identical to those caused by stone tools. This is something we shall encounter again and again. Similarly, preconceptions about the relatively recent origin of anatomically modern humans often influence scientists to reject evidence that they would otherwise take as proof of a human presence.

### 2.3 The Anza-Borrego Desert, California (Middle Pleistocene)

Another recent example of incised bones like those found at St. Prest, again related to the presence of humans in the New World, is a discovery made by George Miller, curator of the Imperial Valley College Museum in El Centro, California. Miller, who died in 1989, reported that six mammoth bones excavated from the Anza-Borrego Desert bear scratches of the kind produced by stone tools. Uranium isotope dating carried out by the U.S. Geological Survey indicated that the bones are at least 300,000 years old, and paleo-magnetic dating and volcanic ash samples indicated an age of some

750,000 years (Graham 1988).

One established scholar said that Miller's claim is "as reasonable as the Loch Ness Monster or a living mammoth in Siberia," while Miller countered that "these people don't want to see man here because their careers would go down the drain" (Graham 1988). Here, perhaps, we see preconceptions influencing the established scholar to reject evidence which, if given a more suitably recent date, he might have accepted.

The incised mammoth bones from the Anza-Borrego Desert came up in a conversation we had with Thomas Demere, a paleontologist at the San Diego Natural History Museum (May 31, 1990). Demere said he was by nature skeptical of claims such as those made by Miller. He called into question the professionalism with which the bones had been excavated, and pointed out that no stone tools had been found along with the fossils. Furthermore, Demere suggested that it was very unlikely that anything about the find would ever be published in a scientific journal, because the referees who review articles probably would not pass it. We later learned from Julie Parks, the present curator of George Miller's specimens, that Demere had never inspected the fossils or visited the site of discovery, although he had been invited to do so (Parks, personal communication, June 1, 1990).

As of June 1990, the Anza-Borrego mammoth bones were still under study. Deposits of sandy matrix were being painstakingly removed from the incisions on the bones, so that the incisions could be examined by a scanning electron microscope. Hopefully, inspection of the minute striations on the surfaces of the cuts under high magnification will confirm whether or not they are characteristic of stone tools. Parks (personal communication, June 1, 1990) said that one incision apparently continues from one of the fossil bones to another bone that would have been located next to it when the mammoth skeleton was articulated. This is suggestive of a butchering mark. Accidental marks resulting from movement of the bones in the earth after the skeleton had broken up probably would not continue from one bone to another in this fashion.

The lesson to be learned from the marked bones found at Old Crow River and in the Anza-Borrego Desert is this: the marked bones of St. Prest and others like them discovered in the nineteenth century should be kept in the active file of paleoanthropological evidence. Even today, scientists are not always able to immediately determine whether or not marks on bones were made by natural forces, animals, or humans. Much careful study and analysis is required to arrive at a conclusion, and even then not all experts will agree. Therefore the marked bones discussed in this chapter and the reports about them should be seriously examined, and be available for

reexamination. If fossils do not pass the test of a certain investigator or school of investigators at a particular point in time, they should not be cast into the outer darkness, so that later researchers will not even know they exist. Rather they should be placed in a category of disputed evidence. In that way, in the event of improvements in the methods of analysis or changes in theoretical constructs of human prehistory, the evidence <sup>11</sup> be available for further study. Who knows? In the future, new pieces to the puzzle of human origins may give new meaning to old pieces that previously did not quite fit.

#### 2.4 Val D'arno, Italy (early Pleistocene or late Pliocene)

Specimens incised in a manner similar to those of St. Prest were found by Desnoyers in a collection of bones gathered from the valley of the Arno River (Val d'Arno) in Italy. The grooved bones were from the same types of animals found at St. Prest—including *Elephas meridionalis* and *Rhinoceros etruscus*. They were attributed to the Late Pliocene stage called the Astian (de Mortillet 1883, p. 47). This would yield a date of 2.0-2.5 million years. Some authorities (Harland et al. 1982, p. 110) put the Astian in the Middle Pliocene, at 3- 4 million years ago.

Modern scientists divide the fauna from the Val d'Arno into two groups— the Upper Valdarno and Lower Valdarno. The Upper Valdarno is assigned to the Late Villafranchian, which is given a quantitative date of 1.0-1.7 million years (Nilsson 1983, pp. 308-309). The Lower Valdarno is placed in the Early Villafranchian, or Late Pliocene, at around 2.0-2.5 million years ago (Nilsson 1983, pp. 308-309).

It is not clear to which group the incised bones reported by Desnoyers belong. But the fact that de Mortillet referred them to the Astian stage of the Late Pliocene seems to indicate that they might be assigned to the Lower Valdarno. On faunal grounds this would not be out of the question. We know that *Elephas meridionalis* occurs in the Lower Valdarno (Maglio

1973, p. 56). As mentioned in our discussion of St. Prest, *Rhinoceros (Dicerorhinus) etruscus* is reported in the Late Pliocene (Nilsson 1983, p. 475) in Europe, and even as far back as the Early Pliocene (Savage and Russell 1983, p. 339). De Mortillet listed *Equus amensis* as present at Val d'Arno. *Equus* is typical of Pleistocene faunal assemblages, but examples of *Equus* are known from the Early Villafranchian (Kurten 1968, p. 147), which is generally thought to extend into the Late Pliocene.

#### 2.5 San Giovanni, Italy (late Pliocene)

In addition, grooved bones also were discovered in other parts of Italy. On September 20, 1865, at the meeting of the Italian Society of Natural Sciences at Spezzia, Professor Ramorino presented bones of extinct species of red deer and rhinoceros bearing what he believed were human incisions (de Mortillet 1883, pp. 47-48). These specimens were found at San Giovanni, in the vicinity of Siena, and like the Val d' Arno bones were said to be from the Astian stage of the Pliocene period. De Mortillet (1883, p. 48), not deviating from his standard negative opinion,

stated that he thought the marks were most probably made by the tools of the workers who extracted the bones.

## 2.6 Rhinoceros of Billy, France (Middle Miocene)

On April 13, 1868, A. Laussedat informed the French Academy of Sciences that P. Bertrand had sent him two fragments of a lower jaw of a rhinoceros. They were from a pit near Billy, France. One of the fragments had four very deep grooves on it. These grooves, situated on the lower part of the bone, were approximately parallel and inclined at a 40-degree angle to the longitudinal axis of the bone. They were 1-2 centimeters (a half inch or so) in length, and the deepest was 6 mm (a quarter inch) in depth (Laussedat 1868, p. 752). According to Laussedat, the cut marks appeared in cross section like those made by a hatchet on a piece of hard wood. And so he thought the marks had been made in the same way, that is, with a handheld stone chopping instrument, when the bone was fresh. That indicated to Laussedat (1868, p. 753) that humans had been contemporary with the fossil rhino in a geologically remote time.

Just how remote is shown by the fact that the jawbone was found in a calcareous sand stratum at a depth of 8 meters (26 feet), in between other strata of the Mayencian age of the Middle Miocene. Furthermore, the incised Jawbone was from a species, *Rhinoceros pleuroceros*, judged by Laussedat to be characteristic of the Early Miocene. According to modern authorities (Savage and Russell 1983, p. 214), *Rhinoceros (Dicerorhinus) pleuroceros* occurs in the Agenian land mammal age of the Early Miocene.

At the meeting of the Academy of Sciences, Mr. Hebert asked if one could be sure of the authenticity of the incisions on the fossil. Edouard Lartet responded with a demonstration that the marks, the surfaces of which had the same appearance as the other parts of the bone, indeed dated from the time of burial (de Mortillet 1883, p. 49).

By what agency were the marks produced? De Mortillet (1883, p. 50) rejected straightaway the idea of gnawing by carnivores, because the incisions did not display the appropriate characteristics. Animal gnawing tends to be accompanied by significant destruction of the bone, whereas the rhinoceros jawbone from Billy bore only the four rather clear incisions. Were they produced by human beings? De Mortillet thought not. The imprints of a stone edge used as a saw are easily recognizable, and there were no traces of sawing on the bone. Because of their irregular edges, cutting instruments of stone generally leave small striations along the longitudinal axis of the V-shaped groove produced. But on the markings of the Billy fossil the striations were said to be transverse to this axis, i.e., running from the top of the cut, vertically down to the bottom of the groove. Furthermore, the marks on the jawbone were wider and deeper than might be expected from the action of a thin stone blade drawn across the bone.

De Mortillet thought the marks were not produced by a stone chopping instrument as proposed by Laussedat. The blow of a stone handaxe, according to de Mortillet, leaves an imprint with rounded sides. The marks on the Jawbone of Billy, however, were straight-sided, and could not, in the opinion of de Mortillet, have been the result of a stone hatchet blow. Furthermore, he noted



that the mark of the blow of a hatchet is distinguished by a surface clean and sharp on the side hit by the blade, and abrupt and rough on the side from which the splinter of bone separates. In the imprints on the jaw of Billy, this feature was, said de Mortillet, absent (1883, p. 50).

What then had been the cause? De Mortillet, sticking to his usual explanation, wrote in LePréhistonque (1883, pp. 50 -51): "They are simply geological Impressions. All geologists know that there exist in many terrains, especially Miocene, rocks that have profound Impressions on them. The cause is not easily recognized, but the fact that it has been observed is Incontestable. There is a great similarity between the marks on some of these rocks and those on the Jaw of Billy. I have collected at Tavel (Gard), and given to the museum of Saint-Gennaln, a quartzite rock, a very hard rock, bearing marks completely analogous to those on the specimen presented by Mr. Laussedat. On examining with care and at length this bone, one notices on one of the extremities a small Impression produced by crushing. There is no removal of material, simply compression. This Impression, which is of the same aspect as the other marks on the bone, is their contemporary and serves to explain them."

About marks on stones from Miocene formations, de Mortillet, as mentioned above, admitted that "the cause is not easily recognized." It is known that glaciers can groove bedrock, but this phenomenon is not applicable to grooved stones (or fossil bones) from preglacial Miocene formations. De Mortillet mentioned a grooved piece of quartzite. But quartzite is a very hard rock (7 on the Mohs scale of hardness, with talc at 1 and diamond at 10). It would thus require a harder mineral, which de Mortillet did not name, and extreme pressure, which de Mortillet did not explain, to mark quartzite with deep grooves. One must also consider the possibility that grooves in quartzite might be caused by chemical corrosion and recrystallization rather than cutting.

It is apparent that neither we nor de Mortillet know for certain what produced the grooves in the quartzite rock he found at Tavel. But it is probably not the same agency that would produce grooves on bone, a very different material, found in a freshwater deposit of calcified sand (de Mortillet 1883, p. 49). In essence, we find de Mortillet proposing that we should accept a completely unknown geological mechanism to explain the marks on the rhinoceros Jaw of Billy, in preference to the known mechanism of human action. Although de Mortillet may be right, he offers insufficient evidence to justify his view.

Another factor to consider is the character and placement of the marks on the rhinoceros Jaw of Billy. A highly regarded modern authority on cut bones is Lewis R. Binford, an anthropologist from the University of New Mexico at Albuquerque. In *Bones: Ancient Men and Modern Myths*, a comprehensive study of incised faunal remains, Binford pointed out that a key element in distinguishing human incisions from others is the exact placement of the marks. Extensive research has shown that in almost all cultures, ancient and modern, butchering marks tend to occur, though with some degree of variation, on specific bones and in specific locations on those bones, as dictated by the anatomy of the animal. For example, Binford (1981, p. 101) stated: "Marks on the mandible [lower Jaw] tend to be slightly oblique incised marks on the inside of the mandible generally opposite the M2 tooth [second molar]. The marks are believed to originate

from the underside of the mandible and to be related to the severing of the mylohyoid muscle during the removal of the tongue." The marks described by Laussedat appear to conform to this general description, but because no drawing or photo accompanied the available reports on the Billy Jawbone, this remains to be more exactly confirmed.

The marks on the Jawbone of Billy, which Laussedat described as a group of short parallel cuts, also appear to be consistent with the type of pattern that might be made by stone Implements. According to Binford (1981, p. 105): "Most of the cut marks made on bones with metal tools are almost hairline in size. ... the marks are generally long, resulting from cuts running across tissue for considerable distances. Cutting with stone tools requires a much less continuous action, more of a series of short parallel strokes. . . . Marks from stone tools tend to be short, occurring in groups of parallel marks, and to have a more open cross section. "

It seems difficult to categorically reject human action on the rhinoceros Jawbone of Billy, at least on the basis of the available published information. The action of carnivores can be safely ruled out. The geological explanation proposed by de Mortillet appears unlikely. The cut marks are on a bone that typically would be cut in butchering operations, and they appear to be in an appropriate location on the bone. In addition, the short length and parallel grouping of the marks resembles the pattern to be expected from the use of stone tools. So despite de Mortillet's objections, it does not seem impossible that a stone instrument pressed forcefully on a bone could make the kind of marks found on the Miocene rhinoceros fossil from Billy, France.

## 2.7 Colline de Sansan, France (Middle Miocene)

The report of the rhinoceros Jaw of Billy led to the opening, at the meeting of the French Academy of Sciences on April 20, 1868, of a sealed packet deposited at the Academy on May 16, 1864 by the researchers F. Garrigou and H. Filhol. These gentlemen wrote on that date: "We now have sufficient evidence to permit us to suppose that the contemporaneity of human beings and Miocene mammals is demonstrated" (Garrigou and Filhol 1868, p. 819). This evidence was a collection of bones, apparently intentionally broken, from Sansan (Gers), France. Especially noteworthy were broken bones of the small deer *Dicrocerus elegans*. The bone beds of Sansan were judged to be of Middle Miocene age (Mayencian). One may consider the devastating effect that the presence of human beings about

15 million years ago would have on current evolutionary doctrines.

Were the nineteenth-century scientists correct in their determination of the age of the site? Once more, the answer to this question is yes. Modern authorities (Romer 1966, p. 334) still place Sansan in the Middle Miocene, and *Dicrocerus elegans* is assigned to the Helvetian land mammal stage, which is considered Middle Miocene (Klein 1973, p. 566; Romer 1966, p. 334).

According to de Mortillet, Edouard Lartet, who also excavated fossils from Sansan and himself sent to Garrigou some of the bones on which Garrigou and Filhol founded their assertions, did not believe in human action on the bones. There were many broken bones at Sansan, and de Mortillet

(1883, pp. 64 -65), In his usual fashion, said that some were broken at the time of fossilization, perhaps by dessication, and others afterward by movement of the strata.

Garrigou, however, maintained his conviction that the bones of Sansan had been broken by humans, in the course of extracting marrow. He made his case in 1871 at the meeting in Bologna, Italy, of the International Congress of Prehistoric Anthropology and Archeology. Garrigou (1873) first presented to the Congress a series of recent bones with undisputed marks of butchering and breaking. For comparison, he then presented bones of the small deer (*Dicrocerus elegans*) collected from Sansan. Among them was a humerus (the long bone of the upper forelimb) with a set of breaks exactly resembling those on a cow humerus from the Neolithic age. On its inner surface, the deer bone bore a profound incision, filled up with material from the stratum in which it was found.

Garrigou also displayed a radius (one of the bones of the lower forelimb) presenting a longitudinal fracture terminating at a right angle to the end of the bone. The fracture had the same patina as the rest of the bone, indicating the break was made when the bone was fresh, and the broken part had a surface so clean and sharp that it was impossible to see it as a natural geological effect. Subterranean pressure and shifting, if it had occurred, would have almost certainly damaged the perfectly intact edges and joint surfaces of the fractured long bone. In making these observations, Garrigou showed a good grasp of taphonomic principles. He also pointed out that the longitudinal fracture on the specimen he showed was identical to those encountered on hundreds of similar bones at Sansan.

Here we may note that longitudinal fracturing is characteristic of breaking bone for the purpose of obtaining marrow. Binford (1981, p. 162) stated: "Marrow is primarily contained in the medullary cavity of the body or shaft of long bones. This shaft is shaped like a cylinder, so access to the medullary cavity and hence the marrow is facilitated by collapsing or fracturing the cylinder longitudinally. Transversal fractures in the center of long-bone shafts do not provide ready access to the marrow."

Garrigou also showed that many of the bone fragments had very fine and delicate striations such as found on broken bones of the Late Pleistocene. The marks could be indications of processing the bone for marrow breaking, as described by Binford: "The secret of controlled breakage of marrow bones is the removal of the periosteum [the sheath of connective tissue covering bone surfaces] in the area to be impacted. The Nunamiut invariably do this by scraping it back with the edge of a knife, a rough surface on a hammerstone, or almost any handy crude scraping tool. This means that longitudinal scratches and striations along the shafts of long bones are commonly produced when bones are prepared for cracking during marrow processing. Such marks are noted in Mousterian [Neanderthal] assemblages" (Binford 1981, p. 134).

Garrigou also displayed two metacarpals (foot bones), each with the smaller end removed by a direct blow. He pointed out that since flint tools had been found in the Miocene, one should not be astonished to find the effects of their usage. Food is the primary human need, so one should expect to observe signs of human attempts to secure it (Garrigou 1873, p. 137). In the next three

chapters, we shall consider in detail the evidence for flint tools in the Miocene and Pliocene, but for now we should keep in mind that reports of such discoveries were very common at this time, and were accepted by many reputable scientists.

Garrigou did, however, meet with strong opposition at the Congress, from, among others, Professor Japetus Steenstrup, secretary of the Danish Royal Society of Science and director of the Museum of Zoology in Copenhagen. Steenstrup argued that a broken bone should have a percussion mark (Garrigou 1873, p. 140). The fractured edges of a bone fragment should converge at this point, where a blow had been struck. According to Steenstrup, the bones displayed by Garrigou did not show percussion marks and converging fractures. Steenstrup therefore believed that the bones had been broken by the gnawing of carnivores.

Garrigou disagreed that fragments must show a percussion mark: its absence would not, in the case of any particular fragment, rule out direct impact as the cause of fracturing. In experiments, Garrigou had seen fresh bones broken into many flakes by a blow, and only one or two flakes would have the percussion marks. And if the instrument used happened to be sharply pointed, the bone would split immediately like a piece of wood, with no percussion imprint whatsoever (Garrigou 1873, p. 141).

The observations of both Steenstrup and Garrigou are in line with modern test data. In support of Steenstrup, we find that Binford stated (1981, p. 163): 'Impact scars from hitting the bone during marrow cracking are quite distinctive. First, they are almost always at a single impact point, which results in driving off short but rapidly expanding flakes inside the bone cylinder. At the point of impact the bone may be notched, in that a crescent-shaped notch is produced in the fracture edge of the bone.' But Binford's surveys showed that only about

14-17 percent of bone splinters in marrow cracking assemblages will have impact notches on them, indicating human action; this lines up with Garrigou's assertion that the vast majority of fragments will not have the impact marks. It would seem appropriate to analyze some Sansan bone splinter assemblages in terms of Binford's impact notch frequency criterion to test for human or animal action.

Garrigou also pointed out that Steenstrup's assertion that the bone breakage was caused by animal gnawing was incorrect, because the bones should then have displayed the marks of their canines and molars, and such was not the case. Animal gnawing results in extensive bone destruction, and the clean edges of the longitudinal fractures described by Garrigou contradicted that hypothesis.

Binford (1981, pp. 179-180) advised: "If one observes a pattern of bone destruction and knows that destruction is the normal consequence of animal behavior, one should view one's task as disproof of the proposition that animals were responsible for the observed patterns. . . . One might suspect that the reverse strategy might prove helpful when a pattern of bone breakage or modification by percussion is noted. Namely, knowing that breakage is a normal consequence of

human behavior, one should view one's task the disproof of the proposal that man was responsible." The bones of Sansan seem to fit in the category of breakage rather than destruction.

What sort of tests might be applied to disprove human action? Binford pointed out that animals typically destroy the articulator (or Joint) ends of long bones during gnawing, whereas human breakage normally does not result in articulator destruction. Binford (1981, p. 173) suggested that it should therefore be possible to examine ratios of articulator ends to shaft pieces in broken bone assemblages as a method of discriminating between animal and human action. In the case of animal action, one would expect a low ratio of articulator ends to be present. Of course, the possibility that animals might scavenge bones left by humans introduces a complicating factor.

So in the case of the broken bones of Sansan we once more encounter evidence for a human presence in very ancient times. This evidence certainly cannot be ruled out in the absence of further study. Garrigou's methodology and analysis appear to be quite rigorous, relying on sound taphonomic principles, extensive comparison with bones indisputably broken by human action, and evidence gathered from direct experiments in bone breakage patterns. We can only wonder why this report has remained buried. Whatever the reason, It would appear that the present data collection upon which ideas about human origins are based may be quite incomplete.

## 2.8 Pikermi, Greece (late Miocene)

At a place called Pikermi, near the plain of Marathon in Greece, there is a fossil-rich stratum of Late Miocene (Tortonian) age, explored and described by the prominent French scientist Albert Gaudry. During the meeting in 1872 at Brussels of the International Congress of Prehistoric Anthropology and Archeology, Baron von Ducker reported that broken bones from Pikermi proved the existence of humans in the Miocene (von Ducker 1873, pp. 104 -107). Modern authorities still place the Pikermi site in the Late Miocene (Nilsson 1983, p. 476; Jacobshagen 1986, pp. 213,221).

Von Ducker first examined numerous bones from the Pikermi site in the Museum of Athens. He found 34 jaw parts of Hipparion (an extinct three-toed horse) and antelope as well as 19 fragments of tibia and 22 other fragments of bones from large mammals such as rhinoceros. All showed traces of methodical fracturing for the purpose of extracting marrow. According to von Ducker (1873, p. 104), they all bore "more or less distinct traces of blows from hard objects." He also noted many hundreds of bone flakes broken in the same manner. It would thus appear that these fractured bones would satisfy the requirements of nineteenth-century authorities such as Steenstrup as well as modern authorities such as Binford with regard to impact notches as a sign of intentional breakage.

In addition, von Ducker observed many dozens of crania of Hipparion and antelope showing methodical removal of the upper jaw in order to extract the brain. The edges of the fractures were very sharp, which may generally be taken as a sign of human breakage, rather than breakage by gnawing carnivores or geological pressures. One might question whether the bones in the museum collection actually belonged to the Miocene stratum of Pikermi, but many of them had a

matrix of red clay clearly confirming the layer from which they were recovered. The museum personnel said, however, that no stone tools or traces of fire had been found with the bones.

Von Ducker then journeyed to the Pikermi site itself to continue his investigation. During the course of his first excavation, he found dozens of bone fragments of Hipparion and antelope and reported that about one quarter of them bore signs of intentional breakage. In this regard, one may keep in mind Binford's finding that in assemblages of bones broken in the course of human meat extraction about 14-17 percent have signs of impact notches. "I also found," stated von Ducker (1873, p. 105), "among the bones a stone of a size that could readily be held in the hand. It is pointed on one side and is perfectly adapted to making the kinds of marks observed on the bones.'

Von Ducker's second excavation was made in the presence of one of the founders of the International Congress of Prehistoric Anthropology and Archeology, Professor G. Capellini of Bologna, Italy. Capellini, who believed that broken bones were by themselves insufficient to demonstrate the presence of human beings at a site, did not attach as much significance to the Pikermi finds as did von Ducker. Nevertheless, he thought the bones had been fractured before the time of deposit.

Capellini reported that he had visited the museum and found the majority of bones were not broken by humans, as believed by von Ducker. Capellini pointed out that in fact there were many bones and skulls on display that remained whole and in good condition. Von Ducker replied that the fact that some bones were not broken did not change the fact that others were broken, and these in a way that suggested intentional work. He noted that Gaudry had naturally selected the best bones for his museum displays (von Ducker 1873, p. 106). Von Ducker stated that Capellini's very brief examination could hardly compare with his own lengthy and careful study, lasting for a period of several months, both in the museum and at the site.

De Mortillet stated that von Ducker's report was submitted to Gaudry, who found no evidence of human work. De Mortillet also examined the bones, and agreed with Gaudry and Capellini that the breakage was "accidental." It is, however, interesting to note that von Ducker, after communicating his observations to Gaudry, received the following statement from Gaudry: "I find every now and then breaks in bones that resemble those made by the hand of man. But it is difficult for me to admit this" (von Ducker 1873, p. 107). In Gaudry's remark surfaces one of the central questions confronting us in our examination of the treatment of paleoanthropological evidence. The evidence appears in general to be quite ambiguous. So on what basis can one draw conclusions? Gaudry hinted that his preconceptions were in subtle conflict with his perceptions. Humans in the Miocene? It was too difficult for him to admit. Preconception triumphed, however quietly, over perception.

In the final analysis, what are we to make of the fractured bones of Pikermi? Any clear answer to that question shall have to wait until such time as the final analysis is made. And it remains doubtful whether any totally "final" analysis ever can be made. Ambiguity is inherent in the enterprise. Surely, we cannot yet conclude, on the basis of the available reports, that humans

were not responsible for the breakage observed on Hipparion bones from the Miocene formations at Pikermi, Greece.

Another thing to keep in mind is that some modern researchers believe that in general evidence for human breaking of bone has been neglected or gone unrecognized. Robert J. Blumenshine and Marie M. Selvaggio, anthropologists at Rutgers University, conducted experiments in which they used pieces of sandstone to break African mammal (gazelle, impalla, wildebeest) longbones in order to extract marrow. According to Science News of July 2, 1988: "The resulting pits and grooves or 'percussion marks' on the bones, usually found near the notches created by the impact of stone, look much like carnivore tooth marks at first glance, the researchers report in the June 24 Nature." But the scanning electron microscope revealed "patches of distinctive parallel lines" different from those made by hyaena teeth. Blumenshine and Selvaggio maintained, stated Science News, that "researchers probably have underestimated or overlooked the breaking of bones by early humans to obtain marrow."

## 2.9 Pierced Shark Teeth from the Red Crag, England (Late Pliocene)

At a meeting of the Royal Anthropological Institute of Great Britain and Ireland, held on April 8, 1872, Edward Charlesworth, a Fellow of the Geological Society, showed many specimens of shark (Carcharodon) teeth, each with a hole bored through the center, as is done by South Seas islanders for the purpose of making weapons and necklaces. The teeth were recovered from the Red Crag formation, indicating an age of approximately 2.0-2.5 million years (Nilsson 1983, p. 106).

The record of the meeting, published in the Journal of the Anthropological Institute, informs us: "Mr. Charlesworth pointed out the conditions under which boring molluscs, as Pholas and Saxicava, perforate the texture of stones or other solid substances, and glanced at the perforating action of burrowing sponges (Chona) and destructive annelides (Teredo). Reasons were given at length why these could not have produced such perforations as those now exhibited. The most searching and cautious examination was also bestowed to demonstrate that the perforating body, whatever it was, was coeval with the crag period; i.e., that specimens existed in which the true crag matrix filled up the hole from end to end, thus showing that it had been immersed in the crag sea after the period of its perforation" (Charlesworth 1873, p. 91).

Charlesworth (1873, pp. 91-92) did not personally suggest human agency, but did show a letter from Professor Owen, who had carefully examined the specimens and stated: "the ascription of the perforations to human mechanical agency seemed the most probable explanation of the facts."

During the ensuing discussion, Mr. Whitaker suggested tooth decay as the cause, noting one specimen with holes in various stages, from slight indentation to perforation (Charlesworth 1873, p. 92). Then Dr. Spencer Cobbold, an expert on parasites, suggested parasites as the agent of perforation but admitted, according to the summary report: "it might be said with truth, perhaps, that no entozoon [internal animal parasite] had hitherto been known to take up its abode in the bones or teeth of fishes" (Charlesworth 1873, p. 92).

At that point Dr. Collyer gave his opinion in favor of human action. The record of the meeting summarized his remarks as follows: "He had carefully examined by aid of a powerful magnifying glass the perforated shark's teeth. . . . The perforations, to his mind, were the work of man. His reasons were—First, the bevelled conditions of the edges of the perforations. Secondly, the irregularity of the borings. Thirdly, the central position of the holes in the teeth. Fourthly, the choice of the thin portions of the tooth where it would be most easily perforated. Fifthly, the marks of artificial means employed in making the borings. Sixthly, they are at the very place in the tooth that would be chosen in making an instrument of defence or offence, or for ornament in the form of a necklace. Seventhly, the fact that rude races—as the Sandwich Islanders or New Zealanders—have from time immemorial used sharks' teeth and bored them identically with those exhibited. His reasons for supposing the perforations not to have been produced by molluscs, or boring-worms, or any parasitic animal, were—First, those creatures invariably had a purpose in making a hole for lodgement: it was therefore evident they would not choose the thin portion of the tooth, which would be totally unadapted for the object sought. Secondly, there was not a case on record of any parasite or mollusc or worm boring a fish's tooth. Thirdly, those animals had no idea that the exact centre of the tooth would be preferable to the lateral portion. Fourthly, had the holes been the result of animal borings, they would have presented a uniform appearance. As to the tooth being perforated by decay, that seemed to him the most extraordinary proposition. The appearance of a decayed tooth had no analogy whatever to the borings presented. Moreover, sharks were not subject to decayed teeth" (Charlesworth 1873, p. 93).

Mr. T. McKenny Hughes then argued against human boring, pointing out that in some cases the holes on the front and back sides of the tooth are not perfectly lined up with each other. It is not, however, obvious how this would preclude human action. Just to consider one possibility, one could easily imagine a worker partially boring the tooth on one side, turning it over, and completing the perforation by boring in from a slightly different angle starting on the other side.

Hughes then offered another curious objection. He observed that the same types of perforation are found on fossils not only in the Crag, a formation on the Plio-Pleistocene boundary, but also on shells in other deposits more ancient, such as the green sandstone strata of Secondary age. He asserted that it was clearly impossible for humans to have existed at this remote time; therefore the perforations in fossils in the green sandstone were clearly natural in origin. And, by analogy, so were the perforations in the shark teeth from the Red Crag. Here is yet another very typical example of preconceptions determining what kind of evidence for human antiquity can be accepted. Another possible way to look at the perforated shells found in the older green sandstone strata is that they also could be the result of the action of human beings. As previously mentioned, the most recent Secondary period is the Cretaceous, which ended about 65 million years ago.

In any case, Hughes suggested that the perforations in the Red Crag shark teeth were caused by a combination of wear, decay, and parasites (Charlesworth 1873, p. 93). Mr. G. Busk presented the same conclusion at the 1872 meeting of the International Congress of Prehistoric Anthropology and Archeology in Brussels. In *Le Préhistorique*, de Mortillet (1883, p. 68) sarcastically remarked



that It was really curious how some people searched so obstinately for proof of the existence of Tertiary humans In marine deposits.

But In looking at the arguments presented In this case, both those in favor of human work and those opposed, it would seem that obstinacy Is more clearly evident in those who refused to accept the possibility of human action. What are the alternatives that were presented? Some suggested tooth decay, although sharks are not known to have cavities; others suggested parasites, although one of Britain' s leading experts admitted there was no known Instance of a parasite Inhabiting the teeth of fish or sharks. Others suggested wear had a role to play, though one would be hard pressed to find examples In nature of wear causing clean round holes through the centers of teeth.

## 2.10 Carved Bone from the Dardanelles, Turkey (Miocene)

In the Journal of the Royal Anthro<sup>p</sup>ological Institute of Great Britain and Ireland, Frank Calvert(1874, p. 127) reported; "I have had the good fortune to discover, In the vicinity of the Dardanelles, conclusive proofs of the existence of man during the Miocene period of the tertiary age. From the face of a cliff composed of strata of that period, at a geological depth of eight hundred feet, I have extracted a fragment of the Joint of a bone of either a dinotherium [Deinotheriim] or a mastodon, on the convex side of which Is deeply Incised the unmistakable figure of a horned quadruped, with arched neck, lozenge-shaped chest, long body, straight fore-legs, and broad feet. There are also traces of seven or eight other figures which, together with the hind quarters of the first, are nearly obliterated. The whole design encircles the exterior portion of the fragment, which measures nine Inches In diameter and five in thickness. I have found in different parts of the same cliff, not far from the site of the engraved bone, a flint flake and some bones of animals, fractured longitudinally, obviously by the hand of man for the purpose of extracting the mai<sup>T</sup>OW, according to the practice of all primitive races."

Calvert (1874, p. 127) added: 'There can be no doubt as to the geological character of the formation from which I disinterred these Interesting relics. The well known writer on the geology of AsiaMinor, M. de Tchlhatcheff, who visited this region, determined It to be of the miocene period; and the fact is further confirmed by the fossil bones, teeth, and shells of the epoch found there. I sent drawings of some of these fossils to Sir John Lubbock, who obligingly Informs me that having submitted them to Messrs. G. Busk and Jeffreys, those eminent authorities have identified amongst them the remains of dinotherium, and the shell, of a species of melania, both of which strictly appertain to the miocene epoch." The Deinotherium Is said by modern authorities to have existed from the Late Pliocene to the Early Miocene In Europe (Romer 1966, p. 386). It Is thus quite possible that Calvert's dating of the Dardanelles site as Miocene was correct. The Miocene is now said to extend from 5 to 25 million years before the present. According to the current dominant view, only exceedingly apelike homInids are supposed to have existed during that period. Even a Late Pliocene date of

2.5- 3.0 million years for the Dardanelles site would predate the first toolmaking hominid (Homo habilis).

Calvert appears to have been sufficiently qualified to estimate the date of the Dardanelles site. David A. Traill (1986a, pp. 53-54), a professor of classics at the University of California at Davis, gives this information about him; "Calvert was the most distinguished of a family of British expatriates that was prominent in the Dardanelles . . . he had a good knowledge of geology and paleontology." Calvert conducted several important excavations in the Dardanelles region.

Calvert also played a very important role in finding the site of the famous city of Troy. Scholars usually give the credit for this to Heinrich Schliemann. But Traill (1986a, pp. 52-53) said of Calvert; "After excavating the 'Tumulus of Priam' on the Balli Dag (1863) and reading Charles Maclaren's *A Dissertation on the Topography of the Plain of Troy* (Edinburgh 1822), he decided that Hissarlick must be the site of Troy. He purchased part of the mound and started to excavate in 1865, but lack of funds and the pressure of other commitments caused him to abandon the task. . . . After Schliemann's unsuccessful diggings at Bunarbashi in 1868, Calvert persuaded him . . . that Hissarlick, not Bunarbashi, was the true site of Troy. Schliemann later downplayed both the significance of Calvert's excavations and his role in awakening his interest in Hissarlick and successfully appropriated all the glory for himself. Calvert, however, was much the better scholar."

During his excavations, Schliemann came upon a group of weapons, utensils, and ornaments that he called "Priam's Treasure." Calvert reviewed these findings and Schliemann's excavations in general. Traill (1986b, p. 120) stated: "He pointed out, with remarkable acuity, that the excavated material should be dated before 1800 b.c. and after 700 b.c. but that nothing was attributable to the period between these dates. Since the missing period included the time of the Trojan War, these findings enraged Schliemann. His response was to ridicule Calvert's views and misrepresent his role in the excavation of Hissarlick. . . . Calvert was, as far as I have been able to determine from extensive reading of his correspondence, scrupulously truthful." The so-called treasure of Priam, thought Calvert, was genuine, but not of the classical Trojan era, and this view conforms with the opinion of modern scholars.

Altogether, Calvert seems to have been a quite competent field investigator, with a reputation for truthful and careful reporting. It thus seems that in the case of his Miocene discoveries, he would not have missed any obvious sign that the carved bone, broken bones, and stone implements he discovered had been recently cemented into the deposits. It should be noted that the carved bone from the Dardanelles was no less securely positioned stratigraphically than a great many thoroughly accepted discoveries. Most of the Java *Homo erectus* finds and most of the East African *Australopithecus*, *Homo habilis*, and *Homo erectus* finds occurred on the surface and are presumed to have washed out from underlying formations varying from Middle Pleistocene to Late Pliocene in age.

In *Le Préhistorique*, de Mortillet did not dispute the age of the Dardanelles formation. Instead he commented that the simultaneous presence of a carved bone, intentionally broken bones, and a flint flake tool was almost too perfect, so perfect as to raise doubts about the finds (de Mortillet 1883, p. 69). This is quite remarkable. In the case of the incised bones of St. Prest, de Mortillet complained that no stone tools or other signs of a human presence were to be found at the site.

But here, with the requisite items discovered along with the carved bone, de Mortillet said the ensemble was "too perfect," hinting at cheating.

De Mortillet then alluded to the well-publicized disputes between Calvert and Schliemann, which he claimed had discredited both men. In addition to Calvert's disagreements with Schliemann about the dates of his archeological discoveries at Hissarlick and their relation to the classical Troy of Homer, there were also some financial bickerings. Calvert and Schliemann had an agreement that they would share the proceeds from the sale of any discoveries at Hissarlick. A particularly fine statue was the source of some controversy, with Calvert charging that Schliemann paid him far less than it was actually worth (Traill 1986a, pp. 53 -54). But It seems that Calvert emerges from all this as an honorable and truthful person, who had a better grasp of the archeology of the Hissarlick site than Schliemann. This tends to increase, rather than decrease, the credibility of Calvert's reporting about his Miocene discoveries.

Finally, de Mortillet (1883, p. 69) stated that because no further reports of a serious nature or new discoveries of human artifacts had emerged from the Dardanelles site, the original Miocene finds reported by Calvert should be considered unconfirmed. But perhaps if new finds had been made, de Mortillet would have reacted as he had to the first ones—by calling them "too perfect," questioning the character of the discoverer, and demanding more discoveries.

#### 2.11 Balaenotus of Monte Aperto, Italy (Pliocene)

During the latter part of the nineteenth century, fossil whale bones bearing curious marks turned up in Italy. On November 25, 1875, G. Capellini, professor of geology at the University of Bologna, reported to the Institute of Bologna: "Recently as I was cleaning a bone that I myself extracted from the blue Pliocene clay, synchronous with that of the Grey Crag of Anvers, of Astian age, I saw to my great surprise on the dorsal surface a notch and an incision. The former, especially, was so clean cut and deep as to indicate it was made by a very sharp instrument. I am able to say that the bone found is so completely petrified as to preserve all the most delicate details of its microscopic structure; furthermore, it has acquired such hardness that it is not possible to scratch it with a steel point. This circumstance enables us to completely reject suggestions that tend to attribute the marks to modern action" (de Mortillet 1883, p. 56). During further cleaning Capellini discovered three other lighter marks on the bone. He announced this discovery and others that followed at the Academy of Lynxes at Rome and the International Congress of Prehistoric Anthropology and Archeology meetings at Budapest in 1876 and Paris in 1878. Capellini, a founding member of the Congress, was a prominent member of the European scientific community.

The whale bones studied by Capellini were from the extinct small baleen whale *Balaenoketus*, which is characteristic of the Late Pliocene of Europe (Romer 1966, p. 393). This confirms Capellini's assignment of his discoveries to the Pliocene.

In 1876, Capellini showed his principal specimens at the Congress at Budapest, where he told the members (1877, pp. 46-47): "For fifteen years I have been researching and studying cetacean

fossils. After my work on the Balaenopteridae in the province of Bologna, I decided to undertake researches into the baleen whales of Tuscany. . . . By the kindness of Professor D' Ancona, I was able to examine at my leisure the remains of fossil baleen whales at the Museum of Natural History of Florence. I then became convinced of the great importance of extending my researches beyond the specimens in the glass cases and dusty vaults of the museums. I was certain that direct investigations in the strata that had already yielded much precious material would be extremely fruitful for further progress in the study of fossil whales."

We shall now consider Capellini's extensive report in detail, making liberal use of direct quotations, translated from the original French. This procedure is being followed for the two reasons previously mentioned: (1) a report, in this case a very important one, is itself, for all practical purposes, the evidence; and (2) readers could not otherwise obtain the original report except by referring to a rare nineteenth-century volume of conference proceedings.

"In October of 1875," continued Capellini (1877, p. 47), "I journeyed to Siena to continue my stratigraphic studies of that region's Tertiary terrains and at the same time examined the remains of fossil cetaceans in the museum of the Academie des Fisiocritici. On the advice of Dr. Brandini, I also began excavations at Pogglarone, in the neighborhood of Monte Aperto. I was greatly fortunate to make a double discovery: first, I recovered numerous remains of a skeleton of Balaenotus, a fossil cetacean first recognized by van Beneden, and heretofore found only in the Grey Crag of Anvers: and secondly on these very same bones I noticed the first traces of the hand of man, demonstrating the coexistence of human beings with the Pliocene whales of Tuscany. "

Capellini went on to display some samples of his discoveries. "I have the honor," he said, "of presenting remarkable specimens that bear marks which, by their form and placement on the fossil bones, demonstrate in an irrefutable manner the action of a being manipulating an instrument. This is the opinion of all the most experienced naturalists and anatomists, not only in Italy, but from all over Europe, who have examined these specimens, judging them without preconceived ideas" (Capellini 1877, p. 47). It may be noted that by considering the "form and placement" of the cuts, Capellini was adhering to modern criteria for distinguishing human workmanship from animal gnawing on bone. His reference to scientists tending to have "preconceived ideas" is particularly relevant to our discussion.

Regarding the geological age of the strata in which the Balaenotus fossils had been discovered, Capellini observed in his report: 'The geological position of the strata in which the Balaenotus was found in the neighborhood of Monte Aperto and the shells that were found in the same bed do not permit us to doubt their Pliocene age and their resemblance to the Grey Crag of Anvers. The alternation of beds entirely of sand with others of clay and sand, give evidence that the animal was beached in the shallows along the shore of an island of the Pliocene archipelago that occupied what is now central Italy during the last part of the Tertiary epoch."

Capellini (1877, p. 48) then described the placement of the cut marks on the fossil bones: "The marks on the skeleton of the Balaenotus are found on the lower extremities, the exteriors of the ribs, and on the apophyses [spines] of the vertebrae." The presence of cuts on the vertebral

spines, or apophyses, conforms with the observations of Binford (1981, p. III), who stated that In flesh removal, cuts are made to free flesh from the dorsal spines of the thoracic and lumbar vertebrae, producing "cut marks . . . commonly oriented transversely or slightly obliquely to the dorsal spines of the thoracic vertebrae. " As far as the ribs are concerned, Binford (1981, p. 113) stated that In the most common butchering operation "transverse marks, derived from the removal of the tenderloin, occur along the dorsal surface of the rib just to the side of the proximal end of the rib." The marks observed by Capellini, all on the dorsal (exterior) surface of the rib, correspond to this description.

Applying principles of taphonomic analysis, Capellini (1877, p. 49) then stated: "On the dorsal apophysis of an almost complete lumbar vertebra, I have moreover marked the presence of intersecting cutmarks and next to them one sees tiny oysters, evidence that indicates the deposition took place in very shallow water not far from the shore. One should not forget that the entire region formerly occupied by the sea in the environs of Siena has been raised and lowered many times, which accounts for the alternation of marine, brackish, and freshwater deposits one is able to observe and study at Siena." These alternations are indications of a littoral, or shoreline, area, which is important. Some critics believed the marks had been made by the teeth of sharks, and according to their analysis this would necessitate deep water.

For example, in his book *Le Prehistorique, de Mortillet* (1883, p. 59) stated that some Italian naturalists (Strobel and de Stefani) were of the opinion that the beds yielding bones of *Balaenotus* were not littoral but deep ocean. This seems to be at variance with the firsthand observations of Capellini, who was himself an experienced geologist. In his review, de Mortillet does not mention the evidence that Capellini cited in support of his conclusion that the location where the *Balaenotus* bones were found represented the shallows along the beach of the Pliocene sea.

"Having surveyed the excavations of the remains of the skeletons of *Balaenotus* in the environs of Siena," Capellini (1877, pp. 49-50) went on to say, "I was able to easily account for the existence of the marks on only one side, and always the same side. In effect, it is evident that for the specimen in question the marks were made by a human being that came upon the animal beached in shallow waters, and by means of a flint knife or with the aid of other instruments attempted to detach pieces of flesh. " Capellini (1877, p. 50) added: "From the position of the remains of the *Balaenotus* of Poggiarone, I am convinced that the animal ran aground in the sand and rested on its left side and that the right side was thus exposed to the direct attack of humans, as is demonstrated by the places in which marks are found on the bones. " The fact that only the bones on one side of the whale were marked would tend to rule out any purely geological explanation as well as the action of sharks in deep water.

Capellini (1877, p. 50) noted: "That which happens at present to the *Balaenopterae* and cachalots [sperm whales] that from time to time become beached on our shores also happened to the *Balaenotus* of Poggiarone and to other small whales on the shores of the Islands of the Pliocene sea." Capellini (1877, p. 50) then made an important observation: "After an attentive examination of skeletons found in the majority of Europe's museums of natural history, it is very

easy to convince oneself that all of these, which were prepared by humans, present the same kinds of markings as those on the bones you have seen and others which I will show you." Comparison with examples of undoubted human work is still one of the main methods scientists use in determining whether incisions on bones are of human origin.

Capellini (1877, p. 51) then reported that he had found examples of the kind of tool that might have made the cuts on the bones: "In the vicinity of the remains of the *Balaenotus* of Poggiarone, I collected some flint blades, lost in the actual beach deposits. " He added: "with those same flint implements I was able to reproduce on fresh cetacean bones the exact same marks found on the fossil whale bones" (Capellini 1877, p. 51).

"Before leaving the environs of Siena," Capellini (1877, p. 51) went on to explain, "I should point out that the remains of a human being found in 1856 by the Abbe Deo Gratias in the marine Pliocene clays of Savona in Liguria can be referred to approximately the same geological horizon as Poggiarone and other locales in Tuscany where I have found numerous cetacean remains." The details of the discovery of human skeletal remains in the Pliocene at Savona will be discussed at length in Chapter 6, which also contains many other such reports. For now, it will be sufficient to note that the discoveries of incised bones in the middle and late nineteenth century were accompanied by a great many simultaneous discoveries of flint implements and actual human skeletal remains in Pliocene and Miocene strata. These discoveries are practically never mentioned in modern textbooks. It bears repeating that the existence of human beings of the modern type in the Pliocene period would completely demolish the presently accepted evolutionary picture of human origins.

Capellini then discussed another find of human skeletal remains that he believed to be contemporary with the incised whale bones he had discovered in Pliocene strata. "In my first notice on Pliocene man in Tuscany (Nov. 1875) I mentioned the human cranium discovered by Professor Cocchi in the upper valley of the Arno, in Tuscany, and for the moment I accepted the conclusions given by my associate concerning the age of the strata in which the cranium was found." Cocchi had given them a Pleistocene date.

"Dr. F. Major, however," said Capellini (1877, pp. 51-52) to his colleagues at the Congress of Budapest, "has for many years been particularly interested in studying the fossil vertebrates of the upper valley of the Arno, and after new researches into the geological position of the human skull found at Olmo has reached an opinion contrary to that of Professor Cocchi. According to Dr. Major, the fossils of the strata in which the cranium of Olmo was found and those collected with the cranium itself by Professor Cocchi prove the Pliocene age of the stratum and that it is contemporary with the marine deposits containing incised bones of small whales." Modern authorities, however, assign a Pleistocene date to the Olmo skull (Appendix 1.2.1).

"Some months after the discovery of the *Balaenotus* of Poggiarone," continued Capellini (1877, p. 52), "I was, by means of similar discoveries, able to conclude that Pliocene man was present on other islands in the Tuscan archipelago. In examining the numerous remains of fossil cetaceans which Sir R. Lawley recently contributed to the museum of Florence, I discovered a fragment of a

humerus and three fragments of cubitus with marks Just as well-defined and instructive as those in question. Among the remains of Balaenotus from La Collinella, near Castelnuovo della Misericordia in the valley of the Fine, there have been recovered a good number with incrustations of gypsum. It was in the course of removing these incrustations, aided by the preparator E. Berclgli, that I noticed the markings. Shortly thereafter, the specimens were examined and the marks confirmed by M. d'Ancona, professor of paleontology, M. Gglioli, professor of zoology and comparative anatomy, Dr. Cavanna, Dr. Ch. Major, and others. "

Many of Italy's leading scientists concurred with Capellini's Judgement that the markings were caused by sharp instruments manipulated by human beings. Capellini (1877, p. 53) said: "The unanimous opinion of the naturalists of Florence, confirmed by that of the anatomists and naturalists of Bologna, all of whom examined the specimens with great care, was also supported by the academicians of the Rome Society of Lynxes, the names of whom may be found at the end of my published memoir."

Returning to consideration of the actual specimens, Capellini (1877, p. 53) said: "The Museum of Florence has allowed me to present these precious specimens for the inspection of the members of the Congress. I am very pleased to present them to the assembly because all of you interested in this question can verify that drawings alone do not allow one to appreciate all the fine details that permit us to exclude explanations other than that of a human being or other animal. who operated with the aid of instruments, and who by means of cuts in several directions, mostly deep and confined to a very limited area, was often able to facilitate the breaking of the bone. "

"On one of the fragments of cubitus," said Capellini (1877, p. 53), "I left intact a portion of the gypsum incrustation that covered a deep incision, a section of which is visible. If one removed the gypsum one would see that the entire mark had been made on the bone while it was fresh, and then conserved by fossilization and incrustation." This was good proof that the cut marks were not made in recent times.

Capellini (1877, pp. 53-54) also found similar cut marks on the apophyses of vertebrae he saw in the whale bone collection of Lawley. 'The fragment of the dorsal apophysis of a lumbar vertebra, in the space of a few centimeters," stated Capellini (1877, p. 54), "presents on the right side nine different incisions oriented in different directions. In examining the original with the aid of a lens, one can assure oneself that these marks, and the other marks that you will see, were made when the bone was fresh. One may also note that one side of the cut is smooth while the other is rippled, as occurs when one, with a knife or other instrument, marks a bone, either by a direct blow or by manipulation of the instrument in the manner of ordinary cutting [Figure 2.1]. It is to be remarked that the side of the bone opposite that bearing the marks is intact, and whatever incisions have been inflicted on the bone are so profound as to have been able to break it off. Two fragments of the apophyses of vertebrae broken at the place where they were cut or grooved are

represented ... in my memoir." The marks on the spine of the lumbar vertebra are in a location that according to Binford typically displays cut marks from butchering operations.

Figure 2.1. Magnified cross section of a cut on a fossil whale bone from a Pliocene formation at Monte Aperto, Italy (de Quatrefages 1887, p. 97).

Capellini then returned to geological considerations, describing the location at which several of his specimens were found. "The pieces . . . come from San Murino, near Pieve Santa Luce on the coast of the ancient Pliocene island of Monte Vaso, on La Collinella, in the valley of the Fine. Some meters from where M. Paco, a fossil hunter, found bone fragments of small whales, the ancient limestone rocks, which formed the shore of the Pliocene sea, are regularly pierced by lithophages. Because the depth at which these creatures establish their residences and leave their traces is well known, it is, in the valley of the Fine near Santa Luce, quite easy to establish the ancient level of the sea frequented by the small whales that human beings came upon in the Pliocene period, Just as in our own day we come upon small whales beached on the shores of the Mediterranean." Here is more evidence that the whale bones were most probably deposited in shallows by the shore. It is surprising that de Mortillet neglected to mention this in his review, where he gave the impression that scientific opinion is decidedly in favor of a deep water interpretation.

Returning to the question of the age of the strata in which the fossil whale bones were found, Capellini, himself a professor of geology, then stated (1877, pp. 55-56): "Among those who recognize without difficulty the work of humans in the markings on the whale bones, are some who are not persuaded that they are ancient, and who have demanded to know if there is perhaps not some doubt about the Judgement that the beds bearing the bones of Balaenutn) are really Pliocene in age. This question has been discussed by me in my memoir presented to the Rome Society of Lynxes in the presence of eminent geologists and paleontologists from Central Italy, such as Messrs. Sella, Meneghini, Ponzi, and others, who confirmed all that I had said. Their exact knowledge of the locality sufficed to allow them to appreciate the geological drawings by which I sought to decipher and record the stratigraphic series of the ancient fjord (presently the valley of the Fine) where the cetaceans perished in the Pliocene. After the publication of my memoir, complete with geological notices, I believe it useless to here repeat all the facts about the age of the strata of the small whales and the circumstances favoring the opinion that the whales were captured by human beings.'

After Capellini's presentation, the members of the Congress engaged in discussion. Sir John Evans accepted the geological age of the fossils, but said he thought the bones had some marks that appeared to have been made by the teeth of fish. This suggested to him that the bones had lain on



the bottom of the sea, where the other more prominent marks were perhaps made by the teeth of sharks. He believed that proof for the strata being on the shoreline was lacking. Thus, questioned Evans, if humans did exist in the Pliocene, how could it be that they were getting food from the deep sea? Furthermore the marks were so sharp that if it were an instrument that made them, it would seem to have been one of metal rather than stone. He also maintained that marks made accidentally by humans in detaching flesh would be of a different nature (Capellini 1877, pp. 56-57).

These appear to be fairly weak objections. Capellini gave adequate geological reasons to suggest at least the strong probability that the strata in which the fossils were found were littoral. Capellini had also examined, in museums, many skeletons of whales from which the flesh had been detached by humans, and had found the markings practically identical to those on the fossil bones of the Tuscan *Balaenotus*. Capellini (1877, p. 51) had in at least one case found flint implements near fossil whale bones and demonstrated that the flint blades could make marks identical to those found on the bones. Evans simply seems to have had some strong bias against the presence of humans in the Pliocene.

Next to speak was Paul Broca, a surgeon and secretary general of the Anthropological Society, headquartered in Paris. Broca was famous as an expert on the physiology of bones, particularly the skull. He lined up on the side of Capellini. Interestingly enough, Broca was a Darwinist, but the evidence he supported at the Congress of Budapest in 1876 would, if accepted now, completely destroy the modern Darwinian picture of human evolution.

"The discovery of Quaternary [Pleistocene] man was the greatest event in modern anthropology," said Broca. "It opened a great field of investigation, and none here can fail to recognize its importance, because, it was this event, one could say, that was most responsible for the grand movement of ideas that resulted in the founding of our Congress. The discovery of Tertiary man could be an even greater event, because the period it could add to the life of humanity is incomparably greater than that we know at present" (Capellini 1877, p. 57). The Tertiary includes the Pliocene, Miocene, Oligocene, Eocene, and Paleocene periods.

"This is not the first time this question has arisen in our discussions," continued Broca. "Already in 1874, at the Congress at Brussels, Abbe Bourgeois showed a series of flints from Tertiary strata and in which he believed he could see proof of human work, but few shared his opinion. For my part, I examined many times the flints of Abbe Bourgeois, and remained among those not accepting his demonstrations. The other facts relative to Tertiary man that have been put forward, from Europe and America, have not been conclusive enough for me. To this day I remain doubtful about the stratigraphic location and about the work attributed to human hands" (Capellini 1877, p. 57). In the next few chapters of this book, one will have the chance to draw one's own conclusions about the many discoveries of flint implements and human skeletal remains referred to here by Broca.

"But today," confessed Broca, "for the first time, I sense my doubts disappearing. I would declare myself entirely convinced, if I were relying totally on my own judgement. But I should also take into account the judgement of my colleagues. I should fear that I might be mistaken when I find

myself opposed by such competent men as Franks and Evans. With these reservations, I shall explain the evidence that leads me to admit the interpretation of Capellini" (Capellini 1877, pp. 57-58).

Broca then proceeded to present arguments against the hypothesis that the marks on the fossil bones of *Balaenotus* had been produced by the teeth of sharks. "In the first place," he said, "it is evident that the marks shown to us have been produced by cutting. All the world agrees on this point. We are only discussing the question of whether these cuts were made by the sharp pointed teeth of sharks or by the human hand armed with sharp flint. There is another point which seems to me incontestable. That is that all the incisions, in their diverse forms, those perpendicular as well as oblique, can be easily reproduced, with all their characteristics, with a flint implement on fresh whale bones. The hypothesis of Capellini explains very well the observed facts, while the other hypothesis encounters very strong objections. Capellini has remarked with reason that every bite should produce two imprints corresponding to the two Jaws that seize the bone at two opposite points. But without exception all the incisions are on the convex surface of the ribs, with the concave surface totally exempt from all markings. I do not believe that one can respond to this argument" (Capellini 1877, p. 58).

Here Broca seems to be thinking that the shark would completely devour the whale carcass, thus breaking apart the rib cage. Given the feeding frenzies of sharks, especially the great white shark, present in the Pliocene as *Carcharodon megalodon*, one might expect this to happen. Otherwise, it is difficult to see how the shark could place bite marks on both sides of the rib.

Some years later, de Mortillet (1883, p. 62) suggested, in *Le Préhistorique*, that the particular nature of a shark's jaw and method of biting would result in tooth marks being placed on only one side of a bone subjected to its attack. As usual, however, de Mortillet only painted speculative scenarios and did not present any hard experimental evidence.

Broca continued: "Among the incisions, the majority penetrate obliquely into the bone. One of the sides of the V-shaped incision slices into the bone at a small angle, departing only slightly from the horizontal plane of the surface of the bone; while the opposite side, shorter than the first, is abrupt, almost vertical. The incision shows breakage. That is to say, the cutting action results in the separation of a small shaving of bone, broken at its base [Figure 2.1]. The cutting action of a sharp edge produces marks of this type. I don't believe that the teeth of any animal could produce the same effect" (Capellini 1877, p. 58). The same thing was admitted by de Mortillet himself, who raised the point in his discussion of the bones of St. Prest (Section 2.1).

"Finally," said Broca, "—and I insist on this point, which Capellini touched upon only lightly—the direction of certain of the marks is incompatible with the idea of a bite. The Jaws do not execute such a movement. They open and they close.

The sort of curve described by a tooth rests always on the same plane. The incision produced by a pointed tooth on a hard surface, convex and immobile, is of determinate form. It is that of a plain curve, from one point to another by the shortest path, like a meridian on the surface of a sphere.

The majority of incisions before our eyes do not present such a character. Here is one among others in which the direction changes many times [ Figure 2.2]. ... the whole incision is made up, first, of a path perpendicular to the axis of the rib, then another longitudinal path, and finally an oblique one. It is a turning movement that a jaw could not make. The human hand, on the contrary, is capable, because of its multiple articulations, of perfect mobility, of guiding and inclining in every direction over the surface the instruments with which it is armed" (Capellini 1877, pp. 58-59).

Figure 2.2. A Pliocene whale scapula from Monte Aperto, Italy, with cut marks similar to those described by Broca (de Quatrefages 1887, p. 97).

Even though there may be some justification for pursuing the shark hypothesis with regard to the markings on the Pliocene whale bones of Italy, there is no reason to immediately abandon the hypothesis of human action, for which there is a great deal of evidence.

It is interesting that Broca, one of the foremost authorities on bone physiology of his time, favored Capellini's view that the marks on the fossil whale bones were the product of intentional human work. Perhaps not all of Broca's observations about the action of teeth on bone are correct. But this does not detract from Capellini's conclusions, which were founded on years of painstaking research, and not on Broca's extemporaneous statements.

After Broca's remarks, Capellini (1877, p. 60) himself offered some concluding words: "I have of course taken into consideration bones gnawed by different animals. At the same time, I have not neglected to examine all the kinds of fish teeth found in the same strata as the small whales, of which Mr. Lawley possesses a truly extraordinary collection. If one comes to tell me that with such teeth (using them as tools) he has been able to make such marks as you see on the fossil bones, I am ready to admit this, but if he pretends that the fish itself made the marks, that is another thing. In that case I would invite my illustrious contradictor to bring to my consideration the species of fish to which he would attribute marks

Identical to those we know as the work of man." Capellini (1877, p. 61) pointed out that such objections had not been raised by the naturalists who were knowledgeable about fish, but rather by archeologists.

One naturalist suggested the marks had been made by a swordfish, and to demonstrate this had taken a swordfish beak in hand, delivering thrusts that left some impressive marks on pieces of

fresh whalebone. But even de Mortillet (1883, p. 61), on seeing them and comparing them with the Incisions on the Tuscany fossils, rejected this view.

De Quatrefages was among the scientists accepting the Monte Aperto Balaenotus bones as being cut by sharp flint Instruments held by a human hand. He wrote: "However one may try, using various methods and Implements of other materials, one will fail to duplicate the marks. Only a sharp flint Instrument, moved at an angle and with a lot of pressure, could do it" (de Quatrefages 1884, pp. 93-94). De Quatrefages believed a band of Pliocene hunters found the whale beached and set upon it with stone knives of the type used by the present-day Australian aborigines.

The whole Issue was nicely summarized in English by S. Laing, who wrote in 1893 (pp. 115-116): "An Italian geologist, M. Capellini, has found in the Pliocene strata of Monte Aperto, near Siena, bones of the Balaenotus, a well-known species of a sort of Pliocene whale, which are scored by Incisions obviously made by a sharp-cutting instrument, such as a flint knife guided by design, and by a human hand. At first it was contended that these incisions might have been made by the teeth of fishes, but as specimens multiplied, and were carefully examined, it became evident that no such explanation was possible. The cuts are in regular curves, and sometimes almost semi-circular, such as a sweep of the hand could alone have caused, and they invariably show a clean cut surface on the outer or convex side, to which the pressure of a sharp edge was applied, with a rough or abraded surface on the inner side of the cut. Microscopic examination of the cuts confirms this conclusion, and leaves no doubt that they must have been made by such an Instrument as a flint knife, held obliquely and pressed against the bone while in a fresh state, with considerable force, just as a savage would do in hacking the flesh off a stranded whale. Cuts exactly similar can now be made on fresh bone by such flint knives, and in no other known or conceivable way. It seems, therefore, more like obstinate prepossession, than scientific skepticism, to deny the existence of Tertiary man. If it rested only on this single instance."

Continuing his commentary, Laing (1893, p. 116) stated: "As regards the evidence from cut bones it is very conclusive, for experienced observers, with the aid of the microscope, have no difficulty in distinguishing between cuts which may have been made accidentally or by the teeth of fishes, and those which can only have been made in fresh bone by a sharp cutting Instrument, such as a flint knife."

A modern authority, Binford, stated (1981, p. 169): "There is little chance that an observer of modified bone would confuse cut marks inflicted during dismembering or filleting by man using tools with the action of animals." Binford (1981, p. 169) further noted: "The marks of animals' teeth are somewhat different. They follow the contours of the bone's surface. . . . Tooth marks may frequently take the form of depressed or mashed lines. . . . On many of the wolf specimens, the tooth mark under magnification appears as a 'cracked' surface scar rather than as a cut or incision in the bone."

But the teeth of sharks are sharper than those of terrestrial mammalian carnivores such as wolves and might produce marks on bone that more closely resemble those that might be made by cutting implements. After inspecting fossil whale bones in the paleontology collection of the San

Diego Natural History Museum. we concluded that shark's teeth can in fact make marks closely resembling those that might be made by implements. However, we also concluded that it is nevertheless possible, in some cases, to distinguish marks made by implements from those made by shark teeth.

Figure 23. Tooth of *Carcharodon megalodon*, a Pliocene great white shark (G de Mortillet and A. de Mortillet 1881, plate 4, figure 19).

The bones we saw were from a small Pliocene species of baleen whale. The marks on one bone, a jaw fragment, were the subject of a report by Thomas A. Demere and Richard A. Cerutti (1982) of the San Diego Natural History Museum. The ventral margin of the jaw fragment showed a pair of V-shaped grooves that ran transversely to that surface (Demere and Cerutti 1982, p. 1480). One of the marks measured 16 mm (0.63 inch) long, and slightly curved. The other one ran 11 mm (0.43 inch) in a straight line. Our inspection of the incisions through a magnifying lens showed evenly spaced parallel longitudinal striations such as one would expect from the serrated edge of a shark's tooth (Figure 2.3). Even so, Demere, who showed us the marked fossil at the San Diego Natural History Museum paleontology collection on May 31, 1990, stated that as far as he was concerned these V-shaped incisions alone were inconclusive. That is to say, they might have been caused by something other than shark teeth.

More useful for diagnostic purposes was another mark on the bone. Demere and Cerutti (1982, p. 1480) described this as a beveled surface "characterized by 12 sinuous but parallel small-scale ridges and grooves." Demere and Cerutti (1982, p. 1480) went on to state: "This very distinctive pattern has been duplicated by using a piece of paraffin and a tooth from the Pliocene great white shark, *Carcharodon siilcidens* Agassiz, 1843. . . . The teeth of *Carcharodon* are characterized by serrated

Figure 2.4. Pattern of grooves and ridges produced by a serrated shark tooth moving across the surface of a whale bone (Demere and Cerutti 1982, p. 1481).

margins." The pattern of grooves and ridges observed on the fossil whale bone (Figure 2.4) could have been produced by a glancing blow, with the edge of the tooth scraping along the surface of the bone rather than cutting into it. With this knowledge, it should be possible to reexamine the Pliocene whale bones of Italy and arrive at some fairly definite conclusions as to whether or not the marks on them were made by shark teeth. Patterns of parallel ridges and grooves on the surfaces of the fossils, such as those described by Demere and Cerutti, would be an almost certain sign of shark predation or scavenging. And if close examination of deep V-shaped cuts also revealed

evenly spaced, parallel longitudinal striations, that, too, would have to be taken as evidence that shark teeth made the cuts. One would not expect the surfaces of marks made by flint blades to display evenly spaced striations.

Even so, care would have to be taken to examine each and every cut on the fossil whale bones. Demere and Cerutti (1982, p. 1480) reported that carcasses of sea otters, with the bones marked by shark teeth, have been found washed up on the California coast. One can imagine that in the past a whale carcass, partially devoured by sharks, might similarly have washed ashore, and then been butchered by humans. Therefore fossil whale bones might bear both the marks of shark teeth and human implements.

The following statement by Demere and Cerutti (1982, p. 1480) calls attention to one of the drawbacks of the way anomalous evidence is treated by the scientific community: "It appears then that our fossil specimen preserves a late Pliocene scavenging and/or predator event by *Carcharodon* on cetaceans. To our knowledge this represents the first well- documented report of such activity." It is significant that two working paleontologists, with a special interest in shark teeth and whale bones, were unaware of the extensive debate that occurred in the nineteenth century on the topic of possible *Carcharodon* (versus human) markings on Pliocene cetaceans. Therefore, rather than casting controversial evidence into oblivion, it would be wiser, perhaps, to somehow keep it readily available for further study. That is one purpose of this book.

#### 2.12 Halitherium of Pouance, France (Middle Miocene)

In 1867, L. Bourgeois caused a great sensation when he presented to the members of the International Congress of Prehistoric Anthropology and Archeology, meeting in Paris, a Halitherium bone bearing marks that appeared to be human incisions (de Mortillet 1883, p. 53). Halitherium is a kind of extinct sea cow, an aquatic marine mammal of the order Sirenia.

The fossilized bones of Halitherium had been discovered by the Abbe Delaunay in the shell beds at Barriere, near Pouance in northwestern France (Maine-et-Loire). Delaunay was surprised to see on a fragment of the humerus, a bone from the upper forelimb, a number of cut marks (Figure 2.5). The surfaces of the cuts were of the same appearance as the rest of the bone and were easily distinguished from recent breaks, indicating that the cuts were quite ancient. The bone itself, which was fossilized, was firmly situated in an undisturbed stratum, making it clear that the marks on the bone were of the same geological age. Furthermore, the depth and sharpness of the incisions showed that they had been made before the bones had fossilized.

Figure 25. Cut marks on Halitherium bone from the Miocene at Pouance, France (de Mortillet 1883, p. 54).

Some of the incisions appeared to have been made by two separate intersecting strokes. Even de Mortillet (1883, pp. 53-55) admitted that they did not appear to be the products of subterranean scraping or compression. But he would not admit they could be the product of human work, mainly because of the age of the stratum in which the bones were found. The shell beds of this region were said to date to the period represented by the Mayencian formation of the Middle Miocene. But they could be somewhat older. The marine layers in which the Halitherium bone was discovered, known as the Faluns of Anjou, are assigned by modern authorities to the Early Miocene (Klein 1973, table 6). Halitherium is generally thought to have existed in Europe from the Early Miocene to the Early Oligocene (Romer 1966, p. 386).

De Mortillet (1883, p. 55) wrote in his book *Le Préhistorique*, "This is much too old for man." It is easy enough to see how a scientist who was committed to the evolutionary hypothesis would think so—the Middle Miocene dates as far back as 15 million years, and the Early Miocene to somewhere around 25 million years.

Here again, we have a clear case of theoretical preconceptions dictating how one will interpret a set of facts. De Mortillet (1883, p. 55) attributed the marks on the bones to large sharks of the requin family: "It is a fact that the shell beds of Anjou contain an abundance of sharp pointed teeth of fish of this family. These fish, encountering Halitherium beached on the coast, then ate them and left on their bones the numerous marks of their voracity and the strength of their teeth." De Mortillet (1883, p. 55) also stated that on May 5, 1879 Mr. Toumouer presented to the Geological Society of France an incised Halitherium bone, attributing the marks to shark teeth. However, in light of the foregoing discussion, it seems the case of the Halitherium bone of Pouance should remain open for further investigation.

On the general subject of cut bones as a category of viable evidence, Laing (1894, pp. 353-354) wrote in his book *Human Origins*, which went through five reprintings: "cut bones afford one of the most certain tests of the presence of man. The bones tell their own tale, and their geological age can be certainly identified. Sharp cuts could only be made on them while the bones were fresh, and the state of fossilization, and presence of dendrites or minute crystals alike on the side of the cuts and on the bone, negate any idea of forgery. The cuts can be compared with thousands of undoubted human cuts on bones from the reindeer and other later periods, and with cuts now made with old flint knives on fresh bones. All these tests have been applied by some of the best anthropologists of the day, who have made a special study of the subject, and who have shown

their caution and good faith by rejecting numerous specimens which did not fully meet the most rigorous requirements. . . . The only possible alternative suggested is, that they might have been made by gnawing animals or fishes. But as Quatrefages observes, even an ordinary carpenter would have no difficulty in distinguishing between a clean cut made by a sharp knife, and a groove cut by repeated strokes of a narrow chisel; and how much more would it be impossible for a Professor trained to scientific investigation, and armed with a microscope, to mistake a groove gnawed out by a shark or rodent for a cut made by a flint knife."

Laing's observations are significant in that they counter certain modern prejudices about the caliber of scientific work at that time. On first encountering reports like those concerning the cut bones of St. Prest, Monte Aperto, or Pouance, one might think something like this: "How quaint these nineteenth-century scientists were, in those old days of the infancy of paleoanthropological investigation. How quick they were to accept questionable evidence upon cursory inspection." But from Laing's statements we can see that scientists like de Quatrefages, Desnoyers, and Capellini were carefully applying standards of investigation and evaluation comparable to those of the present day. In particular, they displayed a considerable grasp of the principles of the modern discipline of taphonomy. One might also postulate something like the following: "Well, perhaps in the nineteenth century, before there were many actual human fossils uncovered, these naturalists focused undue attention on these cut bones, reading too much into them, because they had nothing else to concern themselves with." But even today, many researchers are investigating the presence of humans at certain sites solely on the basis of animal bones bearing signs of intentional workmanship. And, as we shall see in coming chapters, it is not true that nineteenth-century naturalists interested in human antiquity had nothing but cut bones to study. They also extensively investigated many finds of stone tools and human skeletal remains that have since slipped into near total obscurity.

### 2.13 San Valentino, Italy (Late Pliocene)

In 1876, at a meeting of the Geological Committee of Italy, M. A. Ferretti showed a fossil animal bone bearing "traces of work of the hand of man, so evident as to exclude all doubt to the contrary" (de Mortillet 1883, p. 73). This bone, of elephant or rhinoceros, was found finally in place in Astian (Late Pliocene) strata in San Valentino (Reggio d'Emilia), Italy. The bone's dimensions are 70 mm (2.8 inches) by 40 mm (1.6 inches). Of special interest is the fact that the fossil bone has an almost perfectly round hole at the place of its greatest width. According to Ferretti, the hole in the bone was not the work of molluscs or crustaceans. The next year Ferretti showed to the Committee another bone bearing traces of human work. It was found in blue Pliocene clay, of Astian age, at San Ruffino. This bone appeared to have been partially sawn through at one end, and then broken. De Mortillet, who included the above-mentioned information in his book, stated (1883, p. 77) that he had not seen the bones nor heard any further discussion about them. This indicated to him that they had not been (and thus should not be) taken seriously. It would perhaps have been more appropriate, and scientific, for de Mortillet to have inspected the bones before concluding they were of little scientific value. Many modern scientists react in a similar fashion when confronted with unfamiliar, little-discussed anomalous evidence. They assume it is not of



any importance; otherwise they would have seen it discussed in the published works of scientists committed to the established views.

At a scientific conference held in 1880, G. Bellucci, of the Italian Society for Anthropology and Geography, called attention to recent discoveries in San Valentino and Castello delle Forme, near Perugia. Found there were bones of different animals bearing incisions, both straight and intersecting, and with imprints probably made with rocks employed for the purpose of breaking the bones. Bellucci said there were also two specimens of carbonized bones, and finally flint flakes. All were recovered from lacustrine Pliocene clays, characterized by a fauna like that of the classic Val d'Arno. According to Bellucci, these objects proved the existence of man in the Tertiary period in Umbria (Bellucci and Capellini 1884).

#### 2.14 Clermont-Ferrand, France (Middle Miocene)

Turning once more to France, we note that in the late nineteenth century the museum of natural history at Clermont-Ferrand had in its collection a femur of *Rhinoceros paradoxus* with grooves on its surface. The specimen was found in a freshwater limestone at Gannat, in a quarry said to be dated by fossils to the Mayencian age of the Middle Miocene (de Mortillet 1883, p. 52). M. Pomel presented this piece to the anthropological section of the French Association for the Advancement of Science meeting of 1876 in Clermont. Pomel said the marks were from carnivores, which were numerous in the French Middle Miocene. But de Mortillet disagreed that an animal could have been responsible. He pointed out that the grooves on the Miocene rhinoceros femur could not have been made by a rodent, because rodent incisors usually leave pairs of parallel marks. The grooves on the rhinoceros femur were not arranged in pairs. De Mortillet also believed that the marks were not caused by larger carnivores, because, as noted by Binford (1981, p. 169) in modern times, carnivore teeth leave many irregular impressions and cause distinctive patterns of bone destruction. Binford stated that "association of scoring with patterns of destruction is not expected when man dismembers an animal with tools." According to this standard, the Miocene rhinoceros femur, which displayed scoring but no pattern of destruction, might very well have been cut by ancient humans using stone tools.

For de Mortillet, however, the marks were a purely geological phenomenon. He concluded that the grooves on the rhinoceros femur of Clermont-Ferrand were probably produced by the same subterranean pressures responsible for the marks on the Billy specimen (de Mortillet 1883, p. 52). But de Mortillet's own description (1883, p. 52) of the markings on the bone leaves this interpretation open to question: "The impressions occupy a portion of the inner surface near the condyles. They are parallel grooves, somewhat irregular, transverse to the axis of the bone." The condyles are the rounded prominences on the articulator, or joint, surfaces at the end of the femur, or thighbone. The orientation and position of the

marks on the fossil were identical to those of incisions made in the course of butchering operations on a long bone such as the femur. Binford's studies (1981, p. 169) revealed: "cut marks

are concentrated on articulator surfaces and are relatively rare as transverse marks on long bone surfaces. ... cut marks from stone tools are most commonly made with a sawing motion resulting in short and frequently multiple but roughly parallel marks. Such marks are generally characterized by an open cross section. Another characteristic of cut marks derived from the use of stone tools is that they rarely follow the contours of the bone on which they appear. That is, the cut does not show equal pressure in depressions and along prominent ridges or across the arc of a cylinder." As described by de Mortillet, the short parallel grooves found on the Miocene rhinoceros femur conform to these criteria, leaving one to wonder how it is possible that chance geological pressures could so closely duplicate, in terms of position and character, the distinctive marks of human butchering.

The Miocene dating of the Clermont-Ferrand site is confirmed by the presence of *Anthracotherium magnum*, an extinct mammal of the hippopotamus family. In fact, the site could be older than Middle Miocene. According to one modern authority, *Anthracotherium* existed in Europe from the Late Miocene to the Early Eocene (Romer 1966. p. 389). Savage and Russell (1983. p. 245) last report *Anthracotherium* in the Orléanien land mammal stage of the Early Miocene.

#### 2.15 Carved Shell from the Red Crag, England (Late Pliocene)

In a report delivered to the British Association for the Advancement of Science in 1881. H. Stopes, F.G.S. (Fellow of the Geological Society), described a shell, the surface of which bore a carving of a crude but unmistakably human face. The carved shell was found in the stratified deposits of the Red Crag (Stopes 1881. p. 700). The Red Crag, part of which is called the Walton Crag, is thought to be of Late Pliocene age. According to Nilsson (1983. p. 308). the Red (Walton) Crag is between 2.0 and 2.5 million years old.

British Association meeting, it was considered wrong to suggest that man could have been alive at so early a date." Arguing against forgery, Marie Stopes (1912, p. 285) stated: "It should be noted that the excavated features are as deeply coloured red-brown as the rest of the surface. This is an important point, because when the surface of Red Crag shells are scratched they show white below the colour. It should also be noticed that the shell is so delicate that any attempt to carve it would merely shatter it." It is therefore quite possible that this shell was carved and deposited in the Red Crag strata during the Late Pliocene. If true, this would place intelligent human beings in England as far back as 2.0 million and maybe as much as

2.5 million years ago. One should keep in mind that in terms of conventional paleoanthropological opinion, one does not encounter such works of art until the time of fully modern Cro-Magnon man in the Late Pleistocene, about 30,000 years ago.

Discoveries of incised bones dating back to the Pliocene or earlier persisted into the early part of the twentieth century. Opposition to them also persisted, and eventually prevailed. For example,

Hugo Obermaier, professor of prehistoric archeology at the University of Madrid, wrote (1924, pp. 2-3): 'traces (chiefly fluted, engraved, or grooved) have been observed on the bones of animals and shells of molluscs in Tertiary deposits at Saint-Prest, Sansan, Pouance, and Billy, France; in the Tertiary basin of Antwerp, Holland; at Monte Aperto near Siena, Italy; in North and South America: and in several other places. ... it is easy to explain supposed traces of human activity as the result of natural causes—such, for example, as the gnawing or biting of animals, earth pressure, or the friction of coarse sand.' But can we say for certain that this "easy" explanation is the correct one?

## 2.16 Bone implements From Below the Red Crag, England (Pliocene to Eocene)

In the early twentieth century, J. Reid Moir, the discoverer of many anomalously old flint Implements (Section 3.3), described "a series of mineralised bone Implements of a primitive type from below the base of the Red and Coralline Crag of Suffolk" (1917a, pp. 116-131). The top of the Red Crag in East Anglia is now considered to mark the boundary of the Pliocene and Pleistocene, and would thus date back about 2.0-2.5 million years (Romer 1966, p. 334; Nilsson 1983, p. 106). The older Coralline Crag is Late Pliocene and would thus be at least 2.5-3.0 million years old. The beds below the Red and Coralline Crag, the detritus beds (Table 2.1, p. 78), contain materials ranging from Pliocene to Eocene in age (Section 3.3.2). Objects found there could thus be anywhere from 2 million to 55 million years old. One group of Moir's specimens is of triangular shape (Figure 2.7). In his report, Moir (1917a, p. 122) stated: "These have all been formed from wide, flat, thin pieces of bone, probably portions of large ribs, which have been so fractured as to now present a definite form. This triangular form has, in every case, been produced by fractures across the natural 'grain' of the bone." Moir (1917a, p. 116) then began to describe some of his attempts to reproduce the specimens: "having conducted a number of experiments in which mineralised and unmineralised bones were subjected to the effects of fortuitous blows and pressure, and after having fractured numerous modern shank bones of the bullock by striking and cutting them with flints and other stones held in the hand with a view of thus shaping them to the forms of the sub-Crag examples, he [the author] is compelled to regard these latter specimens as undoubted works of man." According to Moir, the triangular pieces of fossilized whale bone discovered in the strata below the Coralline Crag might have once been used as spear points.

Figure 2.7. Three bone tools from the detritus bed beneath the Coralline Crag, which contains materials ranging from Pliocene to Eocene in age. These implements could thus be anywhere from 2 to 55 million years old (Moir 1917a, plate 26).

Moir had himself collected most of the specimens, but he also described one discovered by another naturalist, a Mr. Whincopp, of Woodbridge In Suffolk, who had In his private collection a 'piece of fossil rib partially sawn across at both ends" (Moir 1917a, p. 117). This object came from the detritus bed below the Red Crag and was 'regarded by both the discoverer and the late Rev. Osmond Fisher as affording evidence of human handiwork" (Moir 1917a, p. 117). Indications of sawing would be quite unexpected on a fossil bone of this age. A piece of sawn wood was recovered from the more recent Cromer Forest Bed in the same region (Section 2.20).

Osmond Fisher, who was a Fellow of the Geological Society, made some interesting discoveries of his own. In a review published in *The Geological Magazine*, Fisher (1912, p. 218) wrote: "When digging for fossils in the Eocene of Barton Cliff I found a piece of jet-like substance about 9<sup>2</sup> inches square and 21<sup>1</sup>", inches thick. ... It bore on at least one side what seemed to me marks of the chopping which had formed it into its accurately square shape. The specimen is now in the Sedgwick Museum, Cambridge." Jet is a compact velvety-black coal that takes a good polish and is often used as jewelry. The Eocene period dates back about 38-55 million years from the present.

#### 2.17 Dewlish Elephant Trench, England (Early Pleistocene to Late Pliocene)

Osmond Fisher also discovered an interesting feature in the landscape of Dorsetshire—the elephant trench at Dewlish. Fisher (1912, pp. 918 - 919) stated in his 1912 review: "This trench was excavated in chalk and was 12 feet deep, and of such a width that a man could just pass along it. It is not on the line of any natural fracture, and the beds of flint on each side correspond. The bottom was of undisturbed chalk, and one end, like the sides, was vertical. At the other end it opened diagonally on to the steep side of a valley. It has yielded substantial remains of *Elephas meridionalis*, but no other fossils. ... This trench, in my opinion, was excavated by man in the later Pliocene age as a pitfall to catch elephants; and if so, it proves that he was already an intelligent and social being." *Elephas meridionalis*, or 'southern elephant,' was in existence in Europe from 1.2 to 3.5 million years ago (Maglio 1973, p. 79). Thus, while the bones found in the trench at Dewlish could conceivably be Early Pleistocene in age, they might also date to the Late Pliocene.

In Fisher's original reports in the Quarterly Journal of the Geological Society of London, we find the following more detailed description: "The trench was . . . followed for about 103 feet, until it suddenly terminated in a smooth 'apse-like' end. . . It was a deep, narrow trench, with nearly vertical sides of undisturbed chalk. Mr. [Clement] Reid says: The fissure (or rather trough) ended abruptly, without any trace of a continuing joint: It was not a fault, for the lines of flint-nodes corresponded on each side" (O. Fisher 1905, p. 35). The base of the trench was reported to be a smooth surface of chalk, twelve feet down (O. Fisher 1905, p. 36). Photographs accompanying the report show the vertical walls of the trench, carefully chipped as if with a large chisel.

In response to suggestions that flowing water might have excavated the trench, Fisher (1905, p. 36) stated: "A stream in such a locality would be unlikely to excavate a deep and narrow channel, much less, if it did so, would it come to an abrupt ending. And, even if we could account for the natural formation of such a trench, how came it that the remains of so many elephants were found in it, and (so far as appears) no other animals?"

Fisher (1905, p. 36) referred to reports showing that primitive hunters of modern times made use of similar trenches: "Sir Samuel Baker describes this method of taking elephants by natives of Africa. He says that an elephant cannot cross a ditch with hard perpendicular sides, which will not crumble nor yield to pressure. Pitfalls 12 to 14 feet deep are dug in the animals' routes towards drinking-places, and covered with boughs and grass. The pits are made of different shapes, according to the individual opinions of the trappers. When caught, the animals are attacked with spears while in their helpless position, until they at last succumb through loss of blood. . . If the stream which now runs at the bottom of the hill, despite subsequent changes in the contour of the country already existed, then this trench would have been made in a position suitable to intercept the route to the drinking place."

Some critics pointed out that the trench appeared too narrow to accommodate a fully grown elephant, but evidently the deep trench was simply meant to incapacitate an adult animal by injuring its legs or to capture a young animal. Also, further excavation of the trench by the Dorset Field Club, as reported in a brief note in Natuie (October 16, 1914; p. 511), revealed that "instead of ending below in a definite floor it divides downward into a chain of deep narrow pipes in the chalk." But it is not unlikely that ancient humans might have made use of small fissures to open a larger trench in the chalk. It would be worthwhile to examine the elephant bones found in the trench for signs of cut marks or selective preservation.

## 2.18 More on implements From Below the Red Crag (Pliocene to Eocene)

Ten years after his first report (Section 2.16), J. Reid Moir (1927, pp. 31-32) again described fossilized bone implements taken from below the Red Crag formation (Figure 2.7): "In the sub-Red Crag Bone Bed where these flint implements are found, there are a number of bones comprising, chiefly, pieces of whale rib, very highly mineralised. Among these I have found certain specimens that have every appearance of having been shaped by man. Such pieces are of great rarity and assume, usually, a definite pointed form which cannot well have been produced by any natural, non-human means. The 'worked' portions of these bones show the same deep and ancient

coloration of the other parts of the specimens, and experiments which I have carried out demonstrate that, in the present mineralised state of the bones, it is not possible to shape them to the forms they have assumed. In order to produce such forms from bone I found it necessary to operate on fresh specimens, and that these, by flaking' and rubbing with a hard quartzite pebble, could be made into shapes quite comparable with those found below the Red Crag. I have little doubt, therefore, that these latter specimens have been shaped by man and represent the most ancient bone implements yet discovered.'

Bone implements, like Incised bones, remain a major category of paleoanthropological evidence. For example, Mary Leakey (1971, p. 235) has reported from Olduvai Gorge in Africa: "It is probable that the majority of the broken mammalian bones found on living sites in Bed I and II at Olduvai merely represent food debris. Some may also have been further broken by carnivores after the sites were abandoned. There is, however, a relatively small number which appear to have been artificially flaked and abraded.'

Leakey (1971, p. 235) then gave the following example: "Part of an equid [horse family] first rib showing evidence of polishing and smoothing at the fractured end. . There is an oblique fracture of the shaft of the rib, towards the proximal end, which runs transversely from the lower to the upper margin. One edge of the fracture is abraded and smooth, showing that the bone was used after it had been broken.' '

She also described a series of humeri (the bones of the upper forelimb): 'A proportion of these specimens appears to represent the ends of bones in which the shafts were shattered to extract the marrow and which have been subsequently utilised, but others, including the pointed series and those split longitudinally, seem to have been expressly shaped' (M. Leakey 1971, p. 236).

Leakey qualified her apparent acceptance of these implements with only this statement: 'At the time of this writing there is, as yet, no general agreement regarding the extent to which bone was worked and used in Lower and Middle Pleistocene times. It is evident that more basic research on the effect of artificial fracture and use of bone, as distinct from damage caused by natural means, is required before bone debris from early living sites can be satisfactorily interpreted' (M. Leakey 1971, p. 235).

Despite this cautionary remark, Mary Leakey's statements about the bone implements of Olduvai Gorge seemed positive. The question is this: will scientists show the same openmindedness in the case of the sub-Crag bone tools reported by J. Reid Moir? If the answer is yes, then paleoanthropologists will have to rework their ideas about human origins to include toolmaking humans over 2 million years ago, and maybe as much as 55 million years ago, in England.

## 2.19 Implements from Cromer Forest Bed, England (Middle to Early Pleistocene)

J. Reid Moir (1927, pp. 49-50) also wrote of bone tool finds from the Cromer Forest Bed: "During this year (1926) Mr. J. E. Sainty found upon the beach at Overstrand a piece of heavily mineralized bone which is evidently referable to the Cromer Forest Bed. ... the bone is of a markedly

implemental form: In fact, on the surface figured and at the butt- end, it exhibits flaking and hacking, which, Judging from the experiments I carried out in shaping this material, I think has been intentionally produced. ... Sir Arthur Keith, F.R.S. [Fellow of the Royal Society], who examined the specimen, has kindly given me the following opinion upon it There can be no doubt, I think, that your implement has been fashioned out of the lower Jaw of the larger whalebone whales. None of the original surface of the bone is left: it has been removed by flaking.' From the extreme fossilization of this specimen, I Judge It to belong to the earliest Cromer Forest Bed deposit, and to be contemporary with the great flint implements found at that horizon. Remains of whales have been discovered in the Forest Bed and it was doubtless the skeleton of one of these that supplied the material from which this Implement was made by one of the earliest Cromerian men."

The most comprehensive recent study of the Cromer Forest Bed formation is by R. G. West. According to West (1980, p. 201), the oldest part of the Cromer Forest Bed is the Sheringham member. West Identified the lower part of the Sheringham member, representing the base of the Cromer Forest Bed, with the Pre-Pastonian cold stage of East Anglia ( Table 2.1, p. 78).

Even after much study, West was not able to give a conclusive date for the Pre-Pastonian. He suggested that the lowest level of the Pre-Pastonian, might be equivalent to the basal part of the northwestern European cold stage called the Erburonian. This would give the Pre-Pastonian cold stage a maximum age of about 1.75 million years (West 1980, fig. 54). But Nilsson (1983, p. 308) puts the base of the Erburonian at 1.5 million years.

TABLE 1.1 Stratigraphy of East Anglia

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Division

Stages of West ::1980)

Cramer Till (g)

^glian (g)

Cromer Forest Bed

Cromerian (t)

lower limit (Nilsson)

Beestonian (c)

Weyboimie »' \*;

Pastonian (t)

NofVi, icli Crag

(West)

Pre-Pastonian (c >

Red Crag

Waltonian (c)

Detritus Bed (Cretaceous ^ Pliocene)

Coralline Crag (Pliocene)

Detritus: Bed (Cretaceous ^ Pliocene)

London Clay (Eocene)

According to West (1980, fig. 54), the Pre-Pastonian cold stage of EastAnglla might also be Identified, on paleomagnetic grounds. with the Menapian glaciation of northwestern Europe at .8-.9 million years. The Pre-Pastonian might also be Identified with the early part of the northwestern European Cromer complex, a series of alternating glacials and interglacials extending from about A million to .8 million years ago ( West 1980, p. 120; Nilsson 1983, p. 308). The early part ofthe Cromer complex of glacials and interglacials can be estimated at about .6-.8 million years according to the correlation table of Nilsson (1983, p. 308).

Therefore, according to West. the Cromer Forest Bed series might be as old as 1.75 million years or as young as .6-.8 million years. Nilsson (1983, p. 308) shows the CromerForest Bed series beginning at about .8 millionyears ago.

So if the heavily mineralized bone implement reported by Moir actually did come from the lowest levels of the Cromer Forest Bed, as he surmised, it might be as much as 1.75 million years old. The oldest Homo erectus fossils from Africa only date back about 1.6 million years.



If, however, we take the younger of the possible dates for the oldest levels of the Cromer Forest Bed (about .6 million years) that would still be quite anomalous for England. According to Nilsson (1983, p. 111). the oldest stone tools from England come from Westbury-sub-Mendip deposits equivalent to the terminal phase of the Cromer Forest Bed, at about A million years ago.

Of course, Moir could have been wrong about the source of the mineralized bone implement. The beds at Overstrand cover almost the entire span of Cromer Forest Bed time (West 1980, p. 159). Thus the implement from Overstrand might have come not from the earliest but from the latest part of the Cromer Forest Bed sequence, making it the same age as the stone tools from Westbury-sub-Mendip. about A million years old—quite within the range of conventional acceptability. This possibility makes it all the more remarkable that the bone tool reported by Moir is not given serious attention by modern paleoanthropologists.

In some additional remarks on the Cromer Forest Bed discoveries, Moir (1927, p. 50) went on to describe incised bones rather than bones modified as tools: "The discovery of flint implements in the Forest Bed Induced me to make a close examination of the mammalian bones from this deposit, in the possession of Mr. A. C. Savin of Cromer. This examination revealed three specimens, all found in the peat, representing the upper part of the Forest Bed at West Runton, by Mr. Savin, which show on their surface clearly defined cuts which, I think, can only have been produced by flint knives in removing flesh ... the Cromer examples are quite comparable with others exhibiting cuts which I have discovered in various later prehistoric epochs. The lines are fine, and straight, and were evidently produced by a sharp-edged flint. Some of the smaller mammals might cut a bone with their teeth in a similar way, but they could not produce such long cuts as are present on the bones from West Runton. Nor is it possible to regard these markings as due to glacial action."

The part of the Cromer Forest Bed sequence represented especially well at West Runton is the Upper Freshwater Bed. According to West, the Upper Freshwater Bed, as defined during Moir's time, contained elements as old as the Pastonian temperate stage. The Pastonian stage of East Anglia was thought by West (1980, fig. 54) to be equivalent to the latter part of the Waalian temperate stage of northwestern Europe, dated at 1 million years (Nilsson 1983, p. 308).

Alternatively, the Pastonian temperate stage might correlate with an interglacial within the Cromer glacial complex. at about .5 million years. In any case, West (1980, p. 116) believed most of the Upper Freshwater Bed was within the time range of the Cromer complex of northwestern Europe, giving it an age of A-.8 million years (Nilsson 1983, p. 308).

Taken together, the different estimates of the age of the Upper Freshwater Bed would give the cut bones from West Runton a possible date range of between OA and 1.0 million years. At the older end of the date range, the cut bones would be extremely anomalous: at the younger end, less so.

Moir observed that the marks on the West Runton bones were not of the kind produced by glaciers and further noted that the bed in which the specimens were found contained many fragile, unbroken shells and thus appeared undisturbed. "The bones comprise part of the humerus

of a large bison, and portions of the lower Jaws, with teeth in place, of deer," stated Moir (1927, p. 50). The cuts, he also observed, ran under thick ferruginous deposits, indicating their great age. "I have recently carried out some experiments in scraping modern bones with a sharp flake of flint," continued Moir (1927, p. 51), "and find that the cuts so produced are in every way comparable with those upon the Cromer examples. It was noticed that these latter specimens, in addition to the easily recognised cuts, exhibited a large number of minute Incisions which could only be examined adequately by means of a lens. Upon the experimental bones I found that a precisely similar assemblage of small cuts was present, and I have no doubt that these are due to the microscopic projections present on the cutting-edge of the flint which I used." The specific identifying characteristics of Incisions made with flint flakes

on bone have been confirmed by modern investigators such as Rick Potts and Pat Shipman. John Gowlett (1984, p. 53) stated: "Their work involved use of the electron microscope, at a very high magnification. They found that many bones

from Olduvai preserved carnivore gnawing marks, as well as stone tool cut-marks. Very close parallel striations were indisputable evidence of the stone tools, for no edge of a flake is perfectly straight, and each protruding sharp piece leaves its mark. " It is apparent that Moir's methods of identification compare favorably with those employed by modern professional paleoanthropologists.

## 2.20 Sawn Wood from Cromer Forest Bed, England (Middle to Early Pleistocene)

J. Reid Moir (1927, p. 47) also described a piece of cut wood from the Cromer Forest Bed ( Figure 2.8) that suggested human action: "the late Mr. S. A. Notcutt of Ipswich dug out of this deposit, at the foot of the cliff near Mundesley, a piece of wood which, in my opinion, was shaped by man. The bed in which the wood was found consisted of undisturbed sand and gravel. and was overlain by Lower Glacial Clay in situ."

Figure 2.8. Piece of wood from the Cromer Forest Bed, England. The piece of wood, apparently sawn at the right end, is between 0.5 and 1.75 million years old (Moir 1917b).

The beds at Mundesley extend from the lattermost Cromer Forest Bed times, at about .4 million years, to the lower part of the Pre-Pastonian cold stage, estimated variously at 0.8 or 1.75 million

years (West 1980, p. 182; Nilsson 1983, p. 308). But most of the Mundesley strata are identified with the Cromerian temperate stage of East Anglia (West 1980, p. 201). One should note that the Cromerian temperate stage of East Anglia, dated roughly at .4-.5 million years, is not the same as the Cromer complex of northwestern European glacials and interglacials, dated at .4-.8 million years (Nilsson 1983, p. 308).

Figure 2.9. Cross section of a piece of cut wood from the Cromer Forest Bed. The arrow indicates a groove, possibly from an initial cut by a sawing implement (Moir 1917b).

In the course of his comments about the piece of cut wood, Moir (1927, p. 47) made these observations: "The specimen, which is quite comparable with other wood found in the Forest Bed, is . . . slightly curved, four-sided, and is flat at one end and pointed at the other. . . . The flat end appears to have been produced by sawing with a sharp flint, and at one spot it seems that the line of cutting has been corrected [Figure 2.9], as is often necessary when starting to cut wood with a modern steelsaw. The present form of the specimen is due to the original round piece of wood—which has been identified by Dr. A. B. Rendle, F.R.S., as yew—having been split four times longitudinally in the direction of its natural grain. The pointed end is somewhat blackened as if by fire, and it is possible that the specimen represents a primitive digging stick used for grubbing up roots."

While there is an outside chance that beings of the *Homo erectus* type might have been present in England during the time of the Cromer Forest Bed, the level of technological sophistication implied by this sawn wood tool is extraordinary and suggestive of sapiens-like capabilities. In fact, it is hard to see how this kind of sawing could have been produced even by stone implements. Small flint chips mounted in a wooden holder, for example, would not have produced the clean cut evident on the specimen because the wooden holder would have been wider than the flint teeth. Hence one could not have cut a narrow groove with such a device. A saw blade made only of stone would have been extremely brittle and would not have lasted long enough to perform the operation. Furthermore it would have been quite an accomplishment to make such a stone blade. Thus it seems that only a metal saw could produce the observed sawing. Of course, a metal saw at .4-.5 million years is quite anomalous.

It is remarkable that the incised bones, bone implements, and other artifacts from the Red Crag and Cromer Forest Beds are hardly mentioned at all in today's standard textbooks and references. This is especially true in the case of the Cromer Forest Bed finds, most of which are, in terms of their age, bordering on the acceptable, in terms of the modern paleoanthropological sequence of events.

In Gowlett's *Ascent to Civilization* (1984, p. 88), we read: 'There is a possibility that some finds from Britain are older than the Hoxnian [an interglacial period dated approximately 330,000 years ago]: for example the high terrace finds from Fordwich and from Kent's Cavern near Torquay. The importance of such finds lies in the demonstration that perhaps as much as 500,000 years ago, man was able at least for a time to colonize Europe out to its extremities. At Westbury-sub-Mendip, in south-west England, remains of extinct animals associated with very few stone tools suggest contemporaneity with the Cromerian phase, estimated at c. 0.7-0.5 million years, and named after beds in eastern England, where there are faunal remains but no archaeological traces.' Elsewhere Gowlett stated "it is safest to assume that the first occupation of Europe would have been by tool-making men in the earlier Pleistocene." This would "imply a date about 1.5 million years ago" (Gowlett 1984, p. 76).

Considering that Gowlett was prepared to find evidence of toolmaking humans in Europe at 1.5 million years ago, it is odd to find him stating that the Cromer Forest Bed contains "no archaeological traces" (Gowlett 1984, p. 88). Gowlett, a professor at Oxford University, should have been knowledgeable about the recent history of paleoanthropology in England. Was he unaware that in the early twentieth century Moir and others found bone tools, incised bones, and other artifacts (including a whole flint industry) in the Cromer Forest Bed? That would seem unlikely. Did he think the finds to be not genuine? Perhaps he was aware of the discoveries and considered them genuine but deliberately avoided including them in his discussion, even though they would have helped his case. Why? It could be that mentioning them would have implied his acceptance of the still older sub-Red Crag discoveries of Moir and others, which pose a strong challenge to the whole scenario of human origins and antiquity.

## 2.21 Concluding Words about Intentionally Modified Bone

It is really quite curious that so many serious scientific investigators in the nineteenth century and early twentieth century independently and repeatedly reported that marks on bones from Miocene, Pliocene, and Early Pleistocene formations were indicative of human work. Among the researchers making such claims were Desnoyers, de Quatrefages, Ramorino, Bourgeois, Delaunay, Bertrand, Laussedat, Garrigou, Filhol, von Ducker, Owen, Collyer, Calvert, Capellini, Broca, Ferretti, Bellucci, Stopes, Moir, Fisher, and Keith.

Were these scientists deluded? Perhaps so. But cut marks on fossil bones are an odd thing about which to develop delusions —hardly romantic or inspiring. Were the above-mentioned researchers victims of a unique mental aberration of the last century and the early part of this one? Or does evidence of primitive hunters really abound in the faunal remains of the Tertiary and early Quaternary?

Assuming such evidence is there, one might ask why it is not being found today. One very good reason is that no one is looking for it. Evidence for intentional human work on bone might easily escape the attention of a scientist not actively searching for it. If a paleoanthropologist is convinced that toolmaking human beings did not exist in the Middle Pliocene, he is not likely to give much thought to the exact nature of markings on fossil bones from that period.

Even for those prepared to find signs of human work, the interpretation of marks on fossil bones is a difficult matter. This led Binford (1981, p. 181) to write: "One might reasonably ask at this point that if we cannot establish a pattern of bone modification unambiguously referable to man, why study the faunal products of man and seek greater understanding of his highly variable behavior? The answer to this is simply that the basic task of anthropology—of which archaeology is a part—is to seek an understanding of man's variable cultural behavior." Binford clearly defined the dilemma inherent in the empirical approach to such questions—it is imperfect, yet there appears to be no other choice. So it seems that great caution is required. In fact, our study of the empirical methods used by paleoanthropologists suggests these methods cannot give a completely reliable picture of the past, and of human origins in particular.

Eoliths

### 3.1 Anomalously Old Stone Tools

Even when considered alone, the evidence gathered from incised and broken bones, as detailed in the preceding chapter, inflicts heavy damage on the conception that toolmaking hominids emerged only in the Pleistocene. But we now turn to a more extensive and significant category of evidence—ancient stone implements.

Nineteenth-century scientists turned up large quantities of what they presumed to be stone tools and weapons in Early Pleistocene, Pliocene, Miocene, and older strata. These were not marginal discoveries. They were reported by leading anthropologists and paleontologists in well-established journals, and were thoroughly discussed at scientific congresses. But today hardly anyone has heard of them. One wonders why. As in the case of the bones discussed in the previous chapter, the hard facts of these discoveries, though disputed, were never conclusively invalidated. Instead, reports of these ancient stone implements were, as time passed, simply put aside and forgotten as different theoretical scenarios of human evolution came into vogue.

Here is what appears to have taken place. In the 1890s, Eugene Dubois discovered and promoted the famous, yet dubious, Java ape-man (Section 7.1). Many scientists accepted Java man, found unaccompanied by stone tools, as a genuine human ancestor. But because Java man was found in Middle Pleistocene strata, the extensive evidence for toolmaking hominids in the far earlier Pliocene and Miocene periods no longer received much serious attention. How could such toolmaking hominids have appeared long before their supposed ape-man ancestors? Such a thing would be impossible; so better to ignore and forget any discoveries that fell outside the bounds of theoretical expectations.

And that is exactly what happened—whole categories of facts were interred beneath the surface layers of scientific cognition. By patient research we have, however, managed to locate and recover a vast hoard of such buried evidence, and our review of it shall take us from the hills of Kent in England to the valley of the Irrawady in Burma. We shall also give consideration to anomalously old crude stone tool industries discovered by researchers in the late twentieth century.

The anomalous stone tool industries we shall consider fall into three basic divisions: (1) eoliths, (2) crude paleoliths, and (3) advanced paleoliths and neoliths.

According to some nineteenth-century authorities, eoliths (or "dawn stones"), were stones with edges naturally suited for certain kinds of uses. These, it was said, were selected by humans and used as tools with little or no further modification. Often one or more of the natural edges of the stone would be chipped to make it more suitable for a desired function. To the untrained eye, Eolithic stone implements were often indistinguishable from ordinary broken rocks, but specialists in lithic technology developed criteria for identifying upon them signs of human modification and usage.

In the case of more sophisticated stone tools, called paleoliths, the signs of human manufacture were more obvious, involving an attempt to form the whole of the stone into a recognizable tool shape. Questions about such implements centered mainly upon the determination of their correct age. Some Paleolithic implements, such as those used in Europe during the Late Stone Age and in recent historical times by the American Indians, display a high degree of artistry and craftsmanship, with very fine and elaborate chipping and graceful, symmetrical shapes. Most of the implements we shall be examining, however, are far more rudimentary. In fact, some researchers of the nineteenth and twentieth centuries have categorized them among the eoliths. But we have chosen to make a rough distinction between eoliths and crude paleoliths.

While the eoliths are formed from naturally broken pieces of stone, perhaps with some slight chipping on a working edge, the crude Paleolithic industries include some specimens that have been deliberately flaked from stone cores and then modified by more extensive chipping into definite tool shapes. In distinguishing crude paleoliths from eoliths, we have also relied on experts who have testified that anomalously old paleoliths from the Pliocene, Miocene, and earlier periods are identical to accepted Paleolithic implements of the Late Pleistocene.

Our third division, advanced paleoliths and neoliths, refers to anomalously old stone tools that resemble the very finely chipped or smoothly polished stone industries of the standard Late Paleolithic and Neolithic periods.

Over the years, the terms eolith, paleolith, and neolith have been used in various ways. For most researchers, they have denoted not only levels of technical development but also a definite temporal sequence. Eoliths would be the oldest implements, followed in turn by the paleoliths and neoliths. But in the course of our discussion we will mainly use these terms to indicate degrees of

workmanship. The evidence, we propose, makes it impossible to assign dates to stone tools simply on the basis of their form.

In this chapter, we shall discuss anomalous eoliths. In Chapter 4, we shall discuss anomalous crude paleoliths, and in Chapter 5, we shall discuss anomalous advanced paleoliths and neoliths. This threefold division is not perfect. We were confronted with borderline cases in which assignment to one chapter or another was difficult. Within the cruder stone tool industries are often found individual implements and groups of implements that might be classified as more sophisticated; and similarly, among the more sophisticated industries are found examples of implements that might be classified among the most crude. Also, some individual researchers discovered a number of industries, of varying levels of complexity, and for the sake of convenience, these have been grouped together. Because of this, it has not been possible, or practical, to achieve a complete segregation of tool types in different chapters. Still, we have found it useful to attempt to make a rough division between (1) the Eolithic, (2) the crude Paleolithic, (3) and the advanced Paleolithic and Neolithic types.

Having expressed these cautions, we can now embark upon our examination of the Eolithic stone tools, beginning with those found by Benjamin Hamson in England and proceeding to tools found in other countries during the latter part of the nineteenth century. We shall then consider the discoveries of J. Reid Moir in England. In the last sections of this chapter, we shall examine attempts by H. Breuil and A. S. Barnes to discredit Eolithic industries, and finally we shall review modern examples of Eolithic industries.

## 3.2 B. HARRISON AND THE EOLITHS OF THE

### KENT PLATEAU, ENGLAND (PLIOCENE)

#### 3.2.1 Young Harrison

The small town of Ightham, in Kent, is situated about twenty-seven miles southeast of London. Nearby one finds the home of the unfortunate second wife of Henry VIII, Anne Boleyn, who lost her head to the executioner's blade. In the more sedate years of the Victorian era, a respectable small businessman named Benjamin Harrison kept a grocery shop in Ightham. On holidays he roamed the nearby hills and valleys, collecting flint implements which, though now long forgotten, were for decades the center of protracted controversy in the scientific community.

Even as a boy, Harrison was interested in geology and read Lyell's *Principles of Geology* at age thirteen. In the course of his walks, he grew well acquainted with the landscape around Ightham. This region of southeastern England, known as the Weald of Kent and Sussex, had a complex geological history. In the past, it was a broad rise. In later times, the central part of the rise was eroded away by the forces of nature (Figure 3.1), leaving hills to the north (the North Downs) and south (the South Downs).

## N

Figure 3.1. The Weald region of Kent and Sussex, England. The dotted line shows the ancient land surface, now eroded away, leaving the present North Downs (N) and South Downs (S) (Moir 1924, p. 638). The Kent Plateau is in the North Downs region.

The North Downs rise to the Kent Plateau near Ightham, and it is on the Kent Plateau that Harrison made some of his most significant discoveries. Young Harrison developed into an accomplished amateur paleoanthropologist. Perhaps semi-professional would be a better word than amateur, for Harrison did much of his work in close consultation with, and sometimes under the direct supervision of, Sir John Prestwich, the famous English geologist, who lived in the vicinity. Harrison also corresponded regularly with other scientists involved in paleoanthropological research and carefully catalogued and mapped his finds, according to standard procedures.

A room over Harrison's shop served as a museum where he kept his flint tools. On the walls he displayed geological maps of the Weald region of Kent and Sussex, water colors of implements he had found, and portraits of Charles Darwin, Sir John Prestwich, and Sir John Evans.

### 3.2.2 Neoliths and Paleoliths

Harrison's first finds were not of the very crude Eolithic variety. They were Neolithic implements. Neoliths are smooth-surfaced, polished stone artifacts, displaying highly sophisticated craftsmanship. According to modern opinion, Neolithic cultures date back only about 10,000 years, and are associated with agriculture and pottery. Harrison found neoliths scattered over the present land surfaces around Ightham.

In the early 1860s, the discoveries of Boucher des Perthes in France were attracting the attention of British scientists. Boucher des Perthes had found paleoliths in the gravels of the Somme River valley. These implements were older and somewhat cruder than the neoliths Harrison was collecting. Having learned of the finds of Boucher des Perthes, Harrison himself began to search for similar specimens. These Paleolithic implements, although cruder than Neolithic implements, are still easily recognized as objects of human manufacture. They are thus distinct from Eolithic implements. Modern authorities would assign European Paleolithic tools to the Middle and Late Pleistocene. Harrison looked for paleoliths in ancient deposits of gravel on river terraces, and in 1863 discovered his first paleolith in a gravel pit near Ightham (E. Harrison 1928, p. 46). In addition to searching himself, Harrison trained local workmen to recognize flint implements and collect them for him. Over the years, he amassed a substantial collection of paleoliths.

In 1878, William Davies, a geologist of the British Museum, saw some of Harrison's flint implements and agreed that some of them were paleoliths. Harrison sent a report and some specimens to Sir John Lubbock, who also stated that some of the implements were definitely Paleolithic. G. Worthington Smith, of the Royal Anthropological Institute, visited Ightham and after inspecting the flints initially agreed that some were paleoliths but then later changed his mind (E. Harrison 1928, p. 81).



In 1879, Harrison first met Sir John Prestwich, an eminent geologist, who had a country house eight miles away, at Shoreham. Harrison asked Prestwich some questions about the geological position of the discoveries of Boucher des Perthes in relation to the present level of the Somme River. From Prestwich's window, they could see the Darent River valley. Prestwich said: "If we take the Darent to be the Somme, the gravels would lie at about the level of the railway station." The author of Benjamin Harrison's biography, Sir Edward R. Harrison, wrote (1928, p. 84): "As this remark was made, it flashed through Harrison's mind that some of his own palaeoliths had been found in gravels that were higher, in relation to the level of the streams to which they belonged, than was the level of the railway station in respect to the Darent. Broadly speaking, greater relative height meant greater antiquity, and, consequently, amongst his finds were implements that might be older than those found by Boucher des Perthes in the Somme valley."

To further clarify the matter, let us suppose we have a river running on a level plain a million years ago. As it excavates a channel, it will deposit gravel on the terraces of its banks. As the river descends through the strata, it will deposit more gravels at successively lower levels. In this way, it may be seen that the oldest river gravels, about one million years old, would be found at the higher levels of the valley, while the most recent ones would be found at the lowest levels, on the banks of the present river. The ages of the different levels of gravel are therefore the reverse of the ages of a typical sequence of geological strata, in which the higher strata are the youngest and the lower strata are the oldest. It should, however, be kept in mind, that in actual practice, the assignment of ages to river terraces and gravels is rarely so simple as in this hypothetical illustration.

On September 11, 1880, Harrison made a typical discovery. Sir Edward R. Harrison (1928, p. 87) informs us: "He walked to examine a bed of gravel lying on High Field, at the head of the gorge of the Shode. In this gravel, far above the present level of the stream, he found a palaeolithic implement. His thoughts, on making this discovery, must have been somewhat as follows. The gravel was a very ancient gravel, even in a geological sense, and in it was an implement that had been made by man and carried down afterwards by a stream running at a much higher level than the present stream, to the position in which it was found. So man was older than the very old gravel. Harrison sent news of his find to Prestwich, who came at once to Ightham to see for himself the geological position in which the implement had been found." Prestwich pronounced it a very old bed and advised further research. Prestwich himself and workers under his direction made similar finds.

As word of the newly discovered stone implements spread, James Geikie, one of England's leading geologists, wrote about them on May 2, 1881 to G. Worthington Smith: "They will yet be found in such deposits and at such elevations as will cause the hairs of cautious archaeologists to rise on end. I hope other observers will take a hint from you and search for paleolithic implements in places which have hitherto been looked upon as barren of such relics" (E. Harrison 1928, p. 91).

Geikie's remarks about searching for stone tools "in places which have hitherto been looked upon as barren of such relics" help clarify why modern scientists do not often report finding evidence for

ahuman presence in very ancient times. Because of their preconceptions, they do not look for such evidence in all the places where it might be found. For example, since modern scientists do not accept a fully human presence in the Pliocene, they do not look for advanced stone tools in Pliocene deposits. And if they do find such tools in unexpectedly old deposits, they explain them away. But in the nineteenth century, it was not clear to scientists that they should not be looking for evidence of a human presence in the Pliocene and earlier. So they looked for it, and when they found it, reported it straightforwardly.

In 1887, Harrison read an article by Alfred Russell Wallace on human antiquity in America and then wrote Wallace a letter. Wallace, famous for publishing a scientific paper on evolution by natural selection before Darwin, wrote to Harrison: "I am glad you find my article on 'The Antiquity of Man in America' interesting. It is astonishing the amount of incredulity that still prevails among geologists as to any possible extension of the evidence as to greater antiquity than the paleolithic gravels. The wonderful 'Calaveras skull' has been so persistently ridiculed, from Bret Harte upwards, by persons who know nothing of the real facts, that many American geologists even seem afraid to accept it" (E. Harrison 1928, p. 130).

The Paleolithic gravels referred to by Wallace are equivalent to those of the Somme region, in which Boucher des Perthes found stone tools. These belong to the Middle Pleistocene period of the Quaternary. The Calaveras skull as well as many stone tools were found in far older Tertiary strata in California. The Tertiary includes the Pliocene, Miocene, Oligocene, Eocene, and Paleocene periods. We shall discuss the Calaveras skull and several related discoveries later in this book (Sections 6.2.6, 5.5). The tactic of persistent ridicule mentioned by Wallace was, however, so effective that a good many modern students of paleoanthropology have never even heard of the California finds.

Prestwich and Harrison considered some of the stone implements found near Ightham to be Tertiary in age. The geological reasons for this opinion were discussed by Prestwich in a paper presented to the Geological Society of London in 1889. In preparation for his report, Prestwich asked Harrison to catalog and map his finds. Harrison did so, with the following results: 22 flint implements had been found at elevations over 500 feet, 199 at elevations between 400 and 500 feet, and 184 at elevations under 400 feet, amounting to a total of 405 implements found since 1880 (E. Harrison 1928, p. 129).

In his presentation to the Geological Society, with Harrison sitting in the audience, Prestwich first demonstrated that the higher formations of gravel around Ightham could not have been deposited by the present streams, at any point in their history. He gave evidence showing that the Shode could not have flowed any higher than the 340-foot level (Prestwich 1889, p. 273). Thus the tools in the gravels at elevations over 400 feet must have been quite old, having been deposited by ancient rivers.

This analysis is confirmed by modern authorities. Francis H. Edmunds, in a study published by the Geological Survey of Great Britain, wrote (1954, p. 59): "Occasional patches of gravel, unassociated with any present river system, have been recorded at various localities in the Wealden District. . . .

they cap hilly ground and occur usually about 300 ft. above sea level. They consist of a few feet of roughly-bedded flint or chert gravel in a clayey matrix.'

Prestwich, having discussed the geological history of the high-level gravels, which he called hill drifts, then dealt with an important question regarding the implements found in them. Could these implements, perhaps of recent origin, have been dropped into the very old hill drift gravels in an age not long past? Prestwich believed that this was true of some of the implements, the Neolithic ones. But along with the Neolithic tools, dropped in the ancient hill drift gravels within the last few thousand years, there were, according to Prestwich, far older Paleolithic tools. These could be distinguished from the Neolithic tools by their deeply stained surfaces and the wear on their edges. Prestwich (1889, p. 283) stated that the paleoliths "exhibit generally the deep uniform staining of brown, yellow, or white, together with the bright patina, resulting from long imbedment in drift-deposits of different characters." In addition, he said that some of the paleoliths were "more or less rolled and worn at the edges by drift-action—some very much so" (Prestwich 1889, p. 283). The neoliths were relatively unstained and unworn.

Sir John Prestwich (1889, p. 286) went on to say about the paleoliths found by Harrison near Ightham: "It is clear from the condition of the implements that, although now occurring on the surface of the ground, they, unlike the neolithic flints, which are unstained and unaltered except by atmospheric agencies, have been imbedded in some matrix which has produced an external change of structure and colour; while the matrix itself, which has been removed by denudation, has nevertheless in several instances left traces on the implements sufficient to indicate its nature."

Describing the remnants of one kind of matrix, Prestwich (1889, p. 289) stated: "a considerable portion of these paleolithic implements are studded on one side with small dark-brown concretionary incrustations of iron peroxide and sand. . . . From this we may infer that both the flint implements and the flints have at one time been imbedded in a sandy, ferruginous matrix, just as the film of calcite on the under side of some of the St. Acheul specimens shows them to come from one of the seams of calcareous sand or chalky gravel common in the drift there, or as the ferruginous concretions on the Dunks Green specimens indicate their origin in that drift."

The identity of the matrix is hinted at by Edmunds (1954, p. 47): "At intervals along the higher parts of the North Downs, and near the crest of the Chalk escarpment, patches of rusty brown sand are present." The hill drifts of the North Downs and the plateau drifts of the Chalk Escarpment are the locations where Harrison found most of his implements. Edmunds (1954, p. 47) further noted: "similar blocks of fossiliferous ironstone or ferruginous sandstone occur on the South Downs near Beachy Head. The fossils have been proved to be of Pliocene age."

"Unfortunately," stated Edmunds (1954, p. 47), "no fossils have been found in the sand resting on the top of the Downs, but their general resemblance to the fossiliferous sandstones . . . leads to the conclusion that they are the remains of an extensive sheet of sands laid down during a marine transgression which is thought to have taken place subsequent to the Miocene.' Ferruginous sandstone like that of the South Downs also occurs in the Lenham Beds of the Weald region. Some

modern authors (Klein 1973, table 6) date the Lenham Beds to the Early Pliocene or Late Miocene. According to Edmunds, the sandy deposits on the North Downs, the Lenham Beds, and the ferruginous sandstone of the South Downs would all three be of the same Pliocene age.

Granting Edmunds's explanation of the history and age of the iron-stained sands found on the North Downs and Chalk Escarpment, we can consider two hypothetical accounts about how stone implements might have come to be present in them.

The first account involves a Miocene origin for the implements. In the Late Miocene, toolmakers might have left implements on a land surface in the Weald region of southern England, which was later submerged by rising sea levels in the Early Pliocene. The implements were then embedded in marine deposits. Later in the Pliocene, the region again became a land surface, the central portion of which was uplifted (Figure 3.1). Rivers flowing down from the central uplands, in a northerly direction, eroded the ferruginous marine sands. The flint implements and ferruginous sands were deposited in the places where they are now found—as hilltop drifts at very high elevations on the North Downs and as plateau drifts on the Chalk Escarpment (Figure 3.2). During the Pleistocene glacial periods that followed, a different river system carved out valleys and deposited valley drift gravels on terraces below the North Downs hilltops and the Chalk Plateau, with their deposits of sands and gravels from the Pliocene.

Our second account involves a Pliocene origin for the tools. As above, a marine transgression took place in the Early Pliocene, depositing layers of sediment. Later in the Pliocene, the region again became a land surface, drained by rivers.

Figure 3.2. The relationships of gravel deposits (drifts) to generalized Weald landscape.

- (1) Plateau drift deposited by rivers flow'ng north over the Early Pliocene land surface.
- (2) Hilltop drifts deposited by a now vanished Late Pliocene river.
- (3-5) Progressively younger valley drifts deposited by the present river in the Middle and Late Pleistocene.

People living along the banks of these rivers left stone tools, which were transported by the river to their present locations on the North Downs hilltops and the Chalk Plateau. This took place

before the present river systems came into being. Embedded in the gravel deposits for long periods of time, the flint implements acquired their coloration and patina. These Implements, their edges worn by transport, could not be any younger than the now-vanished northwardflowing rivers.

Any Implements more recently dropped into these gravels would have remained unrolled and unworn because no water was flowing at that high level. The new rivers were flowing at much lower levels.

How old were the Paleolithic flint Implements on the Kent Plateau and in the hilltop drifts? Prestwich (1889, p. 292) concluded: "physiographical changes and the great height of the old chalk plateau, with its 'red clay with flints' and 'southern drift' high above the valleys containing the Postglacial deposits, point to the great antiquity—possibly Preglacial—of the palaeolithic Implements found in association with these summit drifts."

According to current opinion, glaciers approached, but did not actually cover the Kent Plateau. The Cromer Till of East Anglia, north of the Kent Plateau, represents the earliest definite geological evidence of glaciation in southern England (Nilsson 1983, pp. 112, 308). A till is a deposit of stones left by retreating glaciers. The Cromer till is .4 million years old. But evidence of an arctic climate occurs somewhat earlier than the Cromer Till, in the Beestonian cold stage at around .6 million years ago (Nilsson 1983, pp. 108, 308).

So strictly speaking, the preglacial period in southern England might be said to begin in the Middle Pleistocene. Interpreted in this light, Prestwich's statement that the Implements found in the summit drifts were preglacial could thus mean they were as recent as the early Middle Pleistocene. But, as we have seen, Edmunds (1954, p. 47) has proposed that the summit drifts, the ferruginous sands, are in fact Pliocene in age.

Hugo Obermaier (1924, p. 8), a leading paleoanthropologist of the early twentieth century, stated that the flint Implements collected by Harrison from the Kent Plateau "belong to the Middle Pliocene." J. Reid Moir, a fellow of the Royal Anthropological Institute, also referred Harrison's discoveries to the Tertiary (Section 3.3.1).

A Late or Middle Pliocene date for the Implements of the Kent Plateau would give them an age of 2-4 million years. Modern paleoanthropologists attribute the Paleolithic Implements of the Somme region of France to *Homo erectus*, and date them at just .5-.7 million years ago. The oldest currently recognized Implements in England are about .4 million years old (Nilsson 1983, p. 111). So the Paleolithic Implements of the Kent Plateau pose a number of difficulties for modern paleoanthropology.

### 3.2.3 Eoliths

Among the Paleolithic Implements collected by Benjamin Harrison from the Kent Plateau were some that appeared to belong to an even more primitive level of culture. These were the eoliths, or dawn stones (Figure 3.3). This name eventually came to be used for a wide variety of very crude stone tool industries from England and other countries.

Figure 3.3. An eolith from the Kent Plateau (Moir 1924, p. 639).

The Paleolithic implements discovered by Harrison, although somewhat crude in appearance, had been extensively worked in order to bring them into definite tool and weapon shapes (Figure 3.4). The Eolithic implements, however, were, as defined by Harrison, natural flint flakes displaying only retouching along the edges.

Figure 3.4. These implements from the Kent Chalk Plateau were characterized as paleoliths by Sir John Prestwich (1889, plate 11). Prestwich (1889, p. 294) called the one on the left, from Bower Lane, "a roughly made implement of the spear-head type."

Such tools are still used today by primitive tribal people in various parts of the world, who pick up a stone flake, chip one of the edges, and then use it for a scraper or cutter.

The question then arises as to how such eoliths could be distinguished from broken pieces of flint unmodified by human action. There were, of course, difficulties in making such distinctions, but even modern experts accept lithic assemblages resembling the eoliths collected by Harrison as genuine human artifacts. We shall consider this subject in greater detail in the course of this chapter, but for now we shall mention as an example the crude cobble and flake tools of the lower levels of Olduvai Gorge (Figure 3.5),

Figure 3.5. Top: Stone implements from Olduvai Gorge (M. Leakey 1971, pp. 45, 113). Bottom: Implements found by Benjamin Harrison on the Kent Plateau, England (Moir 1924, p. 639; E. Harrison 1928, p.

342).

The Olduvai Gorge implements are extremely crude, but to our knowledge, no paleoanthropologists have ever challenged their status as intentionally manufactured objects.

Harrison believed that the Kent eoliths belonged to an older period than that represented by his paleoliths. But in his 1889 report, Sir John Prestwich did not make a distinction between the two forms. Of the eoliths, Sir Edward R. Harrison stated: "Prestwich In his paper made no attempt to claim for them a higher antiquity than that of the Plateau paleoliths, with which they seemed to be associated" (E. Harrison 1928, p. 145). As we have seen, the nature of the drift gravels on the Kent Plateau and the hilltops of the North Downs suggested a Late Pliocene age for the Implements.

In the aftermath of Prestwich's presentation, Harrison found himself somewhat of a celebrity. His name appeared in newspapers, and scientists from all parts of the world began to make the pilgrimage to his museum above his grocery shop in Ightham. In June of 1889, the members of the Geological Society of London visited Ightham for a tour of the sites from which the stone Implements had been recovered.

Even the considerable authority of Prestwich was, however, not enough to end all controversy regarding Harrison's discoveries, particularly the eoliths. Many scientists still saw in the eoliths nothing but the result of purely natural, rather than artificial forces. Nevertheless, Harrison was gradually winning converts. On September 18, 1889, A. M. Bell, a Fellow of the Geological Society, wrote to Harrison: "I am glad that you saw the veteran Professor [Prestwich], and that his verdict on these unbulbed scrapers coincides with our own. I have looked again and again at the edges of those which I selected, and with an increasing feeling that there is a human purpose dimly visible in the working. There seems to be something more in the uniform though rude chipping than mere accidental attrition would have produced. I have come to this conclusion with diffidence: first, because I had hitherto regarded the bulb or trace of artificial blow as a *sine qua non*: second, and more important, because I feel and have all along felt that the real enemy to such a story as ours is the too enthusiastic friend who sees what is not there; but having made my conclusion, I hold it with al finness. Until I see flints carefully and uniformly chipped all round their edges, and only in one direction of blow, by natural action, I shall believe that these are artificial" (E. Harrison 1928, p. 151).

A modern expert in lithic technology, Leland W. Patterson, also believes it is possible to distinguish even very crude intentional work from natural action. Considering "a typical example of a flake that has damage to its edge as a result of natural causes in a seasonally active stream bed," Patterson (1983, p. 303) stated: "Fractures occur randomly in a bifacial manner. The facets are short, uneven, and steeply transverse across the flake edge. It would be difficult to visualize how random applications of force could create uniform, unidirectional retouch along a significant length of a flake edge. Fortuitous, unifacial damage to an edge generally has no uniform pattern of retouch." Unifacial tools, those with regular chipping confined to one side of a surface, formed a large part of the Eolithic assemblages gathered by Harrison and others.

Prestwich, however, was at first very cautious about the eoliths, feeling more comfortable with the more readily identifiable paleoliths. But gradually he began to change his mind. On September 10, 1890, Harrison and Prestwich were searching the West Yoke ochreous gravels, which were stained red (ocher) by iron compounds. Harrison wrote: "Professor Prestwich was impressed by the great spread of worn gravel, and remarked that it was a 'capital exhibition of ochreous drift in an important position.' At his request I filled my satchel with the water-worn flints, which were scattered over the field in abundance. It was the dawn of the era of the eoliths, for on this day he pressed me to take home specimens that only a few months earlier he would have regarded as too doubtful to be preserved" (E. Harrison 1928, pp. 155-156).

In 1891, Prestwich presented at the Geological Society of London another paper, titled "On the Age, Formation, and Successive Drift-Stages of the Valley of the Darent; with Remarks on the Palaeolithic Implements of the District and on the Origin of its Chalk Escarpment." In this paper, Prestwich (1891, p. 163) described a paleolith found by Harrison in a hole dug for the planting of a tree: "I have now seen the fine specimen. . . . It is 6 inches long by 3:1/2", in. wide, very flat and round pointed, and shows no wear. It more resembles one of the large St. Acheul types. It was found on the top of the soil last thrown out of the hole." It is not clear what kind of sediments the tool was found in, but the manner in which Prestwich related the find suggests that he regarded it as a demonstration that the paleoliths were to be found not only on the surface, but in situ.

In addition to the paleoliths, Prestwich mentioned some of the cruder Eolithic implements. This brought some inquiries from William Topley, a fellow of the Geological Society and the author of a Geological Survey memoir on the Weald region. Harrison wrote in his diaries: "Mr. William Topley at the reading of the Darent paper said that he wished to know if there was any clear case of the flints being found in place. He added that the antiquity of the gravels in such an elevation [on the Plateau] was beyond question and certainly preceded the excavation of the great Chalk valleys and the present features of the Weald. In consequence of these remarks I went to the Vigo inn, and searched in and near the post holes dug for a fence. I found worked stones and thus recorded my first finds in situ" (E. Harrison 1928, p. 161). Thus the eoliths as well as paleoliths were to be found within the earth, and not just on the surface.

Harrison also noted that in most cases his eoliths occurred in places where there were no paleoliths. To him, this indicated a different age for the two types of implements.



A. R. Wallace, who was greatly interested in Harrison's finds, asked him for a copy of Prestwich's Darent paper. Harrison forwarded the paper to Wallace, who later replied: "I read Mr. Prestwich's paper with great interest, especially with regard to the rude type of implements, which I had never seen represented before. They are certainly very distinct from the well-finished palaeolithic weapons, and their having a separate area of distribution is strong proof of their belonging to a different and earlier period" (E. Harrison 1928, p. 370).

### 3.2 .4 More on the Geology of the Kent Plateau

In 1891, Sir John Prestwich presented a third major paper on the stone implements of the Kent Plateau. In this paper, delivered to the Royal Anthropological Institute, Prestwich pointed out that the Chalk Plateau of Kent, where Harrison found paleoliths and eoliths, is bounded by a large valley running across its southern border. According to Prestwich, this valley was scooped out by water action during the glacial period. The Kent Plateau, however, contained drift gravels like those present on the South Downs, the hills that still exist on the other side of the southern valley. Prestwich (1892, p. 250) stated: "as the flint implements are closely associated with this plateau drift, and are limited to the area over which it extends, we are led to infer the pre-glacial or early glacial age of the men by whom they were fabricated." Just to clarify the reasoning, let us imagine ourselves in the Late Pliocene, looking south from the present North Downs and Kent Plateau. Instead of the valley now there, we would see the rising surface of the Weald dome (Figure 3.1, p. 88). At this time, according to Prestwich, the now-vanished dome uplands would have been inhabited by humans who made crude stone tools. Rivers and streams running down from the uplands flow north, depositing their gravels and sediments, along with stone tools, on the surface of the region now occupied by the North Downs and Kent Plateau. The rivers also flow south from the divide of the central dome uplands, to the South Downs.

This process continues until the Pleistocene, a time of increased precipitation. Torrents of water flowing along an east-west axis, carve out a large valley where the Weald uplands once rose. Now the landscape is considerably changed, leaving the Kent Plateau and hills in the north separated by a deep, wide valley from hills to the south. At this point, the rivers no longer flow onto the plateau, but rather empty into the valley. But the old gravels and sediments, containing eoliths, remain on the Kent Plateau surface. They could only have been deposited there before the excavation of the valley. The proof of the accuracy of this scenario: the gravels and sediments found today on the Kent Plateau surface greatly resemble those found on the South Downs, now separated from the Kent Plateau by the great transverse valley. As we have seen, Edmunds (1954, p. 47) has identified the ferruginous deposits topping the North Downs with those now found in the South Downs. Since certain kinds of tools were found only in the ferruginous gravels and other such deposits on the North Downs and Kent Plateau. Prestwich concluded that these tools were made by the humans who lived on the central dome uplands. before the glacial period.

Modern authorities relate the geological history of the rivers of the Weald region and their gravel deposits in much the same way as outlined above. For example, Francis H. Edmunds, in a study published by the Geological Survey of Great Britain, wrote (1954, p. 69): "The original rivers of the

Wealden district. . . flowed either northward or southward from an east-to- west watershed along the main axial line of the Weald." These rivers left north-south gaps in the Weald landscape. some of which are not used by the present river systems. Edmunds (1954, p. 63) stated: 'Certain physical features, notably the position of the river gaps through the North and South Downs. connect modern topography with that of the pre-Pliocene epoch." A map by Edmunds (1954, p. 71) shows the Plateau gravels as having been deposited by the rivers flowing from south to north. This tends to confirm the views of Prestwich, who believed the Plateau gravels were laid down by rivers flowing north from the central dome uplands during the Pliocene and perhaps the preglacial Pleistocene.

Concerning the Plateau deposits (Clay-with-flints), Edmunds thought some were produced locally by dissolution of the underlying chalk formations, which contain flint. But Edmunds (1954, p. 56) added: 'The Clay-with-flints in several Wealden localities, however, contains a major proportion of material which could not have been so derived. but which represents a Tertiary beds. of Eocene and Pliocene ages."

This suggests that the worn and patinated eoliths (and paleoliths) found in the Plateau deposits could very well be of Tertiary age.

Maps supplied by Edmunds (1954, p. 71) show that the north-south river systems. which laid down the Tertiary Plateau gravels and the hill drifts, were later diverted into their present east-west channels. These east-west rivers deposited the Pleistocene gravels on terraces below the hill drifts, the higher terraces being the oldest (Figure 3.2. p. 93). This process of gravel deposition began during the glacial period.

The stone implements found in the higher terrace gravels of the present rivers were, according to Prestwich, similar to the Paleolithic implements encountered in the Somme region of France, where Boucher des Perthes conducted his investigations. In his address to the Anthropological Institute. Prestwich explained that in the Kent Plateau region Neolithic Implements were mainly found in the lower, more recent. river beds along with fossil remains of mammoth, woolly rhinoceros, reindeer, and other late Age mammals.

To summarize, the eoliths were found mainly in the Pliocene drift gravels on the top of the Plateau. crude paleoliths mainly in the hilltop drifts of Pliocene rivers, better paleoliths mainly in the Pleistocene higher gravels of the present rivers, and polished neoliths in the lower more recent river gravels.

Most of the high Plateau discoveries were surface finds. But Prestwich (1892. p. 251) noted that 'from the deep staining of the Implements, and their occasional incrustations with iron oxide, we have reason to believe that they have been imbedded in a deposit beneath the surface." This is significant. If the implements were embedded beneath the surface of the now- vanished dome uplands for a long time before they were transported to the Plateau, that would indicate an indefinitely great age for them. In other words, they were at least Late Pliocene in age, and perhaps far older.

Some of the Plateau implements were found not on the surface but in situ deep within the preglacial Plateau drift gravels. This would tend to rule out the supposition that the implements were of fairly recent origin and had been dropped on the drift gravels by the later inhabitants of the Plateau region. Prestwich (1892, p. 251) stated: "A fine specimen was found at South Ash in making a hole two feet deep for planting a tree, but as it was picked up on the thrownout soil, its exact position beneath the surface remains of course uncertain. It was the same with the one obtained in a post-hole at Kingsdown. For two others we have, however, the personal testimony of Mr. Harrison. One he took out of a bank of the red-clay-with-flints on the side of a pond and at the depth of two and a half feet, and the other from a bed of 'deep red clay,' two feet in depth, at the Vigo."

In a footnote to the above passage, Prestwich (1892, p. 251) went on to say: "Mr. Bullen has just had a trench dug on the top of Preston Hill. It was nearly five feet deep, through surface soil (one foot); and the red-clay-with-flints in which, at a depth of three feet ten inches from the surface, he found an unworn white flint—apparently the broken point of a small implement." As we have seen, Edmunds (1954, p. 56) characterized major portions of the clay-with-flints deposits as reworked beds of Tertiary age, some Pliocene, some even Eocene.

### 3.2.5 The Relative Antiquity of Eoliths and Paleoliths

Returning to the eoliths found on the surface of the Plateau, Prestwich (1892, p. 252) asked: "could these implements, like the neolithic implements which occur on the same ground, have been dropped on the surface where they are now found, at some later date?" Although most of the Neolithic implements were found in the lower river terraces, some did occur on the Plateau. Prestwich (1892, p. 252) went on to state, in response to his own question: "The answer to this is, that these neolithic implements show only weathering by exposure on the surface, and are found at all levels, whereas the plateau implements, besides their wear and colour, present all the physical characteristics due to having been imbedded in a special drift, and are confined to a special area. The two sorts, although found on the same ground, remain perfectly distinguishable."

Prestwich (1892, p. 252) then gave an extensive answer to an objection raised by Sir John Evans: "Then again, is it not possible that similar rude specimens occur in the valley drifts, and have been overlooked owing to the prevalence of the better finished implements to which attention had been exclusively given." If eoliths were found in connection with the paleoliths or neoliths in the valleys, that might weaken Prestwich's argument for their great age, which was based on the fact that they tended to be found only in the very ancient Plateau drifts. Prestwich (1892, p. 252) answered as follows: "A large number of rude and badly finished specimens have been collected in the valley drifts, but they all belong to one set of types, and though I have seen and handled many hundreds of these, I question whether, with the exception of the derived specimens [those washed down from the Plateau] to be named presently, there were any like the ruder and most primitive of the plateau types. The distinction is as well marked as that between the ruder specimens of Roman pottery and rude early British pottery." Prestwich (1892, p. 252) went on to

state: "Boucher de Perthes collected everything in the Somme district, which showed any traces of workmanship, however indistinct, or even of similitude, yet I do not remember that in his great collection there were specimens of the peculiar character of these plateau implements." In other words, the evidence from the Somme region confirmed Prestwich's hypothesis that the Kent Plateau eoliths were of a distinct type, different from superficially similar crude implements of later periods. In a footnote, Prestwich (1892, p. 252) added: "I do have one specimen given me by M. Boucher de Perthes, from near St. Riquier, five miles north-east of Abbeville, which may belong to this group. It is said to have been found at a depth of four metres [about 13 feet], and evidently comes from the red clay drift, which there caps, as it does here, the higher chalk hills. It is four inches long by one and half inches wide, rod-shaped, very roughly chipped all around and at ends, and has a white patina, to which some of the red clay as yet adheres." This discovery would appear to be well worth looking into, and is representative of the intriguing items one comes across in old Journal articles. It might represent a stone implement far older than the others discovered by Boucher de Perthes in the river gravels of the Somme valley at Abbeville, now dated to the Middle Pleistocene, about a half million years old.

After giving testimony about not finding specimens like the Plateau eoliths in Boucher de Perthes's collection, Prestwich (1892, pp. 252-253) stated: "Nor had Mr. Harrison, during his rigorous examination of the Shode Valley, discovered any specimens in the valley drifts of the Ightham district to correspond with the group of plateau implements. At my request, he has re-examined several of these localities, as well as the large pit at Aylesford in the Medway Valley, and the pits at Milton Street (Swanscombe) in the Thames Valley, with this special object in view. He reports to me that he finds no contemporary specimens of the plateau type, and very few derived specimens of that type."

Prestwich (1892, p. 268) then cited evidence from De Ban! Crawshay, who stated: "I find that on examination of my collection of over 200 specimens of implements and scrapers from the 100 foot level around Swanscombe, Kent, I have but one . . . which is a plateau specimen undoubtedly derived. . . . I have always made specially careful search for all these ochreous flints in the low level gravels, and have rarely found one at all."

Derived specimens are those washed down from the Plateau and left in the lower level gravels. Prestwich (1892, p. 253) stated: "The derived plateau specimens are easily distinguished, by their greater wear, distinct colour, and peculiar shapes, from the implements contemporary with these valley drifts."

The valley Paleolithic specimens were very extensively worked, with fine, regular chipping, and generally took the form of points meant, perhaps, to be used as spear heads. There were some crude, unfinished specimens among them, but they were obviously of the same type as the finished paleoliths, and not of the Plateau type (Prestwich 1892, p. 255).

About the Plateau eoliths, Prestwich (1892, p. 256) stated: "The trimming slight though it maybe, is to be recognised by its being at angles or in places incompatible with river drift agencies, and such as could not have been produced by natural causes." Prestwich admitted that some specimens

resembling the more advanced valley paleoliths were found along with the Plateau eoliths, and stated (1892, p. 257): "It is not easy to account for the presence of these abnormal specimens. If contemporaneous with the others, we might assume that there were then some workmen more skilled than their neighbors in the fabrication of flint implements." Working against this hypothesis, according to Prestwich, was the fact that the rude Eolithic specimens were heavily patinated and were very worn, whereas the finished Paleolithic specimens were unpatinated and had perfectly sharp edges. Prestwich surmised the latter might have been left on the Plateau by Paleolithic men in more recent times, long after the eoliths had been deposited. Prestwich (1892, p. 258) then made a very important observation: "Though the work on the plateau implements is often so slight as scarcely to be recognisable, even modern savage work, such as exhibited for example by the stone implements of the Australian natives, show, when divested of their mounting, an amount of work no greater or more distinct, than do these early palaeolithic specimens." This implies that it is not necessary to attribute the Plateau eoliths to a primitive race of ape-men. Since the eoliths are practically identical to stone tools made by *Homo sapiens sapiens*, there is no reason to rule out, a priori, the possibility that the eoliths (and the paleoliths) may have been made by humans of the fully modern type in England during the Late Pliocene. As we shall demonstrate later on (Section 6.2), scientists of the nineteenth century made several discoveries of skeletal remains of anatomically modern human beings in strata of Pliocene age.

In the discussion that followed Prestwich's presentation of his report, Sir John Evans repeated his point that the presence in the Plateau drift gravels of paleoliths made it possible that eoliths were contemporary with them and thus more recent than Prestwich and Harrison believed (Prestwich 1892, p. 271). Years later Harrison wrote in a letter, dated June 3, 1908, to W. M. Newton: "At the meeting of the Anthropological Institute in 1891, Dr. Evans closed his observations with the following sentence, 'Before we accept these' [the Eolithic implements] — I looking at Prestwich — 'we must think twice,' — looking at me — 'we must think thrice, and' — looking round the whole meeting — 'we must think again'" (E. Harrison 1928, p. 165).

Other members of the Anthropological Institute also commented. General Pitt-Rivers maintained that stones resembling the eoliths were to be found in all gravels, insinuating that eoliths were simply a product of purely natural forces (Prestwich 1892, p. 272). In support of Prestwich, J. Allen Brown reported that some flints from the upper terraces of the Thames River resembled the Ightham ones, and might be of the same age and origin (Prestwich 1892, p. 275). The Journal of the Anthropological Institute recorded a summary of Prestwich's concluding remarks: "In reply, Professor Prestwich said that he had looked forward to the possibility of there being some substantial objections to his views which might have escaped him. He had, however, heard nothing but an amplified repetition of the very same difficulties which had occurred to him, and had been discussed and explained in the paper" (Prestwich 1892, p. 275).

Careful study of the report bears out Prestwich's statement. With regard to the doubts of General Pitt-Rivers, Prestwich had already demonstrated that the chipping on the eoliths was quite different from that produced by purely natural forces on river gravels. He had also offered explanations for the presence of both paleoliths and eoliths in the Plateau gravels, explaining that

some of the paleoliths, which were sharp and unworn, had probably been introduced into the Plateau gravels at a much later period than the deeply stained and much worn eoliths.

Sir Edward R. Harrison (1928, p. 166) gave a summary of the three papers presented by Prestwich: "The first paper opened up the subject of Harrison's discoveries by describing the palaeolithic implements found around Ightham in the post-glacial valley gravels, in the glacial high-level gravels, and in the very ancient, pre-glacial gravels of the high Chalk Plateau. . . The second paper, on the drift stages of the Darent valley, added to the evidence contained in the Ightham paper. The third paper was directed to the character of the rude implements, the nature of the chipping upon their edges, the classification of the specimens in groups representing different kinds of tools, and the other reasons that existed for attributing them to the hand of man." In light of Prestwich's testimony, it is remarkable that most modern studies of stone implements generally do not mention Harrison's eoliths, and those few that do give only brief, highly critical, and often sarcastic notices of dismissal.

### 3.2.6 A. R. Wallace Visits Harrison

On November 2, 1891, Alfred Russell Wallace, who was at that time one of the world's most famous scientists, paid an unannounced visit to Benjamin Harrison at his grocery shop in Ightham. Harrison recorded the incident in his notebooks: "Dr. A. R. Wallace, accompanied by Mr. Swinton of Sevenoaks, dropped in unexpectedly at 10.30. I had previously purchased Dr. Wallace's *Travels on the Amazon*, and from his portrait, which forms the frontispiece to this work, I recognized him before he entered my shop. I therefore greeted him with 'Dr. Wallace, I presume,' a recognition which puzzled him until I explained that I had many times studied his portrait. This evidently pleased him. A long and patient examination was made of the old types of implement and of some later paleoliths" (E. Harrison 1928, p. 169). Harrison then took Wallace on a walking tour of the sites where the implements had been found.

Harrison also noted: "When I was showing him my rude implements and placing them in groups, he asked, 'Was it not a pleasure to you to find such agreement in form and work when first you became certain of them?' I answered that it was a supreme time. . . . Our conversation turned to the subject of the new and startling find of implements in the auriferous gravels of North America, startling in the fact that although their positions indicated a high antiquity, yet their forms were similar to those of implements in use by the Indians at the time of the discovery of the continent in the fifteenth century" (E. Harrison 1928, pp. 169-170). The stone implements from the auriferous, or gold bearing, gravels were of Neolithic type (Section 5.5). As we shall show, they provided evidence for the presence of humans of the modern type in the very early Pliocene, or perhaps even as far back as the Eocene.

The day following his visit to Ightham, Wallace wrote in a letter to Harrison: "I was very greatly interested in your collection of the oldest paleoliths. Could you not write a popular article giving an account of your discovery of them, with all the main features of their form and peculiarities, and the special areas in which they are found, illustrated by outline sketches of all the chief types of form, and laying particular stress on the fact that each of these types, however made, is illustrated

by numbers of specimens showing how natural flint pebbles of suitable form have been selected, and by being chipped on one side only, have been brought to the required shape and edge? If you could write as you speak, I think such a paper would be published by one of the good reviews" (E. Harrison 1928, p. 171). Harrison did not write such an article immediately, but, according to Sir Edward Harrison, in 1904 he published a pamphlet along the lines suggested by Wallace.

On March 14, 1892, the noted Scottish geologist Sir Archibald Geikie wrote to Benjamin Harrison about the paper presented by Prestwich at the Anthropological Institute: "I was delighted to receive a copy of Mr. Prestwich's paper [on eoliths] a few days ago, and to read his account of your very successful investigations. It is a strange tale which these implements tell, and you may be congratulated on the successful result of your long and laborious, but, no doubt, very interesting quest. Yes, paleolithic man is old. . . . I am at present preparing a work the object of which is to show the results of glacial and archaeological researches into the antiquity of man which have been obtained up to the present time.

The more one investigates the question, the further into the past does paleolithic man seem to recede" (E. Harrison 1928, p. 175).

### 3.2.7 More Objections

Worthington G. Smith, repeating a common objection, wrote to Harrison on March 26, 1892: 'It appears to me that the importance of your discovery of implements rests on your lighting on genuine undoubted examples on the high levels. I don't attach much importance myself to the dubious and disputed forms [the eoliths], because such forms occur with genuine implements in all paleolithic gravels. The very rudest forms can never mean anything, unless such forms are exclusive, and pertain only to certain deposits" (E. Harrison 1928, p. 175). Here Smith appears to have ignored all the evidence amassed by Prestwich for the greater antiquity of the Plateau eoliths, even when found in association with more advanced Paleolithic types. Among other things, Prestwich repeatedly emphasized that the eoliths, and some of the paleoliths, are very much worn and patinated whereas other paleoliths and neoliths retain the original color of the flint and have sharp edges.

All that aside, however, it appears that Harrison did find locations in which the eoliths occur by themselves. Sir Edward R Harrison (1928, p. 176) has stated of the eoliths: "Harrison was influenced principally by their rude character, and he thought it likely that they were, for that reason, the tools of a race older than paleolithic man. Subsequently, when excavations had been made in the drifts, he found confirmation of his views in the fact that whilst certain drifts produced occasional paleoliths in apparent association with rude implements, there was also on Parsonage Farm and elsewhere, an older drift or 'buried channel' which, in his experience, contained rude implements alone."

Of course, the fact that the eoliths are sometimes found by themselves had already been reported by Prestwich. All this reveals much about scientific discussion concerning anomalous evidence. Scientists whose preconceptions dispose them to reject certain evidence often tend to repeat

their objections even after they have been met with apparently adequate responses, as if the response had never been made. Doctrinaire scientists also set conditions they believe should be met, even when such conditions have already been met. All of this makes for an Alice-in-Wonderland type of discourse: "My dear sir, I have found crudely chipped stone tools alone." "Well sir, I really think you should find these chipped stone tools alone." "But I have sir." "Then you very well should do so, or I shall never believe you." Or: 'Dear sir, let me demonstrate how this set of stone tools is older than this other set.' "Very well, but I really think you should now demonstrate that this set of tools is older than the other set." "But I already have." "Yes, but you should do it, and until you do so, I shall never believe you."

Sir John B. Evans provides a good example of this kind of interchange. Evans wrote to Harrison on October 29, 1892: "A certain number of flints, such, for instance, as several from Ash, are to my mind undoubtedly fashioned by man; there are others which probably have been worked, and others again which possibly have had their edges retouched. The great majority, however, seem to me to have assumed their present forms by natural agency. . . . When the more perfect implements are found with these ruder forms, there is no reason for regarding them as otherwise than contemporary. . . . Everyone will accept the ordinary forms of paleolithic implements as having been found at the high levels, and I am doubtful as to the desirability of complicating the question with a second race of men and a set of implements of extremely questionable character" (E. Harrison

1928, p. 184). Here Evans admitted that some of the rude implements display signs of human work. If he admitted that some, however few, were the result of human work, this conclusion was not nullified by the fact that the 'great majority' appeared to have been the result of natural action. As for the relative ages of the eoliths and the paleoliths, he appears to have either missed or deliberately ignored all the evidence suggesting that the Eolithic implements could have been more ancient.

A troubled Harrison wrote to Prestwich, who replied on November 15,

1892: "No explanation necessary. Your collection stands on its merits. Differences of opinion there will always be. All you have to say is that Sir John Evans accepts some specimens and rejects others. Let everyone Judge for himself" (E. Harrison 1928, p. 185).

Despite the continuing controversy, the British Museum still thought enough of the eoliths to purchase, in 1893, a set of representative specimens (E. Harrison

1928, p. 186). Harrison, meanwhile, continued his investigations, with the special intention of proving that the eoliths occurred not in all gravels, as some critics asserted, but only in special locations, in the very old Pliocene drift. In many gravel deposits around Ightham, Harrison noted the complete absence of any stones resembling his Eolithic implements. For example, Harrison's notebook entry for September 3, 1893 read: 'To Fane Hill—a long search, but not a single specimen of old old work.' Sir Edward R. Harrison (1928, p. 188) stated: 'This negative evidence confirmed Harrison in his opinion that the eoliths had been artificially chipped. Had they been



merely the work of natural forces it was to be expected that they would be found in large numbers in all flint-bearing gravels alike."

For years, Harrison's eoliths continued to be a topic of serious discussion in scientific societies, including the British Association for the Advancement of Science. Sir Edward R Harrison (1928, p. 192) wrote: "A. M. Bell championed the cause of the rude implements at the meeting of the British Association at Edinburgh in 1892. It fell to Professor T. Rupert Jones to undertake a like service in 1894, when the meeting was held at Oxford." The 1894 meeting was, according to A. M. Bell, who wrote to Harrison on August 10, 1894, "not a triumph . . . not a defeat, but leaves things much as they were" (E. Harrison 1928, p. 193).

### 3.2.8 The British Association Sponsors Excavations

In order to resolve the controversy over the age of the eoliths, the British Association, a prestigious scientific society, financed excavations in the high-level Plateau drift and other localities in close proximity to Ightham (E. Harrison 1928, p. 194). The purpose was to show definitively that eoliths were to be found not only on the surface but in situ, deep within the Pliocene preglacial gravels. Alfred R Wallace had also expressed a desire for such proof, having written to Harrison on November 8, 1893: "I suppose you have not found any of your old flints yet, in situ by digging, or in the undisturbed gravel at some distance below the surface. When you do that you will have more converts" (E. Harrison 1928, p. 189). It would appear that Harrison had already found some eoliths in situ (such as the ones from the post holes dug near Vigo Inn, see Section 3.2.4), but this excavation, financed by the respected British Association, would be more conclusive.

It should be noted that many accepted flint industries were initially discovered on the surface. For example, John Gowlett (1984, p. 72) described the finds at Olorgesailie, in Kenya: "Hand axes were found weathering out on the surface by Louis and Mary Leakey, and it soon became evident that this was one of the major Middle Pleistocene localities of East Africa." Today there is an open-air museum at Olorgesailie, where visitors may walk on catwalks above a land surface covered with stone implements. A similar situation is found at Kilombe in the Kenya rift valley. Gowlett (1984, p. 68) stated: "Kilombe is a massive Acheulean site in Kenya. Artifacts on this site were first noticed in 1972 by geologist Dr. W. B. Jones as an extensive scatter on the surface, evidently weathered out from nearby Pleistocene beds." Describing the Kilombe hand axes, which were made from flakes of stone, Gowlett (1984, p. 70) stated: "many of these large flakes were only gently retrimmed in the final shaping and the original form is quite apparent." The Kilombe flake implements, with only slight human modification, conform to the description of eoliths. At both Kilombe and Olorgesailie, stone implements were later found in situ. The same was true of the sites on the Kent Plateau.

The British Association selected Harrison himself to supervise the Plateau excavations, under the direction of a committee of scientists. Harrison recorded in his notebooks that he found many examples of eoliths in situ, including "thirty convincers" (E. Harrison 1928, p. 189).

### 3.2.9 The Royal Society Exhibition

In 1895, the same year that the Geological Society of London awarded him part of the Lyell Fund (E. Harrison 1928, p. 196), Harrison was invited to exhibit his eoliths at a meeting of the Royal Society. He was quite pleased to have the chance to show his specimens to this scientific elite (E. Harrison 1928, p. 197). Sir Edward Harrison (1928, p. 197) stated: "This was an opportunity not to be missed, and he informed Prestwich of his intention to send for exhibition the specimens found in situ in the excavation in the drift at Parsonage Farm. Prestwich did not dissent from this proposal, but he advised the exhibition also of carefully selected surface specimens, arranged in groups. Harrison followed this counsel in the main, but he included too large a proportion of specimens from the pit, and amongst them specimens which did not impress those who saw them so much as he had hoped."

Some scientists, however, were quite impressed, among them E. T. Newton, a Fellow of the Royal Society and paleontologist of the Geological Survey of Great Britain, who wrote to Harrison on December 24, 1895: 'I hope you will not mind your specimens remaining with me until after the Christmas holidays. I feel satisfied that most of them, to say the least, show human work, and some of these are definitely from one of the pits. . . . Some of the specimens I should be very doubtful about, but there are others that I cannot bring myself to believe are accidental: they have been done intentionally, and, therefore, by the only intellectual being we know of, Man' (E. Harrison 1928, p. 202). Here we have an example of a qualified scientist fully accepting as genuine human artifacts some of the eoliths excavated from the Pliocene Plateau drifts. Modern authorities, who have never examined the specimens in question, might thus be cautious of prematurely dismissing them.

### 3.2.10 The Problem of Forgery

Of course, recognizing intentional human work is always beset with many difficulties, and in his notebooks Harrison mentioned one of the most vexing—forgery. On March 26, 1896, Harrison was visited by William Cunnington, a Fellow of the Geological Society. Harrison wrote: 'He was well acquainted with Flint Jack, the notorious forger of flint implements. Flint Jack's first appearance was characteristic. He entered Mr. Cunnington's office, and, taking from his pocket some flints wrapped in paper, said, 'I hear you buy flint arrowheads.' 'You are Flint Jack.' 'Yes,' he replied, 'I am, and as I was passing I thought you would like to see some arrow-heads made!' On one occasion Mr. Cunnington set him up in life and gave him decent clothing, hoping to reform him, but in vain. Mr. Cunnington sent him to Farmingham to get some fossils. On his return he produced a stone which he said he had bought for a shilling from a shepherd. Recognizing at once that the stone was a forgery, Mr. Cunnington accused him of making it, and refused to have anything more to do with him. The forged implement was made of sandstone. Flint Jack had shaped it with a pick, and had afterwards rubbed it over with earth to disguise its new appearance" (E. Harrison 1928, p. 205).

Harrison was not without his own experience of forgers. In his notebook entry for May 29, 1894, he stated that Smith, an Ightham laborer, had told him: "When Seldon and I were working on the railway he said to me, 'I wonder whether we shall find any flints for Mr. Hamson.' We did not find

any of the right sort, not your sort, you know. He said, 'Here's a big 'un. I'll take him home and hammer him up a bit, file him, and make him look like one of the right sort.' When he brought it to you, he thought you would not know it, but would think it was one of the right sort. He asked you if it was one of the right sort, and you said, 'This is one of your own make, Seldon.' Seldon said, 'I thought he would not know, but I was too tricky, he knows it. It's no use taking home-made ones to him, he knows too much. But he give me some tobacco for being tricky'" (E. Harrison

1928, p. 195). It should, however, be noted that it is not only laborers who are responsible for forgery. As we shall see, in the case of Piltdown man the finger of guilt points in the direction of the scientists themselves.

### 3.2.11 "The Greater Antiquity of Man"

In 1895, Sir John Prestwich published in *Nineteenth Century*, a popular magazine directed at the intelligent public, a review of the Ightham implements titled "The Greater Antiquity of Man." Since it gives, in layman's terms, an excellent summary of the scientific issues involved in the eolith question, we shall here reproduce some sections of the article.

Prestwich (1895, p. 621) first described the Kent Plateau, as it existed in the Pliocene epoch: "It then, was a high level plain of chalk covered by argillaceous [clayey] and drift beds, which thus became furrowed by the escaping rainfall; and as the furrows gradually deepened they ended in the formation of the existing chalk valleys. It <sup>11</sup>, therefore, be seen that these valleys must be newer than the hills through which they are cut, and consequently that the beds of sand and gravel, with the remains of extinct mammalia, together with the flint implements of Palaeolithic man, found in these valleys, must also be newer than the drift scattered on the summit of those hills."

It was in the Plateau drift that the eoliths were to be found. Prestwich distinguished them from Paleolithic implements. Paleolithic implements were very elaborately worked into recognizable tool and weapon forms. Describing the much more crudely fashioned eoliths, Prestwich (1895, p. 622) wrote: "Other scrapers have been formed out of split Tertiary flint- pebbles, sometimes split naturally, and at other times artificially. The edges are trimmed generally all round, so as to act as a rough scraper in whatsoever position the pebble may best be held. At the present day a similar practice prevails among some North American Indians, who, whenever in want of a scraper, select a pebble, which they split and then trim the edges. They rarely keep the old scraper, fresh ones being so easily obtained. This tool is called a pashoa, or scraper, and is used by the Shoshone Indians to dress skins."

Prestwich then pointed out that these rude Eolithic implements from the Pliocene Plateau drifts had features that distinguished them from rude implements that might be found in more recent deposits. "But says one critic, rudeness of form is no test of age, and leaves it to be inferred that these specimens are no older than other rude forms of later ages. Who of the advocates of the plateau implements ever said that it was? I know of none. We particularly remarked in 1892 that rudeness of form alone was no proof of antiquity, and that there were plenty of very rude

specimens of the valley types. We would again emphasise the fact that there are rude implements not only of the valley gravels, but also of neolithic times, whilst among the stone implements of living savages there are many as rude as those of the plateau group" (Prestwich

1895, p. 624).

Prestwich (1895, p. 624) went on to say: 'Each epoch had, however, its typical forms, and these are broadly persistent, however rude the specimens may be. In the neolithic period axe and chisel shapes predominate; in the valley gravels the long pointed and spatula-shaped implements are characteristic of the period; and in the plateau group various forms for scraping and hammering prevail. There are, no doubt, pointed forms in the plateau group, but they have a different cachet from those of the valley group, as these again differ from those of the subsequent Stone period. There are, besides, certain generalised forms which persist throughout all the periods, though perhaps varying a little in some minor details. Simple flakes likewise, more or less worked, are found in all three periods. "

Prestwich then pointed out that many Eolithic implements had been found not on the surface but in excavations into the drift deposits. Of these drift deposits on the Plateau, Prestwich (1895, p. 624) stated: "The drift on that surface is certainly not of local origin, as is shown by the presence in it of fragments of strata derived from the hills some miles distant to the south." As previously noted, the drift could only have arrived in its present position on the Plateau before the chalk valleys, which now intervene between the Plateau and the southern hills, were excavated.

Answering the charge that the eoliths were perhaps naturefacts rather than artifacts, Prestwich (1895, p. 625) stated: 'It has also been frequently asserted that these implements are natural forms produced by the friction of the shingle on the shore or in the beds of rivers. Challenged to show any such natural specimens, those who have made the assertion have been unable, although nearly three years have elapsed since the challenge was given, to bring forward a single such specimen. If, moreover, implements were formed in that manner, they should be found in gravel beds of all ages and origins. So far from running water having this constructive power, the tendency of it is to wear off all angles, and reduce the flint to a more or less rounded pebble."

So here one of Britain's foremost geologists, a Fellow of the Geological Society, and a Fellow of the Royal Society, made quite a coherent case for the human origin and Pliocene date of the Eolithic implements collected by Benjamin Harrison. He answered in a convincing manner all possible objections to his interpretation. Of course, some scientists maintained their opposition, as might be expected of persons with strongly held beliefs. Nevertheless, we must still wonder why, as far as modern paleoanthropology is concerned, the Plateau eoliths have completely disappeared from view. Apparently there is no place in the modern views on human origins for toolmaking hominids in England at least 2-4 million years ago in the Pliocene period.

### 3.2.12 On the Treatment of Anomalous Evidence

In 1896, Prestwich died, but Harrison, in his prominent patron's absence, continued with the Plateau excavations and answered the doubters. On May 18, 1898, Harrison wrote to W. J. Lewis Abbott, reproducing in his letter a poem called "That Chocolate Stone," written by his son (E. Harrison 1928, p. 219):

If only that chocolate stone could explain what the dickens it did in the past,  
That those sages might cease from exciting the brain, and the hatchet be buried at last,  
Whether eolith, neolith, nature, or man, could they but of that question dispose,  
Why, those eminent men might relinquish the pen—till a new controversy  
arose.

This verse, light and humorous though it may be, strikes at the very heart of an important epistemological consideration. In the absence of direct knowledge of the past, any discussion of paleoanthropological evidence, which is always somewhat ambiguous, is certain to involve controversy, because of the differing preconceptions and methods of analysis of the participants in the debate. Empiricism thus becomes inextricably entangled with speculative modes of thought and deeply held emotional biases and prejudices. In most cases, the speculation and bias are carefully masked with a thin veneer of fact. But as imperfect as this process may be, it is, for scientists, the only one that can be applied; therefore, one can at least insist on consistent application of principles and close reasoning from the observed facts. This granted, the case made by Prestwich and Harrison held up quite well against the arguments thrown by their opponents, who simply seemed to be searching for ways to reject something they were a priori not prepared to accept.

An interesting example of this may be found in G. Worthington Smith's continued opposition to Harrison's eoliths. On March 22, 1899, Benjamin Harrison wrote in a letter to Sir Edward R. Harrison (1928, p. 224): "After I became acquainted with Mr. Worthington Smith in 1878, he from time to time sent me interesting trifles, which were duly marked and placed in a drawer. In going through this lot yesterday, I came upon some interesting rude specimens from Basuto Land. These are about as rude as can be, and are facsimiles of those now found in Bushmen's caves in Central Africa. They feature [resemble] my rude implements. Strange that Smith classes all my Plateau finds [eoliths] as cretins, make-beliefs, casuals, travesties—anything but human made. And yet, as long ago as 1880, he sent me those then-acknowledged stones, as if to encourage me to look for similar specimens. When I find them, he scouts [rejects] them!" Here we have an apparent instance of inconsistent application of principles on the part of Smith.

Harrison wrote to Smith about this, who replied, in a somewhat humorous tone, on March 23, 1899, that although he vaguely recalled perhaps having sent some flakes and stones, he failed to see what bearing they had on the present question: "I don't quite see what. . . modern flakes have to do with high-level implements." Smith then stated that he himself had found stones resembling eoliths but never took them home. He then concluded his letter to Harrison with more humor:

"Now I hope you are quite well and blessed with a happy and peaceful mind, without pre-glacial nightmares . . . and palaeolithic tailless apes" (E. Harrison 1928, pp. 224-225). The not so subtle ridicule of the very idea of Homo sapiens existing in the Tertiary is typical of the unscientific methods used by scientists to dismiss evidence that falls outside their particular circle of comprehension. Smith's admission that he himself had deliberately avoided collecting specimens of eoliths is also somewhat damaging to the notion of evenhanded scientific treatment of controversial questions. It often happens that anomalous evidence is ignored. Smith's statement that he failed to see any connection between modern flakes and ancient ones is also quite curious, for such comparative studies of lithic technologies were, and presently are, recognized as an appropriate method for evaluating intentional human work on stone objects.

Smith once wrote to Harrison, who had asked him to consider certain points bearing on the eolith question: "As for answering questions and giving opinions about dubious subjects, it is not always easy, and silence, philosophic doubt, or no settled convictions are better, especially in face of a high priest like you. It is like a Salvation Army captain full of zeal, coming here and asking me about Noah and his ark, Balaam and his ass, and Jonah and his whale. The better plan, according to my view, is to bolt and say nothing" (E. Harrison 1928, p. 187). When one considers the support given to Harrison's discoveries by reputable scientists such as Sir John Prestwich, Smith's characterization of Harrison seems a bit unfair. As we shall see, the put-offs and put-downs from Smith's repertoire are, for a good many scientists, still the favored methods for dealing with evidence that has uncomfortable implications for established views on human evolution. They avoid acknowledging anomalous evidence, never discuss it on its merits, and if pressed, simply ridicule it and those who support it.

### 3.2.13 More Honors for Harrison

As time passed, however, Harrison continued to receive more honors and his eoliths more attention. In 1899, upon recommendation by Prime Minister Balfour, Queen Victoria awarded him a prestigious Civil List pension "in consideration of your researches on the subject of prehistoric flint implements" (E. Harrison 1928, p. 230). The Royal Society also granted him an annuity. That same year, T. Rupert Jones made a presentation about eoliths at the British Association meeting in Dover, exhibiting some small implements that attracted much attention (E. Harrison 1928, p. 231). In August of 1900, Arthur Smith Woodward of the British Museum and Professor Packard of Brown University paid Harrison a visit. Packard accepted all of Harrison's finds as genuine and Woodward agreed that the Plateau drift in which the eoliths were found was probably Pliocene in age (E. Harrison 1928, p. 237). On August 21, 1900, Harrison received a letter from Dr. H. P. Blackmore, who stated that he accepted the eoliths because of "the fairly uniform heights of deposits in which eoliths are found: differing greatly in age of deposit from the more recent river drift or paleolithic gravels" (E. Harrison 1928, pp. 237-238). In 1902, at the British Association meeting in Belfast, W. J. Knowles and F. J. Bennett came out in favor of the eoliths, while Boyd Dawkins was opposed. Some of Harrison's eoliths were placed on exhibition in the

British Museum.

Ray E. Lankester, who was a director of the British Museum (Natural History), became a supporter of Harrison's Kent Plateau eoliths. On April 15, 1904, Lankester wrote to Harrison: "Good health and happiness to you— courageous and Indomitable discoverer of pre-paleolithic man" (E. Harrison 1928, p. 271). Sir Edward R. Harrison stated: "Professor Ray Lankester, who expressed publicly his belief that the eoliths were artificial, and in the Romanes lecture in Oxford, In 1905, declared that they carried 'the antiquity of man at least as far back beyond the paleoliths as these are from the present day', desired to emphasize the value, as evidence of purpose, of similarity of shape of certain eoliths, and wrote to Harrison for specimens to illustrate a book that he had in course of preparation. He was impressed by the large number of implements with a 'tooth-like prominence rendering the flint fit for use as a "borer"' and also by a group he called trinacral, from their resemblance in shape to the island of Sicily" (E. Harrison 1928, p. 270). In his presidential address to the British Association in 1906, Lankester affirmed his belief in "the human authorship" of Harrison's eoliths (E. Harrison 1928, p. 270).

As time passed, Benjamin Harrison continued to win more and more converts. Sir Edward R. Harrison (1928, pp. 287-288) wrote: "A visit from Professor Max Verworn of Gottingen, who had come to England in connexion with the centenary of Charles Darwin's birth, gave Harrison great pleasure. Professor Verworn, who stated that he did not at first believe in eoliths or in any of the supposed evidence of Tertiary man, but had modified his views after personal investigation of the Miocene deposits of the Cantal [Section 4.3.3]. spent five days at Ightham. The fullest use was made of the time available, both in Harrison's museum and in the field. Professor Verworn found an interesting old paleolith in situ in the Plateau gravel at the Vigo, an implement that from its position near the crest of the Chalk escarpment, and its rolled condition, could only have come from the vanished Wealden hills. . . . Harrison could not have wished for a more striking discovery to have been made by his visitor in order to satisfy him of the great antiquity of man in Kent." If Sir Edward Harrison is using the word paleolith in its then accepted sense, we have here an account of an implement more technically advanced than the Eolithic type being found in the very old gravels of the Plateau, and having the worn appearance of implements belonging to those gravels. This gives added support to the possibility that humans of the modern type may have existed in later Tertiary times in England, perhaps 2-4 million years ago.

On July 25, 1909, Professor Verworn wrote to Harrison from Gottingen: "If up to then I had the slightest doubt of the artificial nature of the eoliths of Kent, my visit on the spot and your splendid collection would have quite converted me" (E. Harrison 1928, p. 288).

### 3.2.14 More Opposition

The controversy over the eoliths continued well into the twentieth century. On April 28, 1911, Lord Avebury (Sir John Lubbock) wrote to Harrison: "I am satisfied that many, if not most of your eoliths are worked, though the numbers are staggering. I am not satisfied, however, that palaeolithic implements are in all cases younger" (E. Harrison 1928, pp. 294-295). In his last edition of his book *Prehistoric Times*, Lord Avebury fully accepted the eoliths of Harrison, as well as the

Implements of J. Reid Moir, which we shall discuss in the next section of this chapter ( E. Hamson 1928, p. 305). The opposition, however, continued to criticize the eoliths. In 1911, F. N. Haward published a paper purporting to show that natural forces were able to chip flints in a way that gives the impression of human work. We shall discuss Haward's objections in connection with the flint Implements of J. Reid Moir.

At this point, one may question the necessity of giving such a detailed treatment of the Harrison eoliths. There are several good reasons for doing so. The authors have discovered that modern students of paleoanthropology are generally not at all acquainted with many nineteenth-century discoveries demonstrating the presence of humans of the modern type in Tertiary times. And when these discoveries are brought to the attention of modern students, they tend to categorize them as "crackpot" or "oddball" cases that somehow gained some public notoriety and were quickly dismissed when brought to the attention of scientific authorities. We have also noted a strong prejudice against anomalous evidence that is "old." Old accepted evidence is honored— for example, Java man, highlighted in all modern textbooks, was a nineteenth-century discovery. But the less familiar nineteenth-century evidence, which goes against the theories presented in modern textbooks, is tainted with suspicion, more so if one has never even heard of it before. In such cases, one often encounters in modern students a very strong assumption that if one has not even heard of some anomalous evidence, then it must have been completely rejected on purely scientific grounds long ago. One reason for presenting a detailed account of anomalous evidence is to show that it was not always of a marginal, crackpot nature. Rather anomalous evidence was quite often the center of serious, longstanding controversy within the very heart of elite scientific circles, with advocates holding scientific credentials and positions just as prestigious as those of the opponents. By presenting detailed accounts of the interplay of conflicting opinion, we hope to give the reader a chance to answer for himself or herself the crucial question—was the evidence actually rejected on purely scientific grounds, or was it dropped from consideration and forgotten simply because it did not lie within the parameters of certain circumscribed theories?

In his book *Ancient Hunters and their Modern Representatives*, W. J. Sollas of Oxford rejected Harrison's finds ( E.

Harrison 1928, p. 298 ), In response, Harrison sent him an eolith, On February 1, 1912, Sollas wrote to Harrison: "The specimen you send for my inspection is one of the most interesting of your finds that I have seen, I read its history as follows: (1) Natural agencies detached it as an irregular flake from a flint nodule, . . . (2) It lay in the bed of a stream with the rough side uppermost and was battered on the exposed surface by pebbles, which have left percussion cones as their mark, . . . (3) Still later, it was chipped in a remarkable manner over a portion of its margin" ( E. Harrison 1928, p. 298 ), Here Sollas attributed a remarkable sequence of manufacturing steps to purely natural forces, The end result was a sharp-edged flint implement, something not usually to be expected from the movement of stones in a stream, the random battering of which, as modern authorities point out and anyone can see, tends to produce rounded pebbles,



Sollas then observed: "It is the chipping which is of especial interest to both of us. Two explanations may be given: (1) That the chipping is the result of superincumbent pressure acting upon a yielding substratum, in favour of this it may be pointed out that the chipping is confined to the margin, which we might judge from the general shape of the stone to have thinned off a blunt edge, (2) That the chipping was done by man, in favour of this is the fact that over one part of the specimen the chipping is such as to remove all sharp edges, as if it had been intended for a comfortable hold for the hand .., while on the opposite side the chipping has produced a projecting point which would be very effective if the flint were used as a weapon for striking a blow. In fact this flint would make a splendid 'knuckle duster.' I should not wonder if this was its true nature. But I should not like to commit myself to the assertion that it was" (E. Harrison 1928, pp. 298-299). One wonders why he should not like to commit himself. The points raised here by Sollas himself seem to run very much in favor of the hypothesis that the stone object was of human manufacture.

Sollas then stated (E. Hamson 1928, p. 299): "Granting that it was, however, what does it prove? The patina of the latest chipping is not deep, it looks to my eyes remarkably fresh, and, since palaeolithic implements are found in your deposits, what evidence have you to show that this was not also palaeolithic?" Here the same old question, to which Prestwich long ago had given a detailed and convincing scientific response, came up again. To repeat Prestwich's basic points, the Eolithic Implements, being quite well worn, were distinctly different in appearance from the paleoliths; furthermore, they were sometimes found by themselves in specific deposits. Despite his doubts, Sollas did, however, request more samples for the Oxford museum and Harrison sent six.

At the beginning of the First World War, the British Army, perhaps fearing a German invasion, dug trenches on the hills around Ightham, creating more exposures of gravel for Benjamin Hamson to search. Sir Edward R. Harrison (1928, p. 317) wrote that one of the local flint hunters trained by Benjamin Harrison 'Joined up at the outbreak of war in 1914, was stationed in the Somme valley, found a palaeolith when digging a trench, carried it with him 'over the top', and finally brought it safely to Ightham, and to Harrison, when he came home on leave."

Harrison died in 1921, and his body was buried on the grounds of the parish church, St. Peter's, in Ightham. On his gravestone one finds the words: "He found in life, 'books in the running brooks, sermons in stones, and good in everything'" (E. Harrison 1928, p. 331). A memorial tablet, set in the north wall of St. Peter's on July 10, 1926, bears this inscription: "IN MEMORIAM.—Benjamin Harrison of Ightham, 1837-1921, the village grocer and archaeologist whose discoveries of eolithic flint implements around Ightham opened a fruitful field of scientific investigation into the greater antiquity of man. A man of great mind and of kindly disposition" (E. Harrison 1928, p. 332). Factually speaking, however, the "fruitful field of scientific investigation into the greater antiquity of man" opened by the eoliths of the Kent Plateau was buried along with Harrison.

### 3.3 Discoveries by J. Reid Moir in East Anglia

Our journey of exploration now takes us to the southeast coast of England and the discoveries of J. Reid Moir. Starting in 1909, Moir found flint implements in and beneath the Red and Coralline

Crags of East Anglia (Suffolk). We shall first give an overview of Moir's discoveries and then discuss in detail the scientific controversies they sparked, concluding with a survey of recent opinion.

### 3.3.1 Moir and Harrison

J. Reid Moir, a fellow of the Royal Anthropological Institute and president of the Prehistoric Society of East Anglia, was acquainted with Benjamin Harrison's eoliths. Moir (1927, p. 17) believed the gravels on the Kent Plateau, from which Harrison had recovered his eoliths, were the remnants of an old Tertiary land surface, perhaps as old as the Eocene. But, as we have seen, some authorities would assign the gravels of the Kent Plateau to the Pliocene (Sections 3.2.2,

3.2.4). Moir wrote: "It is probable that these flints were shaped by a race of apelike people who lived on a land surface which existed at one time over what is now the Weald of Kent, which was then enjoying a tropical climate. . . They were probably small, squat men, with very ape-like skulls and projecting jaws, and in many ways more like animals than men" (1927, pp. 17-18, 19).

Moir was an evolutionist. He believed that the degree of primitiveness shown by a very old stone tool industry was indicative of the correspondingly primitive physiological character of the toolmaker. But even today tribal people, physiologically identical to MIT computer scientists, make implements just like the crudest ever found in ancient strata. Furthermore, skeletal remains of fully human character have been found in strata dating back to the Pliocene and even further (Sections 6.2, 6.3). It is therefore possible that the eoliths discovered by Harrison were made by human beings of the type *Homo sapiens sapiens*,

Harrison found many eoliths during excavations sponsored by the British Association for the Advancement of Science. But he found most on the surface and although geologist Sir John Prestwich argued strongly for their Tertiary age, stubborn critics remained doubtful. The geological position of Moir's finds was more secure, for most of them were found in situ, deep below the land surface in various locations in East Anglia.

### 3.3.2 The Age of the Crag Formations

The Red Crag formation (Table 2.1, p. 78), in which Moir made some of his most significant discoveries, is composed of the shelly sands of a sea that once washed the shores of East Anglia. At some places beneath the Red Crag is found a similar formation called the Coralline Crag. Some authorities have placed the Red Crag wholly within the Early Pleistocene. For example, J. M. Coles (1968, p. 19) proposed that the boundary between the Red and Coralline Crags represents the boundary between the Pleistocene and the Pliocene. Others have said that the Red Crag spans the Pleistocene-Pliocene boundary. W. H. Zaguin (1974), for example, placed the lower part of the Red Crag in the Pliocene (Nilsson 1983, p. 108). And still others, such as A. S. Romer (1966, p. 334), put the Red Crag entirely within the Pliocene. Claude Klein (1973, table 6) also placed the Red Crag in the Pliocene and gave it a date of 2.5- 4.0 million years.

Tage Nilsson (1983) called attention to potassium-argon dates for Icelandic formations that some experts correlate with those of East Anglia. Nilsson (1983, p. 106) stated: "If the correlation of the

uppermost Tjornes Beds with the Butley Red Crag is justified, this would imply a probable age of 2.5-3.0 million years for the youngest Red Crag in Britain." According to Nilsson, this view is supported by paleomagnetic data, which suggest a date of over 2.5 million years for the Red Crag. Paleomagnetic dating relies on the fact that the magnetic field of the earth periodically reverses. Signs of this can be detected in various formations, which are thus labeled normal and reversed in terms of their magnetic polarity. Nilsson (1983, p. 106) stated: "the Red Crag in East Anglia is normally magnetized and probably referable to the later part of the Gauss Normal Epoch, and [is] thus more than 2.5 million years old."

After studying the range of geological opinion, we have arrived at a conservative age estimate of at least 2.0-2.5 million years for the Red Crag. The range of dates assigned to the Red Crag raises an important question. The conventionally accepted evidence for human evolution comes from sites representing only the last 2 or 3 million years of the earth's history. Much depends upon being able to arrange fossils from these sites in an accurate temporal sequence. But if the quantitative age determinations of fairly recent formations can vary by hundreds of thousands of years, or even a million or more years, then the integrity of proposed evolutionary sequences, at least insofar as they are founded on stratigraphic evidence, becomes problematic.

Below the Crag of East Anglia are found detritus beds, sometimes called bone beds, composed of a mixture of loose materials—sands, gravels, shells, and bones derived from a variety of older formations. According to Reid Moir, the detritus beds also contain stone implements.

It is certain that these stone implements are older than the Late Pliocene Red Crag. But how much older they actually are depends upon how one interprets the detritus bed below the Red Crag. J. Reid Moir (1924, pp. 642-643) wrote: "The sub- Red Crag detritus bed, which is sometimes as much as three feet in thickness, is, as its name implies, composed of materials of different periods occurring prior to the time when the deposit was laid down. Sir Ray Lankester has shown that these varying materials have been derived from the following sources:—(a) the chalk, [Cretaceous] (b) the London Clay [Eocene], (c) a Miocene land surface, (d) a marine Pliocene deposit (the Diestian Sand), (e) the earlier sweepings of a land surface which submerged after the Diestian deposit, and (f) later sweepings of the same land surface. It will thus be seen that the flint implements, now to be described, that were found in the detritus bed, may be referable to any of the periods represented by c, e, or f of the above list. We have no reason to think that at the epochs when the chalk and the London Clay were being laid down, man was present upon this planet, nor can he well be associated with the marine accumulation (d)."

Modern authorities give similar accounts of the detritus bed below the Red Crag. Tage Nilsson (1983, p. 105) stated: "At the bottom of the Red Crag deposits there is often a stony layer, constituting a kind of basal conglomerate, the Red Crag Nodule Bed. This mainly consists of flint pebbles and phosphorite nodules, washed out from older bedrock. It contains usually densely mineralized and often well-rounded and polished mammal fossils, which must in part be reworked from Eocene and other pre-Quaternary deposits."

According to Nilsson (1983, p. 105). some fossils of Villafranchian species (such as *Mammuthus meridionalis*) were found in the detritus bed. The Villafranchian land mammal stage spans the Pliocene-Pleistocene boundary. This might suggest that the Red Crag detritus bed contains materials from the Early Pleistocene.

Arguing against this is the fact that the detritus beds are often found in situ beneath the intact Red and Coralline Crag (Moir 1924, p. 641), which can be safely referred to the Pliocene. Thus the Villafranchian component of the detritus bed fauna can be assigned to the Pliocene (rather than Pleistocene) part of the Villafranchian stage.

We note that potassium-argon dates obtained for a Villafranchian site in France reached 2.5-3.0 million years (Nilsson 1983, pp. 24, 158). We can therefore conclude that the age of the materials in the detritus beds at the base of the Crag range from Late Pliocene to perhaps Cretaceous in age. The Cretaceous chalk is, however, a marine formation, making the Eocene London Clay the earliest habitable land surface in the stratigraphic sequence of East Anglia.

### 3.3.3 Tools from Below the Red Crag (Pliocene to Eocene)

J. Reid Moir found in the sub-Crag detritus beds many types of stone tools, showing varying degrees of intentional work (Figure 3.6). He concluded that the cruder tools were older. Since the detritus beds, according to this scheme, appeared to contain a succession of stone tools from different periods, perhaps as far back as the Eocene, Moir (1935, pp. 360-361) wrote: "then it becomes necessary to recognize a much higher antiquity for the human race than has hitherto been supposed. I am fully aware of the implication of such a conclusion and the responsibility attaching to those who support it. Nevertheless, after a very careful and painstaking examination of all the available facts, I have been compelled to accept this conclusion as true, and have no hesitation in stating that such is the case."

Moir connected the crudest tools, resembling the Hamson eoliths, with the Miocene elements of the detritus bed below the Red Crag. He considered them to be contemporary with the flint implements discovered in French Miocene formations at Aurillac (Section 4.3). But further than that he would not go, having stated, as above mentioned: "We have no reason to think that at the epochs when the chalk and the London Clay were being laid down, man was present upon this planet" (Moir 1924, p. 643).

Figure 3.6. Pointed implement from below the Red Crag (Moir 1935, p. 364). This specimen is over 2.5 million years old.

J. Reid Moir may have thought in that way, but, as we shall demonstrate later in this book, there is evidence that humans of the fully modern type were in fact present throughout the Tertiary, including the Eocene period, during the time when the London Clay was being deposited upon the underlying Cretaceous chalk (Sections 3.4.2, 5.5. 6.2, and 6.3; also Appendix 2).

So the stone implements collected by Moir from beds below the Red Crag formation could be of that age. In fact, it is quite possible that any of the stone implements, from the crudest to the most sophisticated, could be referred to any period from the Late Pliocene to the Eocene.

At the very least, then, the implements are Late Pliocene in age. But according to present evolutionary theory one should not expect to find signs of toolmaking humans in England at 2-3 million years ago. Two million years before the present, our toolmaking hominid ancestors (of the *Homo habilis* type) should still have been confined to their homeland in Africa. Three million years before the present, we should expect to find in Africa only the apelike australopithecines, who are not generally recognized as makers of stone tools.

Of his Miocene inhabitants of England, Moir (1927, p. 31) wrote: 'Unfortunately, no actual bones of the people who made these implements have yet been discovered, but, judging from these specimens, we conclude that their makers were possessed of considerable strength, and represent an early and brutal stage in human evolution.'

We do not deny the possibility that ape-man-like creatures might have been responsible for the implements reported by Moir. But even today, modern humans are known to manufacture very primitive stone tools. It is thus possible that beings very much like *Homo sapiens sapiens* could have made the crudest of the implements recovered by Moir from below the Red Crag. In the absence of skeletal evidence directly connected with the stone tools, it is impossible to say with certainty what kind of creature manufactured them. All may have been made by humanlike creatures, all may have been made by ape-man-like creatures, or perhaps some were manufactured by humanlike creatures and others by apeman-like creatures.

The implements themselves were a matter of extreme controversy. Many scientists thought them to be products of natural forces rather than of human work. Nevertheless, Moir had many influential supporters. These included Henri Breuil, who personally investigated the sites (Section 3.4.7). He found in Moir's collection an apparent sling stone from below the Red Crag (Section 5.3.1). Another supporter was Archibald Geikie, a respected geologist and president of the Royal Society (Millar 1972, p. 100). Yet another was Sir Ray Lankester, a director of the British Museum. Lankester identified from among Moir's specimens a representative type of implement he named *rostro-carinate*. This word calls attention to two prominent characteristics of the tools. "*Rostro*" refers to the beaklike shape of the working portion of the implements, and "*carinate*" refers to the sharp keellike prominence running along part of their dorsal surface (Moir 1927, p. 26).

Lankester presented a detailed analysis of the "Norwich test specimen" (Figure 3.7). A particularly good example of the *rostro-carinate* type of implement, it was discovered beneath the Red Crag at Whitlingham, near Norwich (Moir 1927, p. 28; Osborn 1921, p. 576). If the Norwich test specimen

is from below the Red Crag, it would be over 2.5 million years old. If it is from below the Norwich Crag as suggested by Sparks and West (1972, p. 234), it would be over 2.0 million years old (Table 2.1, p. 78). The Norwich test specimen combined a good demonstration of intentional work with clear stratigraphic position. Sir Ray Lankester wrote in a Royal Anthropological Institute report in 1914: "it is not possible for anyone acquainted with flint-workmanship and also with the nonhuman fracture of flint to maintain that it is even in a remote degree possible that the sculpturing of this Norwich test flint was produced by other than human agency" (Coles 1968, p. 27).

Professor J. M. Coles of Cambridge University (1968, p. 27) later noted: "His description was full and was accompanied by drawings and photographs showing that approximately 40 flakes had been removed from various angles and positions around the flint, consisting of two cleaving fractures, a group of large conchoidal flakings, and a third group of smaller flakings directed upon specific parts, particularly the beaked portion."

Figure 3.7. The Norwich test specimen. J. Reid Moir (1927, p. 28) said it was found beneath the Red Crag at Whitlingham, England. The beak (arrow) forms the working portion of the implement, which, if from below the Red Crag, would be over 25 million years old.

At a lecture before the Royal Society in London, Lankester said he hoped "that no one would venture to waste the time of the society by suggesting that sub-crag flints had been flaked by natural causes, as by so doing it would be plain that they had a very scanty knowledge of such matters." Someone present did, however, venture to suggest exactly that. Lankester said it was "the sort of thing I would expect to hear from a savage." Another time G. Worthington Smith, known to us for his skeptical exchanges with Benjamin Harrison of Ightham, said of the eoliths and pebble tools: "We have here choppers that do not chop and borers that do not bore." Lankester retorted: "You, sir, are a bore who does bore" (Moir 1972, p. 100).

About the age of the rostro-carinate tools, Lankester stated in 1941: "I do not intend to proceed without caution to any conclusion on this subject, but it seems to me quite possible that there is a close relationship between the men who made the Upper Miocene rostro-carinate implements of Aurillac [Section 4.3J and those who made similar implements in Suffolk before the deposit of the Red Crag" (Moir 1935, p. 359).

Moir (1935, p. 360) himself also observed that intact counterparts of the beds that provided the materials for the detritus layer below the Red Crag could be found elsewhere in Europe and

contained stone implements: "the Upper Miocene deposits of France [Sections 4.2, 4.3J and some older beds in Belgium [Section 4.4J have already yielded flaked flints, claimed by certain competent investigators as of human origin."

#### 3.3.4 The Foxhall Finds (Late Pliocene)

One important set of discoveries by Moir occurred at Foxhall, where he found stone tools (Figure 3.8) not in the detritus bed but in the middle of the Red Crag formation. Some authorities, including Moir, have placed the upper part of the Red Crag in the Early Pleistocene, but our review of the range of geological opinion has led us to place the entire Red Crag formation in the Late Pliocene. The Foxhall implements would thus be over 2.0 million years old. Moir (1927, p. 33) wrote: "The finds consisted of the debris of a flint workshop, and included hammer-stones, cores from which flakes had been struck, finished implements, numerous flakes, and several calcined stones showing that fires had been lighted at this

spot. The Foxhall implements are, in the majority of cases, of a yellowish white colour, and more finely made than the still more ancient specimens found at the base of the Crag, and give us a very clear idea of the type of workmanship of which these ancient Suffolk people of Early Quaternary times were capable, While if the famous Foxhall human Jaw-bone, which was apparently not very primitive in form, was, indeed, derived from the old land surface now buried deep beneath the Crag and a great thickness of Glacial Gravel, we can form the definite opinion that these ancient people were not very unlike ourselves in bodily characteristics," The jaw spoken of by Moir has an interesting history (Section 6,2,1), For now, we shall simply note that some scientists who examined it considered it like that of a modern human being,

Figure 3.8. Front and rear views of two stone tools from the Red Crag at Foxhall, England. They are Late Pliocene in age. Henry Fairfield Osborn (1921, p. 572) said of the tool on the left: "Two views of pointed flint implement flaked on the upper and lower surfaces and with a constricted base, from sixteen-foot level of Foxhall pit. Primitive arrowhead type, which may have been used in the chase." Of the implement on the right, Osborn wrote: "Borer (perpoii) from sixteen-foot level of Foxhall."

It is unfortunate that the Foxhall Jaw is not available for further study, for it might offer further confirmation that the flint implements from Foxhall were of human manufacture. But even without the presence of actual human skeletal remains, the tools themselves point strongly to a human presence in England during the Late Pliocene, perhaps 2,0-2,5 million years ago,

The American paleontologist Henry Fairfield Osborn (1921, p. 573) came out strongly in favor of the implements having been manufactured by human beings and argued for a Pliocene date: "Proofs which have rested hitherto on the doubtful testimony of irregular eoliths generally considered by archaeologists as not of human manufacture, now rest on the firm foundation of the Foxhall flints in which human handiwork cannot be challenged; these proofs have convinced the most learned and most conservative expert in flint industry in Europe today, namely Abbe Henri Breuil of the Institut de Paleontologie Humaine." According to Osborn (1921, p. 573), the Foxhall specimens included borers, arrowheadlike pointed implements (some hafted), scrapers, and side scrapers very much like early Mousterian radeaux,

Osborn (1921, p. 566) concluded: "This discovery of man in Pliocene time delights the present writer for a personal reason, namely, because it tends to render somewhat more probable his prophecy made in April 1921, before the National Academy of Sciences at Washington that one of the great surprises in store for us in science is the future discovery of Pliocene man with a large brain." This sort of talk would not go down very well today.

Osborn (1921, p. 565) backed not only the Foxhall flints but the rest of Moir's work as well: "The discoveries of J. Reid Moir of evidences of the existence of Pliocene man in East Anglia open a new epoch in archaeology. . . . they bring indubitable evidence of the existence of man in southeast Britain, man of sufficient intelligence to fashion flints and to build a fire, before the close of the Pliocene time and before the advent of the First Glaciation."

But whether one accepts Osborn's Pliocene date or Moir's Early Pleistocene estimate, neither is to be expected if one accepts the standard version of hominid evolution in an African homeland. This is especially true if, as the Foxhall jaw indicates, the maker of the Foxhall flint tools was fully human. The first *Homo sapiens* are thought to have come into existence only a couple of hundred thousand years ago at most, and the standard textbook version is that fully modern *Homo sapiens* is only about 100,000 years old.

Another scientist won over by the Foxhall finds was Hugo Obermaier, previously a consistent and vocal opponent of Eolithic discoveries. Obermaier was one of those scientists who believed that eoliths were produced by natural forces similar to the forces operating in cement and chalk mills (Section 3.5). But Obermaier (1924, p. 41) wrote: "Very recently a large bed of flints with evidences of fire has been found on the eastern coast of England near Norwich and beneath the Late Pliocene deposits known as the

'Red Crag' and the 'Norwich Crag.' The authenticity of the flints as of human origins is disputed by some archaeologists, but is accepted by others, including Louis Capitan, the veteran archaeologist of France, and Henri Breuil, who is frequently quoted in these pages. This discovery of Foxhall is the first evidence we have of the existence of Tertiary man."

Someone might have asked Obermaier if, having accepted the Foxhall flint tools as proof of human existence in the Tertiary, he might reevaluate any of the Tertiary Eolithic industries he had once rejected.



Figure 3.9. Pointed tool from the Cromer Forest Bed, East Runton, England (Moir 1927, p. 45). It could be from about .8 million to 1.75 million years old, depending upon how one dates the Cromer Forest Bed.

### 3.3.5 Cromer Forest Bed (Middle or Early Pleistocene)

Thus far we have considered Moir's discoveries in the Tertiary bone bed below the Late Pliocene Red Crag and his finds in the Red Crag itself at Foxhall. We shall now turn to some discoveries in the more recent Cromer Forest Bed of Norfolk. As we have seen (Section 2.19: Table 2.1, p. 78), the Cromer Forest Bed dates from about .4 million years to about .8 million years ago, or perhaps even as much as 1.75 million years ago. During this period, according to Moir, the delta of the Rhine extended to East Anglia.

Moir found specimens of a stone industry (Figure 3.9), including large handaxes, lying on the beach at Cromer and East Runton in Norfolk. He stated that they originated from a stone bed exposed in the base of the cliffs along the shore. Moir (1924, p. 649) wrote: 'The Cromer specimens are found chiefly upon the foreshore. . . . They lie upon the chalk, and have evidently been derived from a formation at the very base of the Cromer Forest Bed series of deposits, which form the lowermost strata of the high bluffs of the Norfolk coast. ... In some places, as at East Runton, about two miles northwestward of Cromer, large areas of the implementiferous bed can be seen in situ upon the chalk, and from this deposit have been recovered several very definite examples of Early Paleolithic hand axes.' If the implements are, as Moir stated, from the lowest part of the Cromer Forest Bed formation, they would, according to modern estimates, be at least .8 million and perhaps as much as 1.75 million years old.

Moir (1924, p. 652) went on to describe the implements: "There is no doubt that the Cromer industry shows an advance from the sub-Crag culture, but it is nevertheless closely related to it. The ancient Cromerians, using probably large hammerstones of flint, were able to detach in some cases enormous flakes of flint, and the whole industry is on a large and massive scale. On the foreshore at Cromer the contents of a workshop site were found, comprising hand axes, choppers, side scrapers, points, and numerous flakes. . . . Their skill in flint-flaking is evidenced by the immense flake scars produced by the primary quartering blows, the well-formed striking platforms, and the regular and accurate secondary flaking." Critics of anomalous stone tools often ask for just the type of evidence reported by Moir—a variety of finished tool types and flakes in close association, indicating a workshop site.

### 3.3.6 Moir Versus Haward

Having briefly reviewed Moir's discoveries beneath the Red and Coralline Crag, in the Red Crag at Foxhall, and from the Cromer Forest Bed, we shall now examine the history of the scientific

controversies surrounding them. J. M. Coles (1968) of Cambridge gave a rare modern summary of the disputes.

In 1919, F. N. Haward attacked Moir's discoveries, claiming that they were the product of geological pressure acting on flint. Moir and A. S. Barnes replied to Haward in articles published in the Proceedings of the Prehistoric Society of East Anglia. Moir (1919, p. 158) made the following comments: "It appears that Mr. Haward has found in the Norwich Stone Bed a flint, or flints, which exhibit a flake detached, but not removed from the parent block, and he concludes, and rightly concludes, that such flakes have become so detached since the bed in which they occur was laid down. He draws attention to the well-known fact that flints in the chalk, and, I may add, in other deposits as well, break up into pieces of varying size, and that such breakage is of natural origin. And once more I am in agreement. But here, I fear, we take widely different paths."

Haward believed the cause of breakage to be pressure. Moir agreed that this was indeed one possible cause, and pointed out that he had himself published a paper on this topic ("The Fractured Flints of the Eocene 'Bull-Head' at Coe's Pit, Bramford, near Ipswich") in the Journal of the Prehistoric Society of East Anglia. Moir (1919, pp. 158-159) went on to state: "But I know also that pressure flakes exhibit certain peculiarities of their surfaces which differentiate them markedly from other flakes which have been removed by percussion, and so far as I can ascertain, Mr. Haward has not yet demonstrated, scientifically, that the few flakes upon which he bases his portentous argument have without doubt been detached by pressure. It may also be recalled that the Norwich Stone Bed, as I can testify from actual observation, contains very often fragile bones of mammals, and the sands above it, at Whitlingham, where a large proportion of the sub-crag implements described by Mr. Clarke have been found, have embedded in them even more fragile shells. And it is legitimate to ask why, if pressure is fracturing the hard, resistant flints in the Stone Bed, the easily-broken organic remains mentioned are quite frequently found intact." Rejecting the pressure hypothesis, Moir suggested another explanation. Before being embedded in the deposit a flint nodule might have been subjected to blows strong enough to produce incipient bulbs of percussion. Later, under the influence of heat, for example, the flakes might have come off. Moir (1919, p. 159) added that Haward himself had noted that some flaked flints he studied bore signs of percussion.

Moir (1919, p. 159) then stated: "But whatever the exact cause of such fracturing may be, it is clear that such cases are very rare, and moreover, when they are found, only one or two flakes are seen to be in contact with the parent block. Yet Mr. Haward does not hesitate to infer that all the other flints exhibiting numerous flake-scars upon their surfaces, and a definite implemental form, have been produced by this same natural fracturing. When also it is remembered that many, if not most, of these latter specimens show by their colouration and condition that they are definitely more ancient than the bed in which they now occur, it will be seen that this inference rests upon a very attenuated and shaky basis. But if this is the case in regard to the Norwich Stone Bed flints, what is one to say about the extension of Mr. Haward's inference to the specimens found under totally different conditions beneath the Red Crag of Suffolk, and where, up to the present, no evidence of any fracturing in situ has been seen?" Moir (1919, pp 160-161) pointed out that the

specimens found below the Red Crag displayed only signs of flake removal by percussion, with no sign of pressure fracturing.

Moir (1919, p. 161) concluded his remarks with this affirmation: “students of human and animal bones have regarded the existence of man in the Pliocene as almost a necessity, and from my later researches I incline to the belief that not only was man present on this earth at that period, but that he was then culturally much more advanced than has hitherto been imagined.”

### 3. 3.7 Warren’s Attack on Moir

Still the opposition to Moir continued, with scientists clinging with remarkable tenacity to variations of the natural pressure-flaking hypothesis. Coles (1968, p. 27) stated: "A more scholarly attack on the authenticity of the 'industries' was made by S. Hazzledine Warren in 1921, who claimed that mechanical movement of flint upon flint under pressure produced flaking comparable to that seen on not only the Kentish eoliths but also the rostro-carinates and other Crag assemblages. Warren based his argument upon his observations of fractured flints in Eocene deposits in Essex, and upon experiments. Moir and Barnes defended themselves vigorously, and claimed that natural pressure flaking could easily be distinguished from the edge-flaking on the Kentish eoliths and on the Crag series. The naturally-produced specimens claimed by Warren to be of rostrocarinate form from the Essex gravels were said to be entirely different." In 1923, an international commission of scientists concluded that the flaking on the specimens collected by Warren was in fact different from that on Moir's implements (Section 3.3.8).

Warren’s report was delivered in an address to the Geological Society of London, and was later published in the Society's journal. In the Eocene location studied by Warren, the flints were lying beneath layers of sediment, upon a chalk surface, where he claimed that they had been subjected to pressure and differential movement by “solution of the chalk surface.” In other words, the flints had been crushed by the pressure of overlying layers as they slipped into holes eroded in the chalk by the action of ground water. Warren claimed to have found, in locations where such crushing had occurred, many specimens resembling not only eoliths but Mousterian implements as well.

Of one such specimen ( Figure 3.10), Warren (1920, p. 248) stated: ‘This, a good example of a trimmed-flake point, is the most remarkable specimen of the group.

Figure 3.10.S. Hazzledine Warren said that this object, which he believed to be the product of natural pressure flaking, almost exactly resembled a Mousterian trimmed point implement ( MacCurdy 1924b, p. 657). But although found in an Eocene formation, it could in fact be of human manufacture.

If considered by itself, upon its own apparent merits, and away from its associates and the circumstances of its discovery, its Mousterian affinities could scarcely be questioned. But, like all the other specimens illustrated, I dug it out of the Bullhead bed myself in circumstances which preclude the possibility of mistake." In this connection George Grant MacCurdy, director of the American School of Prehistoric Research in Europe, wrote: "Warren states that if the best selected flakes from the Bullhead Bed were mingled with flakes from a prehistoric workshop floor, they could never be separated again unless it were by their mineral condition" (1924b, pp. 657-658).

Much depends upon whether or not the flaking on Warren's specimens actually resembled that of Paleolithic humans. If the flaking was different, then Warren's argument against Moir becomes irrelevant. If, on the other hand, the flaking was similar, then what are we to make of specimens, such as the one depicted in Figure 3.10, which are so very much like accepted Paleolithic stone tools?

Warren appeared to take for granted, in a fashion typical of those who shared his prejudices, that it was impossible to find in Eocene strata implements of human manufacture, particularly those displaying a relatively high level of stoneflaking technique. Moir, as we have seen, expressed the same view—no toolmaking beings could have existed in the Eocene (Section 3.3.3). But those who are free from such prejudices might justifiably wonder whether Warren had actually discovered, in the Eocene strata of Essex, a genuine object of human manufacture.

A similar event occurred some years earlier in France, where H. Breuil, in attempting to prove the natural origin of eoliths by geological pressure, also found in an Eocene formation specimens exactly resembling Late Paleolithic stone tools (Section 3.4.2). Breuil, however, was convinced that humans could not have existed in the Eocene.

As we noted in a previous chapter (Section 2.9), T. McKenny Hughes also expressed a conviction that humans could not have existed as far back as the Eocene, despite the presence in an Eocene formation of pierced shark's teeth like those made by today's inhabitants of the South Pacific. Other finds of objects of human manufacture in formations that might be as old as the Eocene occurred in California (Section 5.5). In this context, Warren's Bullhead Bed discoveries, if regarded as genuine implements, do not seem so out of place.

In the discussion that followed Warren's report, Mr. Dewey, one of the scientists present, pointed out that in some cases the Kent eoliths and Moir's rostro-carinates are found in the middle of Tertiary sedimentary beds and not directly on the hard chalk. This circumstance, said Dewey, would rule out the particular pressure explanation given by Warren.

Warren had displayed some specimens during his talk. But Reginald Smith complained that Warren (and Breuil in France) had compared their natural productions with only a few of the very poorest eolith specimens. Smith accused Warren of discouraging research in early deposits.

The record stated: "Mr. H. Bury thought it unfortunate that such a discussion should have been raised without a fair representation of both sides of the case among the exhibits. The author [Warren] and Mr. Haward had brought forward the best specimens that they could find in support of their case; but for comparison they only produced some half dozen very inferior Kentish eoliths, and no sub-Crag implements at all. It was a mistake to suppose that believers in Pliocene man had ignored these pressure-flaked flints from the Eocene beds; on the contrary, the differences in detail which they observed between the two categories formed an essential factor in their argument" (Warren 1920, p. 251).

Bury's point is well worth noting, for one often encounters something like the following in discussions of eoliths by their detractors. The skeptical authority will point out that such and such scientist found in Tertiary strata stone objects he incautiously believed to be of human manufacture and that the discovery was a matter of controversy for some years until such and such scientist delivered his definitive report that conclusively demonstrated that the stone objects had been produced by the pressure of the overlying layers. But in recounting this history the skeptical authority ignores the fact that the original discoverer had carefully considered and dismissed that very possibility. In considering the eolith question with an open mind, one learns to be suspicious of definitive disproofs, which often turn out to be quite rickety intellectual contraptions.

The notes of the discussion also recorded the following ironic remarks by one of the members of the Geological Society: "Mr. A. S. Kennard congratulated the author [Warren] on an important discovery, and considered that the paper strongly supported the claim for the human origin of the Kentish eoliths. He agreed with the author that it was unfair to decide from a few examples, and that the proper test was the whole group. Judged by this standard, neither of the series shown [by Warren] resembled the Kentish eoliths, since the more numerous and characteristic specimens [shown by Warren] were quite unknown on the Plateau" (Warren 1920, p. 251). Kennard thus turned the tables on Warren, taking his attempt to dismiss the eoliths as proof of their genuineness.

### 3.3.8 An International Commission of Scientists Decides in Favor of Moir

At this point, the controversy over Moir's discoveries was submitted to an international commission of scientists for resolution. Coles (1968, p. 27) related that this group "was overwhelmingly in support of Moir's conclusions, that the flints from the base of the Red Crag near Ipswich were in undisturbed strata, and that some of the flaking was indubitably of artificial origin." In the words of the commission report: "The flints are found in a stratigraphic position, without trace of resorting, at the base of the Red Crag. A certain number of the flints do not appear to have been made by anything other than voluntary human action" (Lohest et al. 1925, p. 44).

The commission, formed at the request of the International Institute of Anthropology, was composed of Dr. L. Capitan, professor at the College of France and the School of Anthropology; Paul Fourmarier, professor of applied geology at the University of Liege and the School of

Anthropology; Charles Fraipont, professor of paleontology at the University of Liege and the School of Anthropology; J. Hamal-Nandrin, professor of the School of Anthropology at Liege; Max Lohest, professor of geology at the University of Liege and the School of Anthropology; George Grant MacCurdy, professor at Harvard University; Mr. Nelson, archeologist of the National Museum of Natural History of New York; and Miles Burkitt, professor of prehistory at the University of Cambridge (Lohest et al. 1923, p. 54).

The commission wanted to settle the following questions (Lohest et al. 1923, p. 53): "(1) At the point where the flints considered worked were discovered, is it established that the strata in which they were found are definitely Pliocene and that no action of resorting or intrusive deposition is responsible for the introduction into ancient beds of modern objects? (2) Are the flints found among rocks or other conditions that could have produced pseudo-retouching by impact or pressure?" Concerning the flints themselves, the commission was to answer the following questions: (1) Are the flints of the Crag worked, retouched, or utilized? (2) Can the retouching be compared to that produced by natural physical action? (3) Can one affirm that the flaking and retouching are due to intelligent and voluntary work?"

To answer these questions, the commission visited the principal sites where Moir had collected his specimens, including locations at Ipswich, Thorington Hall, Bramford, and Foxhall Road. They also examined the collection at the Ipswich Museum, the personal collection of Moir, and Warren's collection of pressureflaked flints from the Bullhead Eocene beds. Also visited were the collections at the Cambridge Museum and the British Museum at South Kensington, as well as the collection of Mr. Westlake at Fordingbridge near Salisbury, which included his enormous collection of flints from Puy Courty and Puy de Boudieu near Aurillac, France (Lohest et al. 1923, p. 54).

The geologists Max Lohest and Paul Fourmarier reported on the stratigraphy of Moir's discoveries. Lohest and Fourmarier stated: "The purpose of our mission to Ipswich was to verify whether flints showing indisputable signs of intentional work are in fact encountered in undisturbed Tertiary strata" (Lohest et al. 1925, p. 54). These two experts confirmed, at Thorington Hall, that the Red Crag lies upon the Eocene London Clay, and that at the bottom of the Red Crag there is a "detritus bed," which contains flints (not rolled), flint pebbles, phosphate nodules, fossil remains of deer, and also flints showing signs of intentional work.

Lohest and Fourmarier reported: "After minute examination, we believe we can affirm that the Red Crag, because of its cross-bedded stratification and numerous fossils at the pit at Thorington Hall, constitutes incontestably a primary deposit in place, not reformed, and that the deposit is Pliocene and formed in the immediate vicinity of the seashore. If the flints of this deposit are really the work of an intelligent being, then there is no doubt, according to us, that this being existed in England before the great marine invasion of Trophon antiquum, considered by all geologists as dating to the late Tertiary epoch" (Lohest et al. 1925, pp. 55-56).

J. Hamal-Nandrin and Charles Fraipont also reported on the geological considerations: "The detritus bed from which the flints are recovered is surmounted by several meters of Red Crag deposits containing Pliocene shells. The Red Crag is apparently an ancient shore, and the shells

accumulated in the sand on the actual shore. There are very delicate shells, such as bivalves; many are found whole, and the least pressure, the least touch, causes them to break. A deposit of this type is primary, not composite or resorted (remanie). It is in the underlying detritus bed that the flints are found. At Thorington Hall the detritus bed lacks many rocks. It contains coprolites, phosphate nodules, and only some small flint pebbles. The superimposed Red Crag is also almost without rocks" (Lohest et al. 1925, p. 57).

Hamal-Nandrin and Fraipont then stated: "The rarity of rocks does not permit us to suppose that the flints may have been retouched by shocks or pressure in situ. It had to be done, either naturally or artificially, before their incorporation into the beds. Below the detritus bed is the London clay, from which some rolled blocks have been incorporated into the detritus bed. The detritus bed contains, along with bones of whales, fossils of terrestrial mammals certainly characteristic of the Pliocene. This gives evidence that it was upon an ancient land surface that the sea of the Late Pliocene deposited the Red Crag, a shoreline formation at this point. If the flints from below the Red Crag at Thorington Hall, in undisturbed strata, give signs of intelligent work, the being that used them is Pliocene" (Lohest et al. 1923, p. 57),

Hamal-Nandrin and Fraipont then turned their expertise to determining the presence of signs of intentional work on the sub-Crag flints: "A certain number of the pieces collected from below the Red Crag, and now found in the collections of Mr. Reid Moir and the Ipswich Museum, present, in our opinion, the characteristics that distinguish worked flints: a striking platform, clear bulb of percussion, and edges with series of small flakes removed, indicating intentional retouching and utilization as a tool. If you were to find these in strata of the Mousterian period, you would not hesitate to say that they are tools showing intentional work and utilization. ... In our present state of knowledge, we cannot see that anything other than intelligent action could be capable of producing such effects, ... At Thorington Hall, the rarity of stones and their dispersal does not permit us to suppose that the flints have been naturally retouched by impact or pressure. One can observe that in the level where the specimens are found one does not find any worn and fractured flints other than the ones appearing to be the result of intentional work. The worked flints are not only rare, but extremely rare, according to prehistorians who have studied the strata" (Lohest et al. 1923, p. 58),

After studying several of the collections of flints previously mentioned, Hamal-Nandrin and Fraipont declared themselves in favor of Moir's view that the sub-Crag flints were implements of human manufacture. They further stated: "The chipped edges of the flints collected by Mr. Warren from the Eocene Bullhead beds, along with those produced artificially by him, are very different from the edges of those belonging to the detritus beds below the Crag at Ipswich" (Lohest et al. 1923, p. 58).

Capitan's report also supported Moir's position, both on the sub-Crag finds and those from the Cromer Forest Bed and related formations on the Norfolk coast. Capitan noted that the Pleistocene Boulder Clay had yielded to Moir and others some rare specimens of Mousterian type. But the middle glacial gravels below the Boulder Clay, according to Capitan, contained an

enormous number of flints modified by glacial action, The flakes and their pseudoretouching from purely natural causes became an object of special study and consideration, for the precise purpose of comparison with the flints recovered from below the Red Crag, Certain pieces from the glacial gravels did, however, appear to be clearly worked, resembling the Chellean and preChellean types of tools, They were chipped in simple fashion and had a bright characteristic patina (Lohest et al. 1923, p. 59).

Capitan described the Red Crag as a sandy clay, colored red by oxides of iron, containing isolated siliceous stones, phosphate concretions of round small size, fragments of shells, rare shark teeth and even more rare whale bones, and also relatively small pieces of fractured flint. These elements, he noted, were concentrated in a layer at the base of the Crag. Capitan stated: 'This is the detritus bed. It is here exclusively (except at Foxhall where there is a second bed almost the same as this) that one finds, only after great trouble, isolated in the midst of the sands, and never in contact with other flints, some flakes and pieces of broken flint, and even more rarely the typical Red Crag specimens" (Lohest et al. 1923, p. 60).

Members of the commission carried out four excavations into the detritus bed over the course of four days and found five or six typical specimens. Capitan stated: "I will not neglect to say that the flints were absolutely in place in compact terrain; two reposed at Thorington Hall on the underlying clay. ... at Thorington Hall you have a detritus bed covered by marine sands. So everything there is from either before or contemporaneous with the sea that deposited the Crag" (Lohest et al. 1923, p 60).

Studying the specimens of Moir and those at the Ipswich Museum, Capitan categorized them as doubtful, probable, and definite. About half the total specimens were in the doubtful category, with almost another half in the probable category. In the probable group were all those flakes that showed traces of adaptation or retouching identical to that on accepted tools. Capitan stated: "We consider that the greatest number of these pieces are genuine tools bearing diverse traces of intentional work which one can distinguish, with practice, from natural fracturing and flaking. But if someone wants to express doubts, then we leave the discussion to them and will not seek to demonstrate the intentional work" (Lohest et al. 1923, pp. 61-62).

But Capitan stated that in addition to the many specimens in the probable category the commission recognized twenty pieces as indisputably worked: "They are of definite form, exactly like accepted Mousterian pieces. These are not freaks of nature or naturally broken stones used without modification as tools—they were products of volition, and show signs of a definite intent to construct a particular kind of tool" (Lohest et al. 1923, p. 62). The commission selected eleven pieces for reproduction in their report: two Mousterian-like side scrapers (racloirs), two discoidal end scrapers (grattoirs), two points, two blades (one with much retouching), an actual handaxe, a sort of big chisel, and a big retouched piece of the grattoir form.

Capitan, praising the rigorous scientific procedures applied by Moir and his collaborators, then stated: "One might object that the small number of definite specimens is not sufficient, but this is due to the extremely rigorous process of selection. We are persuaded that a great many of the



ones not selected are also worked' ' (Lohest et al. 1923, p. 62). Capitan added: "The small number selected for this demonstration is deliberate because their legitimacy as products of human industry cannot in the least be challenged even by technical experts" (Lohest et al. 1923, p. 62).

Capitan concluded: "We need not uselessly continue the discussion about whether these pieces are worked or not, giving undue attention to explanations from incompetents. For any person who has any real acquaintance with the characteristics of worked flints, such questions will not come up" (Lohest et al. 1923, pp. 62-63). If one rejected Moir's finds, stated Capitan, then one would have to reject about 80 percent of the generally accepted Mousterian pieces (Lohest et al. 1923, p. 63).

Figure 3.11. A side scraper (racloir) discovered beneath the Red Crag at Thorington Hall, England (Lohest et al. 1923, p. 63).

Capitan next described some of the undisputed specimens. These came from Thorington Hall, Bramford, and the Bolton Company brickfield. From Moir's reports (1924), it appears that the primary tool-bearing layer at each of these sites is the detritus bed below the Red Crag. This would make the flint tools Capitan described at least 2.5 million years old. And because the detritus bed contains materials from ancient Eocene land surfaces, the tools might be up to 55 million years old.

Concerning an implement from below the Red Crag at Thorington Hall (Figure 3.11), Capitan said: "The very best piece ... is a great and thick racloir (side scraper) fashioned from an irregular oval flake, with numerous bulbs of percussion. It is of the same form as many of the most typical Mousterian racloirs, and like them it is retouched on all sides. On the outer surface, near the point of the instrument ... a carefully retouched depression accommodates a finger for gripping the implement. In truth, this is a piece that can just as much be said to have been manufactured by humans as the best Mousterian racloirs. On the plane surface, on the other end of the implement ... is an enormous bulb of percussion" (Lohest et al. 1923, p. 63).

Of two discoidal grattoirs (end scrapers) recovered from Thorington Hall (Figure 3.12), Capitan stated: "Made from thick flakes, and carefully retouched all around, they both have in the middle of the upper surface a long deep flake removed.

Figure 3.12. Two discoidal scrapers from below the Red Crag at Thon'ngton Hall, England (Lohest et al. 1923, p. 64).

On the other side of each, which is smooth, there is a bulb of percussion" (Lohest et al. 1923, p. 63).

In using a grsttoir, or end scraper, the scraping edge of the implement is held lengthwise along the line of force (or end first). In using a radoir, or side scraper, the tool's scraping edge is held perpendicular to the line of force (or sideways).

In addition, Capitan drew attention to a particular implement (Figure 3.13) that he described as being "well retouched on every side and having an extremity terminating in a bevelled edge carefully made by regular retouching" (Lohest et al. 1923, p. 64).

Capitan also noted "a big radoir, with the cortex partially removed and with the cutting edge carefully dressed and adapted by a series of regular and multiple retouchings. This edge is so perfectly rectilinear as to give clear indication it is of human origin" (Lohest et al. 1923, p. 65).

Another implement (Figure 3.14) was retouched on two of its edges and displayed on its face three long flake scars. The fact that the three flake scars on the implement were parallel was, according to Capitan, a certain sign they were deliberately removed in succession. He believed this specimen from below the Red Crag appeared to be a handaxe absolutely identical to the best pre-Chellean types from the Somme region of France.

Capitan (Lohest et al. 1923, p. 66) described another specimen as follows: "A thin blade with a bulb on the inferior surface, and a very precise imprint of a second blade removed from the upper part.

Figure 3.13. An implement from below the Red Crag (Lohest et al. 1923, p. 65).

Figure 3.14. An implement from below the Red Crag at Bramford, England (Lohest et al. 1923, p. 66).

This work is absolutely human" (Figure 3.15). Yet another object illustrated in Capitan's report was a pointed implement, with an apparent bulb of percussion visible at the base (Figure 3.16).

In concluding his analysis, Capitan definitively stated that "there exist at the base of the Crag, in undisturbed strata, worked flints (we have observed them ourselves). These are not made by anything other than a human or hominid which existed in the Tertiary epoch. This fact is found by us prehistorians to be absolutely demonstrated" (Lohest et al. 1923, p. 67).

Surprisingly, even after the commission report, Moir's opponents, such as Warren, persisted in attempting to show that the flint implements from beneath the Red Crag and elsewhere were the product of some kind of natural pressure flaking.

Moir and Barnes kept defending their position and picked up supporters. Coles (1968, p. 29) stated: "In 1932 T. D. Kendrick outlined some of the different viewpoints, and came down strongly in support of Moir, not so much on the geological problems involved as on the character of some of the flints." About Moir's flints and other Eolithic industries, Kendrick said that "many of them are to be regarded as 'probably artifacts,' while there are one or two (in the British Museum) .. that I feel certain are man's handiwork" (Coles 1968, p. 29).

Figure 3.15. A blade implement found beneath the Red Crag formation at Bramford, England (Lohest et al. 1923, p. 66).

Figure 3.16. A pointed implement from below the Red Crag formation, England, thought to be from Late Pliocene to Eocene in age (Lohest et al. 1923, p. 65).

### 3.3.9 Continued Opposition

As far as the opposition was concerned, their attempted counterexplanations became more strained; indeed, it seems no proposal was too extreme to win the support of those who for one reason or another could not find room for Moir's discoveries within the bounds of their paleoanthropological parameters.

Coles (1968, p. 29) informs us: "One of the final statements was made by Warren in 1948 in an address to the geological section of the Southwestern Union of Scientific Societies. ... He agreed with Moir in considering that, at the present day, wave action was not an effective process in the fracturing of flint in a way comparable to that seen on Moir's Crag specimens, but tried to find some other natural process that could have flaked the submarine flints exposed by erosion of that Chalk. Warren concluded that during the formation of the Crag deposits, the area must have been subject to the arrival of icebergs from the north. Such ice, grounding near the shores of the Crag sea, might well have caused the pressure crushing and striation of the flints exposed on the sea bed. These arguments, apart from being practically the last word in the controversy, also neatly disposed of many of the points made by Moir about the differences between sea action fractures and his 'implements/ and allowed the exposure and deposition of fragile marine shells amidst the ice-fractured stone beds.'

We do not yet have in our possession a copy of Warren's 1948 address, but one gets the impression, from Coles's account, that the iceberg hypothesis was a somewhat desperate exercise in pure speculation. One wonders whether icebergs move onto shorelines in the manner suggested by Warren; and granting that they may, is there at present any hard evidence, anywhere in the Arctic or Antarctic regions, suggesting they have produced implement-like objects in the manner suggested by Warren? To our knowledge, no one has given any proof that icebergs can produce the numerous bulbs of percussion and elaborate retouching reported above by Capitan. Furthermore, as pointed out previously, many of the Red Crag specimens are lying in the middle of sediments and not on hard rock surfaces against which an iceberg might have crushed them. In addition, Coles (1968, p. 29) reported that at Foxhall implements occur in layers of sediment that appear to represent land surfaces and not beach deposits. This would also rule out the iceberg action imagined by Warren.

### 3.3.10 Silence Ends the Debate

After Warren put forward his iceberg explanation, the controversy faded. Coles (1968, p. 28) wrote: 'That. ... the scientific world did not see fit to accept either side without considerable uncertainty must account for the quite remarkable inattention that this East Anglian problem has received since the days of active controversy.' This may be in part true, but there is another possible explanation—that the scientific community decided silence was a better way to bury Moir's discoveries than active and vocal dissent. By the 1950s, with scientific opinion lining up solidly behind an Early Pleistocene African center for human evolution, there would have been little point, and perhaps some embarrassment and harm, in continually trying to disprove evidence for a theoretically impossible Pliocene habitation of England. That would have kept both sides of the controversy too much alive. The policy of silence, deliberate or not, did in fact prove

highly successful in removing Moir's evidence from view. There was no need to defeat something that was beneath notice, and little to gain from defending or supporting it either.

### 3.3.11 Recent Negative Evaluations of Moir's Discoveries

Although most modern authorities do not even mention Moir's discoveries, a notice of dismissal may be found in *The Ice Age in Britain*, by B. W. Sparks and R. G. West (1972, p. 234): 'The beginnings of tool manufacture are shrouded in doubt by the similarity of primitive tools to naturally-occurring flaked pebbles. The earliest dated tools identified are found in Africa (Lower Pleistocene,

1.75 million years) and are of the so-called chopper tool or pebble tool type, made by striking a few flakes from the side of a pebble in one or two directions. Such an industry has been associated with *Homo habilis* and *Homo erectus*. In Britain such Lower Pleistocene industries have not been found. But early in this century many flints from the Lower Pleistocene Craggs were described as being artifacts, such as the flints, some flaked bifacially, in the Red Crag near Ipswich, and the so-called rostrocarinates from the base of the Norwich Crag near Norwich. All are now thought to be natural products. They do not satisfy the requirements for identification as a tool, namely, that the object conforms to a set and regular pattern, that it is found in a geologically possible habitation site, preferably with other signs of man's activities (e.g. chipping, killing, or burial site), and that it shows signs of flaking from two or three directions at right angles." Sparks and West, of Cambridge University, are experts on the Pleistocene in Britain.

Briefly responding to Sparks and West, we may note that Moir and other authorities, such as Osborn and Capitan, were able to classify the Crag specimens into definite tool types (handaxes, borers, scrapers, etc.) comparable to those included in accepted Paleolithic industries, including the Mousterian. The Foxhall site, with the Foxhall jaw, was taken by many authorities to represent a geologically possible habitation site. Moir (1927, p. 33) considered it to be a workshop area and noted signs of fire having been used there. As far as flaking from several directions at right angles is concerned, this is not the only criterion that might be applied for judging human workmanship upon stone objects. Even so, M. C. Burkitt of Cambridge (1956, p. 104) did find flaking from several different directions at right angles on some of the implements that were collected by J. Reid Moir.

Among other scientists who opposed Moir's discoveries and saw fit to say so in print was K. P. Oakley. Coles (1968, p. 29) stated: "Although Oakley (1961) goes so far as to say that 'the chipping in some cases suggests intelligent design,' he believes that none can be accepted without some reserve." As have many other opponents of crude stone tool industries, Oakley included in his book some illustrations of natural products that supposedly resembled objects thought to be implements. Leland W. Patterson (1983, p. 303) has responded: "As an example of superficial observation, the author has received comments that the edge damage on natural flakes illustrated by Oakley resembles retouch patterns of unifacial tools. A careful examination of Oakley's illustrations shows that the flake scars do not form a uniform pattern as is characteristic of the results of perpendicular force applications in making unifacial tools. In Oakley's illustrations, flake scars at the edge go at a variety of angles from the plane of the ventral face of the specimens,

instead of being parallel flake scars mainly perpendicular to the plane of the ventral face. Flake scars also vary widely in size."

Yet another late-twentieth-century opponent of eoliths was F. Clark Howell. Coles (1968, pp. 27-30) stated: 'Howell [1966, p. 89] dismisses all of this material by stating that 'the angles of fracture and the nature of the flake removal... fall outside the range of variation of specimens known otherwise to be of human manufacture,' but this is surely not a valid basis for rejection, particularly in view of the variability of known industries of the Lower and early Middle Pleistocene throughout the Old World.' We fully agree with Coles on this point and shall more fully discuss the important matter of angles of fracture later in this chapter.

### 3.3.12 A Slightly Favorable Modern Review of Moir's Finds

Coles himself provides an exception to the usual instinctive rejection of Moir's discoveries (or complete silence about them). He felt it "unjust to dismiss all this material without some consideration" (Coles 1968, p. 22). But as we shall see, Coles did, after some consideration, dismiss almost all of it.

Concerning the Forest Bed discoveries, Coles (1968, pp. 24, 27) stated: "Of the immense quantity of flints available, only a small proportion were flaked, and Moir believed, rightly it seemed, that wave-action could not have caused this fracturing. Most of the flaked pieces were irregular, but a few straight-edged retouched flakes occurred. Moir examined other areas of foreshore, to serve as a check on natural flaking in exposures, and claimed that there were no struck flakes outside his 'workshop-sites,' which had yielded both fractured flakes and cores."

But then Coles (1968, p. 27) shifted to negative expressions: "These sites, however, are not generally accepted as showing any sign of man's activity." In fact, there is no "general acceptance" of any of Moir's sites. But to what extent does general acceptance reflect the actual truth regarding the human manufacture of Moir's implements? Coles himself admitted that the flaking on the Forest Bed specimens had probably not been accomplished by the action of waves but did not himself propose any specific alternative explanation.

Coles (1968, p. 24) went on to say: "The sites lay on the foreshore, and Moir believed that the occupation had taken place on the Stone Bed, and that it should therefore extend under the cliffs at Cromer." But Coles (1968, p. 27) asserted that "the flint deposit is believed to occur only on the foreshore, and not to extend under the Cromer Forest Bed in the cliffs at Cromer."

Coles appears to have been wrong about this. West (1980), who conducted extensive geological research on the Cromer Forest Bed Formation, made several references to the Cromer Stone Bed underlying the Cromer Forest Bed formations. He identified it as the source of the flints found on the foreshore at various locations and said it was of the same general age as the top part of the Norwich Crag (Table 2.1, p. 78). The Stone Bed, and any implements from it, would thus be about 1.0 to

1.5 million years old.

And about the Foxhall Implements, from bands of black sediment in the middle of the Red Crag, Coles (1968, p. 29) had this to say: "they, and they alone, were stratified in such a position as to make their presence and fracturing in situ under the conditions envisaged by Warren most unlikely."

This statement is not completely accurate. Capitan reported that the implements from the detritus bed below the Red Crag were also found in conditions that ruled out natural fracture by either pressure or impact (Section 3.3.8). As at Foxhall, the implements were found in sandy deposits, distant from other pieces of flint. Burkitt made similar observations (Section 3.3.13).

In any case, Coles (1968, p. 24) made this favorable comment about the implement-bearing layers at Foxhall: "Above and below were horizontally stratified clean sand deposits, showing no evidence of natural agencies sufficient to flake the flints found sporadically in the two dark layers." The flints were also unrolled. Their sharp edges indicated to Coles that the flaking was human in origin. The random battering of natural forces tends not to preserve sharp edges.

Coles (1968, p. 29) further explained that the dark layers in which the flints were found "may represent temporary periods of land exposure during a general marine phase in this area." In other words, the layers represent a probable habitation site. Coles (1968, p. 29) added that the relative rarity of the flints, as well as the fact that it was hard to account for their presence by natural means, indicated that they arrived at their positions in the dark layers in the Red Crag by artificial (that is to say, human) agencies.

"Unfortunately, however," said Coles (1968, p. 29), "few of the flints found by Moir are convincing: a number are small flakes little over one inch in length, others are larger with edge flaking. One or two are bifacially retouched."

The presence of bifacial retouch (retouching on both sides of an edge) is an extremely good indication of human manufacture. Leland W. Patterson, an expert on lithic technology, stated (1983, p. 304): "random forces could seldom produce a long interval of bifacially retouched edge that is sharp. Natural fractures tend to produce blunt and rounded bifacial edges, because of the steep transverse nature of most natural fracture." That even one bifacially retouched implement was found at Foxhall is highly significant. It means that the other flakes cannot be so easily dismissed.

The fact that many of the flaked objects found at Foxhall are small does not rule out human manufacture. At many sites, small flakes are regarded as byproducts of the tool manufacturing process. Another possibility is that the small flakes themselves might have been used as implements. John Gowlett (1984, p. 144) wrote in *Ascent to Civilization*: "Microliths are very small stone tools, generally 3 cm long [about 1.2 inches] or less, made from small flakes or segments of blades. Usually one side has been blunted by the 'backing' technique, a form of retouching in which tiny flakes are struck off the edge. ... flakes which lack retouch are just as likely to have been used as tools."

In fact, microliths, which occur principally in the Middle and Late Stone Ages, are regarded as a technological advance upon the earlier large handaxe industries. They are typical of *Homo sapiens*, and are identified with highly evolved cultural activities such as agriculture and bow-and-arrow hunting. For example, Gowlett (1984, p. 145) stated that "the tools were sometimes fitted end-to-end, in a row, into a curved blade desirable in a sickle." Therefore, small size alone should not lead one to label stone flakes "unconvincing" as tools.

Coles himself (1968, p. 29) noted that one should be careful in ruling out human workmanship simply because stone objects do not appear convincing: "it must be born in mind that a number of the flakes from a site such as Vertesszolos . . . might also not have been accepted as demonstrating human workmanship if they had not been found on an undoubted working floor, in association with other human activities."

Is there any evidence at Foxhall, in addition to the flaked flints, that might lend support to a human presence? The answer to this question is yes. First of all, the variety of flints and flakes found at Foxhall suggested a workshop location. Second, Moir noted the presence of burned stones, a sign that fire had been used at the site. And, finally, as previously noted, a fully human jawbone was recovered at Foxhall, from the same levels that contained the stone implements.

Confronted with this uncomfortable fact, Coles (1968, p. 28) lapsed into the reflexlike response typical of scientists with strong preconceptions about what might and might not be found in strata of certain ages: "As far as Foxhall is concerned, the presence of the Jawbone, quite dearly *Homo sapiens*, suggests disturbance of some sort. Perhaps local landslip has occurred, bringing an upper Crag deposit on top of a recent land surface, which itself overlays Crag sands in situ." But Coles did not provide any actual geological evidence that such a "landslip" had actually occurred. Coles's proposal adds nicely to our collection of examples demonstrating how scientists adhering to the particular view of human evolution now in vogue must often engage in speculative mental exercises in order to bring anomalous evidence within the bounds of an acceptable time frame.

But despite his generally negative opinion about Moir's discoveries, Coles nevertheless felt that three particular implements were worthy of further study. These were: (1) "the undoubted handaxe apparently from the Cromer Till at Sidestrand in Norfolk" (Coles 1968, p. 29); (2) a handaxe from the Stone Bed at Whitlingham; and (3) a handaxelike implement from the detritus bed below the Red Crag at Bramford. According to Coles (1968, p. 29), these three objects were the "one positive source of support for Moir's views." Otherwise, Coles felt that Warren's iceberg hypothesis was essentially correct.

Here we would like to emphasize that we do not share Coles's suspicion that Warren's highly speculative iceberg hypothesis is preferable to the findings of the international commission of geologists and anthropologists, who held that Moir's implements were definitely made by humans. Therefore, we do not believe that the final decision about Moir's discoveries must rest solely on the interpretation of the three test specimens mentioned by Coles. Nevertheless, they are significant, and we shall now examine them, beginning with the Sidestrand find.



Moir (1923, p. 135) gave this description of the Sidestrand handaxe discovery (Figure 3.17): "The specimen was discovered lying upon its flat undersurface, and finnly embedded in Boulder Clay at the foot of the cliff, which passed directly into, and was apparently part of, the underlying mass. "

Coles (196S, p. 27) mentioned thatthe Boulder Clay at Sidestrand, Norfolk, inwhichthe ' undoubted handaxe" was found, was apparently the Cromer Till. The Cromer Till is from the Anglian glacial period (Table 2.1, p. 7S), which began about .4 million years ago. But the handaxe "is believed to have been transported by glacial action from the upper part ofthe Cromer Forest Bed" (Coles 1968, p. 27).

In this regard, Moir (1923. pp. 136-137) stated: "The occurrence of this specimen in Boulder Clay, a deposit composed solely of derived material, makes it certain that the land surface, upon which the implement originally lay, must be looked for in some deposit more ancient than the Till at Sidestrand. Examination of the Cromer Forest Bed immediately underlying the Lower Glacial deposits of the Norfolk Coast has shown me that flints, exhibiting flake-scars of the same colour as those of Mr. Sainty's specimen, occur freely in the Upper Freshwater Bed (the highest division of the Cromer Forest Bed series), and it seems to me very probable that the implement originally belonged to this deposit."

The implement was quite unworn. Moir (1927, p. 47) explained this as follows: "the glacial clay . . . very frequently contains portions of the Freshwater Bed, which were torn up by the glacier in its advance." The sharp edges of the implement could thus have been preserved by the surrounding Fresh-water Bed materials.

According to West (19S0, p. 116), the Upper Freshwater Bed, as defined by J. Reid Moir and his contemporaries, includes materials ranging from the last part of the Cromerian temperate stage, at .4-.5 million years b.p., to the beginning part of the Pre-Pastonian cold stage, at 1.50-1.75 million years b.p. (Table 2.1, p. 7S).

At 1.5 million years ago, the Sidestrand specimen, accepted by Coles as a definite handaxe, would be quite anomalous. Handaxes of this sort are usually attributed to *Homo erectus*, but according to the standard human evolutionary theory, at

1.5 million years ago, *Homo erectus* should still have been confined to Africa, where he should only recently have come into being. At .4-.5 million years, however, the Sidestrand specimen would be barely within the range of conventionally accepted stone implements in England. Let us now consider the two remaining test specimens mentioned by Coles. The first is an implement (Figure 3.1S) "apparently from the Stone Bed at the base of the Norwich Crag, at Whitlingham." The Whitlingham site is at the foot of a cliff near Thorpe, on the Norfolk coast.

Figure 3.17. Four views of a stone implement from the Cromer Till at Sidestrand (Moir 1927, p. 46). Coles (1968, p. 29) called it an "undoubted handaxe."

Figure 3.18. Implement from Whitlingham, England (Coles 1968, p. 26, after Moir). Coles (1968, p. 29) called it "conw'ndng as a handaxe." J. Reid Moir said it came from the Stone Bed beneath the Norwich Crag, giving it an age of about 2 million years.

Coles stated (1968, p. 29): "On the face of it, this object is convincing as a handaxe. Unfortunately, it was not discovered in situ but lay with fallen material at the foot of a tall section. ... It is possible that this object came from the till and not from the Stone Bed although Sainty and Moir claimed it was definitely from the latter."

But Coles (1968, p. 24) also said: "This implement is in fresh condition, and it is unlikely that it could have survived transport in this condition." This observation suggests that the implement might have come not from the glacial till but from the much older Stone Bed. An implement crushed beneath a moving glacier would probably have had its sharp edges removed. In our discussion of the Sidestrand specimen earlier in this section, we noted that Moir offered an explanation why the handaxe found there was not worn by glacial action— it might have been incorporated within a large piece of Forest Bed sediment taken up by the advancing glacier. Moir backed up this assertion by stating that the glacial clay at Sidestrand does in fact contain intact pieces of the Upper Freshwater Bed. But this special explanation (which Coles did not mention) does not necessarily apply at the Whitlingham site. Therefore, the unworn condition of the Whitlingham handaxe is consistent with its being incorporated in the Stone Bed. Coles (1968, p. 24) noted that the Stone Bed at Whitlingham contained 'abundant shells, in situ, and unbroken,' as well as 'many slender nodules of flint. ... also undamaged.'

In addition to the handaxe, a good many other flaked flint objects were recovered from the Stone Bed at Whitlingham, England. In regard to these discoveries, Breuil said (1922, pp. 228-229): "Mr. Reid Moir was able to retrieve some pieces in a stratigraphic position at the base of a cliff. That the enormous flakes found there were made by very violent human percussion cannot be doubted."

Coles (1968. p. 24) stated: "Many of these Thorpe flakes were believed to exhibit deliberate flaking. The flakes include irregular forms with even retouch along one or two edges." The presence of these other flaked implements "In a stratigraphic position" at the base of the cliff at Whitlingham tends to confirm the Stone Bed as the source of the handaxe.

The Whitlingham handaxe, if from the glacial gravels that make up the Cromer Till, would be not much more than .4 million years old. But if, as is most likely, the handaxe is from the Stone Bed underlying the Early Pleistocene Norwich Crag, it would be about 2 million years old (Table 2.1. p. 78).

Coles (1968. p. 29) said that his last test specimen (Figure 3.19) "was found at the base of the Red Crag at Bramford in Suffolk and its stratigraphical horizon is not in doubt." He added: "It lay in the Detritus Bed in London Clay and was sealed by Crag sands. It is reminiscent of Chellean axes with triangular sections, but is considerably rolled; although it bears some 25 fake scars, and has lost all its cortex. the irregular nature of the object itself is not convincing."

In another description of the same piece, Coles (1968. p. 24) stated that it was "superficially of handaxe form, with thirteen fake scars upon one face, and twelve upon the other." He added: "These scars appear to have been directed from a multiplicity of positions on the edges. and are sufficiently elongated to overlap at the center of one face, producing thereby a triangular sectioned 'tool'" (Coles 1968. p. 24).

The position of this specimen in the detritus bed beneath the Red Crag means that it is at least Pliocene in age (2-5 million years old). But because the detritus bed contains materials from land surfaces dating back as far as the Eocene, the handaxe could be as much as 55 million years old.

Figure 3.19. Handaxe from below the Red Crag at Bramford, England (Moir 1935, p. 364). It could be anywhere from 2 million to 55 million years old.

All in all, Coles, in spite of his negative conclusions, can be commended for his willingness to discuss Moir's discoveries. At the end of his review, he stated: "A fair comment on the East Anglian material would, I think, be concerned to point out that the typology of the claimed implements

was not necessarily outside the range of variation known from humanly worked Industries In Europe and Africa, but that we have very little Information about the natural flaking processes available In East Anglia In early Pleistocene times, some of which might well have been capable of producing flaked flints Including bifacially-worked 'handaxes'; no natural sources are known today which could do this under observation. Our greatly augmented evidence about the chronology of early tool-making In other parts of the world continues, however, to suggest how extraordinary It would be If the East Anglian Crag Industries were of human manufacture" (Coles 1968, p. 30). This Is an Incredible line of reasoning. No natural forces known to today's scientists can account for the production of the handaxes and other flaked Implements. Nevertheless, Coles hesitates to accept them as the product of intentional human work.

It may be that In terms of the "greatly augmented evidence" available to Coles, human manufacture of the East Anglia specimens would seem extraordinary In terms of "chronology," that Is to say, their unexpected age. But In terms of the even more greatly augmented evidence presented In this book, human manufacture of the East Anglia Implements during the late Tertiary and earliest Pleistocene would seem quite within the bounds of the ordinary.

In this regard, a modern authority, Gowlett (1984, p. 76), reported that four flakes and five pebble choppers were found at Le Vallonet, southern France, in old beach sediments dated 1-2 million years old. If we assign these eolithlike stone tools to the oldest part of their probable date range, they would be roughly contemporary with some of the East Anglia specimens, such as those from Foxhall. Gowlett called the Le Vallonet specimens doubtful, yet he mentioned them in his book. He did not, however, mention Moir's discoveries.

### 3.3.13 Positive References to Moir's Finds

We shall now consider some isolated examples of positive scientific reporting on J. Reid Moir's discoveries from the latter half of the twentieth century. Cambridge University archeologist and anthropologist, M. C. Burkitt, who served on the International commission that examined Moir's implements In the 1920s, gave favorable treatment to them In

his book *The Old Stone Age*, published In 1956.

Burkitt was particularly Impressed with the site at Thorington Hall, 2 miles south of Ipswich, where flint Implements had been collected from the Crag deposits. "At Thorington Hall bivalve shells with the hinges still Intact have been collected from just above the artifacts. This Is very Important evidence for the prehistorian, as no subsequent differential movement of the gravel, such as might have caused fracturing of the contained flints, can have taken place, since It would certainly have led to the smashing of the delicate hinges of these shells. Incidentally, too, at this site, as well as at Foxhall, the deposit in which the specimens occur Is of a sandy nature and not packed with pebbles. So even if differential movement had occurred no fracturing due to the pressure of one stone against another could have resulted" ( Burkitt 1956, p. 108). That the implements were found isolated in apparently undisturbed sandy deposits also appears to rule out Warren's

suggestion (Section 3.3.9) that they were formed by icebergs crushing flint against the underlying chalk.

As far as Foxhall was concerned, Burkitt (1956, pp. 108, 110) stated: "At Foxhall the chipped flints were found at two different levels only, and this can be best explained If we consider that these levels were actually old land surfaces on which man lived, In other words that we are dealing with 'floors' or actual occupation sites.'

Burkitt ( 1956, p. 110 ) further stated: "The argument that the flints were chipped elsewhere by natural forces and later incorporated In these late pliocene gravels cannot always be maintained. Small flakes, as well as large specimens, occur together and this would not happen under such circumstances, as the selective action of flowing water would cause the smaller and lighter specimens to be collected together at one site and the larger and heavier objects at another." Burkitt's strong arguments in favor of actual living floors at Foxhall help resolve the doubts expressed by Coles and others about human manufacture of the flint objects found there.

Regarding Moir's discoveries from the Cromer Forest Bed formations, Burkitt (1956, pp. 112 - 113) wrote: "For the most part these consist of large flakes carefully struck off from a core, the striking platform being unfaceted and frequently inclined at a high angle to the main flake surface. Although there is not always any further trimming, a sharp cutting edge has often been obtained. . . Occasionally more finished tools are found and rarely specimens of a core-tool type such as choppers, etc. have been collected. Essentially, however, It is a flake industry with which we are dealing. . . It would

appear that these chipped specimens were made by men who lived at a time when the earlier beds of the Cromer Forest series were being laid down, for a few undoubted artifacts have been discovered in them, and the horizon at which they occur probably represents the ancient land surface on which these makers of the Cromerian industries wandered, collecting the raw material for their tools from exposures of the stone bed below. Actually most of the Cromer Forest Bed is now also masked by talus."

Burkitt (1956, p. 112) then delivered a striking conclusion about the Implements discovered in and below the Red Crag: "the eoliths themselves are mostly much older than the late pliocene deposits in which they were found. Some of them might actually date back to pre-pliocene times." In other words, he was prepared to accept the existence of Intelligent toolmaking hominids in England over 5 million years ago. Because there is much evidence, including skeletal remains (as we shall show in our coming chapters), that humans of the fully modern type existed in pre-Pliocene times, there is no reason to rule out the possibility that Moir's implements from the below the Crag formations were made by Homo sapiens over 5 million years ago.

Another supporter of Moir's finds was Louis Leakey (1960d, pp. 66, 68), who wrote: "It is more than likely that primitive humans were present in Europe during the Lower Pleistocene, just as they were in Africa, and certainly a proportion of the specimens from the sub-crag deposits appear to be humanly flaked and cannot be regarded merely as the result of natural forces." Implements

from below the Craggs would, however, be not Early ( Lower) Pleistocene but at least Late Pliocene In age.

Leakey (1960d, p. 68) then made an Important point: "It must be constantly borne in mind that although simple pebble chopping tools without any more elaborate forms are typical of the Kafuan and Oldowan, similar tools continued to be made and used by the makers of much more advanced cultures, just as we ourselves still use candles although we also have electric light. " This observation is essential to understanding lithic remains. There is no reason to suppose that crude stone tools, found in Early Pleistocene or Tertiary beds, must have been made by correspondingly primitive hominids. This is especially true when we consider that examples of much more sophisticated tools, of kinds universally attributed to Homo sapiens, occur in beds of the same Early Pleistocene and Tertiary ages (Chapter 5), as do skeletal remains indistinguishable from those of modern human beings (Chapter 6).

These discoveries are not well known, having been forgotten by science over the course of many decades or in many cases eliminated by a biased process of knowledge filtration. The result is that modern students of paleoanthropology are not in possession of the complete range of scientific evidence concerning human origins and antiquity. Rather most people, including professional scientists, are exposed to only a carefully edited selection of evidence supporting the currently accepted theory that protohuman hominids evolved from apelike predecessors in Africa during the Late Pliocene and Early Pleistocene, and that modern humans subsequently evolved from the protohuman hominids in the Late Pleistocene, in Africa or elsewhere. This book is intended to supply those concerned with paleoanthropological studies access to the full range of evidence. Objectively reviewed, the totality of evidence, in the form of incised bones (Chapter 2), stone implements (Chapters 3-5), and human skeletal remains (Chapter 6), suggests that the current theory of an African evolution is erroneous. It appears that toolmaking hominids indistinguishable from Homo sapiens sapiens were present in habitable areas all over the planet far back into the Tertiary epoch. This does not, however, rule out the simultaneous presence of more apelike hominids. some of whom may have manufactured some of the most primitive stone implements. In Appendix 2, we catalog selected radical evidence suggesting higher cultural levels in the Tertiary and even earlier.

### 3.4 Breuil and Barnes: Two Famous Debunkers of Eoliths

In paleoanthropology, we sometimes encounter the definitive debunking report—a report that is repeatedly cited as having decisively invalidated a particular discovery or general category of evidence. In the case of European eoliths. two papers are good examples of definitive debunking reports. These are H. Breuil's paper claiming that pseudoeoliths were formed by geological pressure in the French Eocene formations at Clermont (Oise), and A. S. Barnes's paper claiming to demonstrate, by statistical analysis of platform striking angles, the natural origin of Eolithic industries. We shall now review these two papers.

#### 3.4.1 Breuil's Attempt to End the Eolith Controversy

In 1910, Abbe Henri Breuil conducted investigations he thought would put an end to the eolith controversy. In his often cited report ("Sur La Presence d'Eolithes a la Base de l'Eocene Parisien"), Breuil said that for several years his attention had been drawn to the gravel pits of Belle-Assise, near Clermont, in the department of Oise, northeast of Paris. Excavations there had exposed a bed of chalk, which formed the stratigraphic base for the overlying formations. Above the chalk was a bed of clay containing layers of angular pieces of flint, interspersed with layers of gravel and sand. Above the flint-bearing clay was a very thick deposit of greenish Bracheux sands, which belong to the Thanetian formation, at the base of the Eocene (Obermaier 1924. p. 12). Breuil concluded that the flint-bearing beds below the sands must therefore belong to the very beginning of the Eocene. They would thus be about 50-55 million years old according to modern dating. Some modern authorities put the Thanetian formation as far back as the Late Paleocene, at 55-60 million years (Marshall et al. 1977, p. 1326). Above the Bracheux sands were gravel deposits from the Pliocene and Pleistocene.

"With the onset of the discussions concerning the question of how the eoliths were produced." wrote Breuil (1910, p. 386), "I frequently thought that an examination of the broken flints at the base of the Bracheux sands of the Thanetian at Belle-Assise would yield some interesting observations." Breuil gathered specimens over the course of three years, carefully observing patterns of breakage. "I always avoided using metal tools to extract the flints, and also took care to reject those that had been subjected to contact from the picks of the workers. It is somewhat easy, at the moment one extracts a flint, to examine its surfaces, to see if the fracturing has been produced recently, or if the breakage took place before the excavation. The surfaces of ancient fractures always have thin deposits of iron or manganese" (Breuil 1910, p. 386).

Breuil (1910, pp. 386-387) then stated: "Having noted, without any possibility of doubt, the presence of flints with fractures indicative of intentional work and retouching, and thus resembling what are called eoliths. I invited many persons to come and confirm the fact. Capitan, Cartailhac, and Obermaier were able, along with me, to collect these characteristic flint objects with their own hands. Mr. Commont, with whom I had the pleasure of making an inspection of the flint-bearing strata, also collected some specimens. Furthermore, Commont found flints with features resembling intentional work and retouching in various Eocene exposures in Picardy. The stratigraphic position of the discoveries was the same as at Belle-Assise."

Breuil then described specimens that displayed retouching, bulbs of percussion, and striking platforms. Some showed regular bifacial flaking, typical of Late Paleolithic implements. Others had chipping confined to the side of the flake opposite the bulb of percussion, another characteristic of human work. But Breuil (1910, p. 388) warned: "If in our descriptions we use terminology that normally is applied to proper tools of human manufacture, that is nothing more than a convention, a manner of expression, and does not at all signify that we suppose for an instant we are dealing with ancient implements made by people of Eocene or pre-Eocene times."

Breuil felt that human action could be ruled out with complete certainty because the flints were found in an Eocene formation. Like many other scientists, he could not imagine human beings

existed in the Eocene. when the mammals known from fossils were. apparently, quite different from those of today. Breuil (1910. p. 406) wrote of "the absolute unlikelihood of the presence, before the deposit of the Bracheux sands or during their deposit, of an intelligent being, a worker of flint. "

But if human action were to be excluded, how, then. had the flint objects been produced? Searching for a natural explanation. Breuil (1910, pp. 387-388) wrote: 'It is easy to observe that the flints have not been subjected to transport, for their sharpest edges remain intact. From among the processes that could have resulted in their fracture. one can therefore eliminate the mechanical action of water, either of oceans or rivers. Further examination of the fracturing gives evidence of a different kind of mechanical action. which was able to produce facets and impressions analogous to those produced by intentional human work, or by energetic localized force. A bulb of percussion, more or less clearly present, is often found at the point where a flake was taken off from the surface of the parent block. One can totally eliminate a thermal origin of the fractures, because fractures produced by heat, in the form of surface flaking or cracking of the entire flint block. are completely different."

Breuil (1910, p. 403) then presented specimens that he believed shed a very clear light on the mode of production of the "pseudotools" he had reviewed: "They are pieces of flint which were flaked while in their positions in the interior of the beds. the fragments remaining in contact with each other. It is easy to see that these fragments present conchoidal fracturing, with the production of positive and negative bulbs of percussion" (Figure 3.20).

Figure 3.20. Henri Breuil (1910, p. 405) found examples of flakes removed from parent blocks of flint by geological pressure in an Eocene formation in Clermont (Oise), France. Such specimens, he believed, showed that eoliths were not made by human beings.

Conchoidal fracturing is fracturing that results in elevations or depressions shaped like the curved inner surface of a shell. A positive (raised) bulb of percussion is found on the surface of a flake detached from a flint core. The core retains a negative impression of the bulb. Breuil held that the fracturing that produced these bulbs of percussion was the result of geological pressure. But what about the further signs of modification that normally are present on even the crudest eoliths?



To account for this, Breuil also described a few flakes, found adjacent to parent blocks of flint, that had some chips removed from an edge. According to Breuil (1910, p. 403), geological pressure caused this apparent retouching. He proposed that as a flake was detached, it rotated, causing chips to be removed from its thinner edge as it scraped over the surface of the parent block of flint (Figure 3.21).

We shall give careful attention to Breuil's arguments, because similar reasoning has been used in attempts to discredit many of the discoveries discussed in this book.

For example, Hugo Obennaier (1924, p. 4) observed in his book *Fossil Man in Spain*. 'The controversy concerning Thenay [France did not subside until the year 1901, when L. Capitan and G. d'Ault du Mesnil showed how purely natural agencies might produce effects very similar to human handiwork, one of the most important being earth pressure above the brittle flint.'

Figure 3.21. (1) Parent block of flint, found in an Eocene formation at Clermont (Oise), France. (2) Flake, apparently removed by geological pressure, found in contact with parent block of flint. (3) Opposite side of flake, with one edge chipped, apparently by geological pressure (Breuil 1910, p. 406).

In his report on the flints found in the gravel pit at Belle-Assise, Breuil (1910, pp. 403-404) stated: "From the fact that the flakes found in connection bore signs of retouching, it can be concluded that retouching, bulbed flakes, and blocks with conchoidal flake scars were produced here exclusively by compression within the interior of the soil. . . . If one attempted to reproduce, on an intact block of flint, either the retouching or the flaking, one would have to employ the processes of percussion and vigorous compression used in working stone."

This might lead one to wonder whether geological pressure was in fact the actual cause for the observed effects. A modern authority (L. Patterson 1983) stated that pressure flaking very rarely produces clearly marked bulbs of percussion. It is not apparent from Breuil's drawings how well developed the bulbs of percussion are on his specimens. Breuil (1910, p. 388) himself described the bulbs of percussion as only "more or less" clearly present. But if the bulbs are well developed, this would, according to Patterson's view, make it unlikely that they were produced by geological pressure.

In general, the bulb of percussion, as the name itself indicates, is taken as a sign of intentional percussive fracturing. But perhaps Breuil was correct in his supposition that geological pressure flaking could produce clear bulbs and retouching, like those found on implements made by

humans. In that case, no crudely chipped stone object should be recognized as a genuine tool unless found directly in contact with other unambiguous evidence of human involvement. Applying this standard across the board, one would have to reject numerous conventionally accepted stone tools, such as the many crude Oldowan tools of East Africa that were not found in the immediate vicinity of hominid fossils.

As we shall see, Breuil (Section 3.4.2), like S. Hazzeldine Warren In England (Section 3.3.7), found Eocene objects resembling not only crude eoliths but advanced tools of the Late Stone Age. Breuil and Warren nevertheless believed that all of these toollike specimens—the most sophisticated as well as the crudest— were the product of natural geological forces. This implies that even specimens resembling very good Paleolithic implements should not be securely identified as tools unless found along with definite signs of human habitation. Of course, if geological pressure can produce very good "tools," then even if such "tools" were found along with signs of human habitation, one could not tell if they were produced by nature or by humans. In order to satisfy skeptics like Breuil, it seems one would have to find even the best sort of implement clutched in the fossil fingers of a human hand.

But perhaps Breuil was wrong to suppose that geological pressure caused the bulbs of percussion on the many specimens he found in the Eocene at Belle Assise. His only evidence was the few bulbed flakes he found directly in contact with parent blocks of flint. Here we can refer to J. Reid Moir's explanation of the same phenomenon (Section 3.3.6). F. N. Haward had found flakes in contact with parent blocks of flint in the stone bed below the Norwich Crag. Haward said they were removed by geological pressure alone, but Moir suggested the following. Before the flints were covered by the deposit, intentional (presumably human) percussion caused the formation of incipient bulbed flakes, which were later completely removed from the parent blocks by geological pressure or heat.

In any case, taking Breuil's specimens as examples of pressure flaking, there is yet another problem to consider. It can be safely assumed that the specimens pictured by Breuil are among the better examples of flints found with flakes in contact with the parent block. But in studying the illustrations (Figures 3.20, 3.21), it is readily apparent that the flaking and retouching are extremely crude, far more so than that manifest on the other specimens of cores and flakes selected by Breuil as examples of pseudoeoliths (Figure 3.22).

Figure 3.23. A stone object discovered in the Eocene strata at Clermont (Oise), France (Breuil 1910, p. 394). It was characterized by Breuil as a pseudoeolith, produced by geological pressure. As evidence Breuil cited the presence in the same formation of detached flakes lying very close to

the parent blocks of flint (Figures 3.20, 3.21). But implementlike objects as sophisticated as the one pictured here were not found with detached flakes lying nearby. This raises serious doubts about the viability of Breuil's geological pressure hypothesis.

It seems, therefore, unfair to insist that the numerous better looking "pseudoeoliths" from the Eocene at Clermont, such as those shown in Figure 3.22, must have been formed by the same process of natural geological pressure flaking that had produced the extremely crude flakes.

But that is just what Breuil did in his report: "By means of this simple mechanical process, which one is able to perceive quite literally, there have nevertheless resulted the fractures, cleavages, terminal and marginal retouchings that simulate with extreme perfection the action of a voluntary agent with the preconceived intention of producing various elementary industrial artifacts, and, in exceptional cases, pseudomorphs of definite implements, not only eoliths" (1910, pp. 403-404).

This assertion does not, however, very easily follow from the examples presented by Breuil. He would have been justified

in making such a statement only if he could have pointed to examples of the better looking eoliths found in contact with the parent blocks. And this he did not do.

Also, some of the implementlike objects from the Eocene formation at Clermont were themselves whole pieces of flint, from which chip had been removed to form the working edge. The object depicted in Figure 3.23 provides a good example. The unidirectional chipping concentrated on the upper edge is typical of intentional human work. If Breuil had discovered the implement shown in Figure 3.23 with a dozen or more chips lying alongside the chipped edge, we might be less doubtful about his argument. But in the absence of such a demonstration, intentional human work remains a more viable explanation.

#### 3.4.2 "Two Truly Exceptional Objects" (Eocene)

The unsatisfactory nature of Breuil's geological pressure hypothesis becomes even clearer when we turn our consideration to what Breuil (1910, p. 402) called "two truly exceptional objects, of which the site of discovery, in the interior of the beds, is absolutely certain."

Describing the first object (Figure 3.24), which he characterized as a grattoir, or end scraper, Breuil (1910, p. 402) wrote: "The grattoir presents a blackish green patina, extremely brilliant, which is present on only a small number of small pieces of flint found in the sands."

The formation of patina occurs where the cortex, or rough outer surface of the flint, is chipped away, exposing the glassy interior to the atmosphere. Breuil (1910, p. 403) observed: "The great majority of the flints are without patination, and their fracturing occurred in the interior of the soil at undetermined times and places."

Breuil believed the presence of a brilliant patina on a small number of the flaked flints in the Eocene formation at Clermont meant they were fractured before they were incorporated into that formation. "Consequently," said Breuil (1910, p. 403), "it can be concluded that the fracturing of these flints occurred in pre-Eocene times." Therefore the pressure fracturing mechanism that Breuil used to explain the eolithlike objects at Clermont would not necessarily apply to the grattoir now under discussion.

In further describing the grattoir from the Eocene of Clennont, France, Breuil (1910, p. 402) observed: "Its plane of fracture shows a clear bulb of percussion; the other face shows fine and regular retouching, principally on the working edge, with a point at the apex, and on the left border."

Figure 3.24. This flint object was found by H. Breuil and H. Obermaier in an Eocene formation at Clermont (Oise), France (Breuil 1910, p. 402). Breuil said it was identical in form to certain Late Pleistocene implements, but he nevertheless considered it the product of natural geological pressure.

The chipping is less well-defined on the right side. This object is a veritable pseudomorph of an Azilium-Tardenoisian grattoir." Scientists generally attribute the Azilium-Tardenoisian stone implements to *Homo sapiens sapiens* in the Late Pleistocene of Europe.

Figure 3.25. A flint object found in an Eocene formation at Clermont (Oise), France (Breuil 1910, p.

402). Although H. Breuil said it resembled a Late Pleistocene pointed tool, he claimed it was formed by geological

pressure.

Breuil (1910, p. 402) then stated about the grattoir: "That it was discovered in place, at the base of the Eocene sands of Bracheux at Belle-Assise, is a cause of profound stupefaction." Indeed it is. We can see no justification for attributing the highly sophisticated flaking on this piece to the kind of crude pressure flaking exemplified by the few specimens cited above by Breuil. It thus appears that we are confronted with yet another example of a stone object displaying definite signs of intentional human work being found in very ancient strata, in this case over 50 million years old. Significantly, it was found by Breuil and Obermaier in person. So it seems that even these two stalwart eolith debunkers may have unwittingly discovered an anomalously old implement of advanced type.

Describing the second exceptional object (Figure 3.25), which he characterized as "another very curious pseudomorph," Breuil (1910, p. 402) wrote: "It is a very fine lamellar, or scalelike, flake, a little short, with, on its dorsal surface, multiple traces of longitudinal flaking, equally lamellar. At the point, the left side has some fine flaking on the dorsal surface; the other side shows fine chipping, like that produced by a burin. This object itself could be a micro-burin of Eyzies." Les Eyzies is a Late Pleistocene site in France. It would have been quite remarkable to find a piece like this as a flake in contact with the parent block, and with the chips taken from it lying next to it. But nothing remotely approaching this was reported by Breuil. The examples he did cite and illustrate were of the crudest sort possible, being essentially nothing more than randomly fractured pieces of stone.

It is quite remarkable that Breuil should have included two technologically sophisticated specimens, of Late Paleolithic type, in his report without recognizing they were sufficient to demolish his entire argument. He skipped right over them, apparently genuinely unaware of their significance. But objects exactly resembling implements of the Late Paleolithic type, especially when found in an undisputed Eocene stratum, should not be skipped over. We can only request the reader to carefully consider what damage the demonstrated presence of toolmaking human beings over 50 million years ago in France would do to all current evolutionary explanations of human origins and antiquity.

Of course, one can always insist that the two remarkable objects reported by Breuil were products of nature. In that case, one could dismiss any stone tools, including conventionally accepted Late Pleistocene tools, for the same reason.

### 3.4.3 An Attempt to Trap Rutot

After describing the finds he had made at Clermont, France, Breuil launched an attack on the Belgian scientist A. Rutot, who had found a series of crude stone tool industries during the first decade of the twentieth century (Section 4.4). Breuil (1910, pp. 404-406) wrote: "Is it possible to distinguish the real eoliths from those produced by nature? We have read, from the pen of Mr. Rutot [1906], that 'the recognition and appreciation of eoliths is not simple or elementary, as many persons believe. . . . It can be in certain cases very difficult to distinguish a pseudoeolith from a real one, just as the task of determining the difference between the closely related *Cerithes* and *Pleurotomes* is not easy to accomplish at first glance.' If Mr. Rutot were confronted with our flints from Belle-Assise, would he Judge them the work of an intelligent being, or simply curious and troubling pseudomorphs? Shown by Mr. Capitan a choice selection of our best specimens, Mr. Rutot, in the absence of information about their stratigraphic position, was willing to formulate his Judgement. He considered them to be so well fashioned as to belong to the transition from the Eolithic to the Paleolithic, the Strepayan, according to his system, the primitive Chellean in French usage. According to Rutot, certain specimens bear rudimentary traces of intentional work, as might be found in trial attempts.' In others 'the intentional work is of a much better character.' Another 'has been utilized as a scraper, of which it has the character.' Another long piece bears on its end attempts at work, for making a dagger or piercer.' Another is 'a very good *racloir* [side scraper], very well worn from use and retouched.' Another is 'a very good *gratoir* [end scraper], equally well worn from use and retouched.' Finally there is a very good 'throwing stone.' Mr. Rutot considers the morphology of the flints of Belle-Assise as characteristic of intentional work, surpassing the simple retouching of natural flakes found in eoliths, and marking the appearance of real intentional manufacture of definite tool types in the dawn of the Paleolithic. Shown the series collected by Mr. Commont, from both Belle-Assise and Picardy, Rutot gave the same diagnosis, though honestly acknowledging he had trouble with the Eocene age of such objects."

If one accepts Breuil's explanation that all of the specimens from Belle-Assise were formed by geological pressures, as demonstrated by a few examples of crudely chipped flakes found in contact with parent blocks of flint, then, of course, Rutot comes off very badly. One can only conclude that the unwitting Belgian geologist foolishly accepted naturally flaked flints as objects of human manufacture. But, as we have shown, Breuil's attempted explanation does not adequately account for all of the implementlike objects found in the early Eocene beds at Belle-Assise and elsewhere. Breuil (1910, p. 287) wrote: "Although parts of broken blocks of flint are frequently found still lying in close connection, this is not the rule, and one does not often find such cases, especially in the sand which is less compacted." It would thus appear that examples of flakes lying next to their parent blocks (Figures 3.20, 3.21) were not all that numerous. Furthermore, the flakes found in contact with the parent blocks did not very closely resemble the many other

specimens that Breuil called "pseudotools" (Figures 3.22, 3.23). In particular, the flakes in contact with parent blocks did not at all resemble the two Late Paleolithic type implements found at Belle-Assise (Figures 3.24, 3.25).

Therefore the assumption that all the specimens shown to Rutot were produced by natural forces is unwarranted. The presence of a few naturally broken flints at Belle-Assise does not rule out the possibility that many others, resembling implements, were in fact made by humans, especially since the latter category display more elaborate patterns of chipping than visible in the few specimens demonstrably broken by geological pressure. It is, therefore, quite possible that Rutot's judgements about the specimens shown to him by Capitan were entirely correct, and that Breuil had inadvertently been the discoverer of a new Eolith industry in the Eocene. Worthy of note is the fact that Rutot found signs of utilization on the edges of many of the specimens. The hypothesis that implemental shapes with signs of wear on the appropriate working edges could have been produced by blind natural forces will induce in at least some unprejudiced minds a sense of improbability.

#### 3.4.4 The Role of Preconception in the Treatment of Eolith Evidence

It can thus be seen that Breuil's main support was simply his unfounded belief that humans or protohumans capable of manufacturing even the crudest stone tools could not have existed in the Eocene. His view was shared by Hugo Obermaier. Many supporters of eoliths have pointed out that modern tribal people, such as the Australian aboriginals, make eolithlike implements. But Obermaier (1924, p. 16) protested: "If, then, from the actual [modern] eoliths we should draw the conclusion that, for the sake of consistency, similar forms from the Tertiary must also be considered as artefacts, we should find ourselves forced to admit the existence of man in Oligocene and perhaps even Eocene times. For these Tertiary products are in no way less 'human' than the corresponding modern forms, and must therefore presuppose similar cultural demands. Both Rutot in regard to Boncelles [Section 4.4], and Vel'vorn in regard to Cantal [Section 4.3], urge the point that the flints from these sites—which really do conform most admirably to the human hand—'appear to have been expressly made for it.' Well, the same is true of Belle-Assise!" It is obvious that Obermaier, like Breuil, was a prisoner of a belief that humans could not have existed in the Eocene. But this belief appears to have been arrived at independently of the available evidence.

Obermaier, citing the work of Max Schlosser, who studied fossil apes at Fayum in Egypt, further stated: "Viewed from the standpoint of palaeontology all this is untenable. The forms most closely related to the Eocene man of Clennont would be the *Pachylemurae* [lemurs]! The oldest known fossil anthromorph. the Oligocene *Propliopilhecus*, was probably no larger than a baby. No one can seriously believe [wrote Schlosser] 'that so small a creature could use such large stones as the eoliths. Neither could this be said of *Anthropodus*, which certainly did not attain the size of a twelve-year-old child. According to this, the theory of Pliocene eoliths must also be abandoned'" (Schlosser 1911. p. 56; Obermaier 1924, pp. 16-17). It should, however, be kept in mind that these statements were founded upon a carefully edited version of the fossil record that deliberately

excluded discoveries of fully human fossil skeletal remains in Pliocene, Miocene, Eocene, and even more ancient strata (Sections 6.2, 6.3). But even taking Obermaier's statements as they stand, they exhibit a questionable logic. Obermaier should not have absolutely ruled out the existence of humanlike primates in the Tertiary simply because the only primate fossils recovered up till that time were nonhumanlike.

### 3.4.5 The Double Standard in Operation

Seeing the eolith question from another point of view, Breuil (1910, p. 406) stated: "It is established that the criterion for distinguishing these natural productions from flints truly used by man, or flints rudimentarily worked by him, has not yet been discovered, and probably does not exist." Many authorities, from the nineteenth century up to the present, would disagree with this observation. The works of Leland W. Patterson (L. Patterson 1983, L. Patterson et al. 1987), outline a combination of criteria (including bulbs of percussion, retouching, striking platform geometry, repetition of particular forms, etc.) for judging human workmanship in even the crudest assemblages. Patterson (1983, p. 303) has stated: "Any experienced lithic analyst with a 10-power magnifier can distinguish fortuitously shaped flakes from unifacial tools."

Breuil (1910, p. 407) did, however, admit: "One is not able to conclude from the discoveries at Belle-Assise that there is no such thing as an Eolithic industry, no intentional work on natural stone flakes, no first manifestation of rudimentary tool types." He then stated that "in order to determine the presence of an intelligent being something more than calling attention to signs of adaptation is required, because the work of nature and that of human beings can be easily confounded. The objects should possess a degree of intentional work that is particularly clear, or should occur in an assembly of circumstances that rule out natural causes, or demonstrate, by the association of food debris or signs of fire, that human beings lived there" (Breuil 1910, p. 407).

But in many cases supporting evidence of the type specified by Breuil has been found in connection with stone implements. The stone tools discovered by Florentino Ameghino in an Early Pliocene formation at Monte Hermoso, Argentina, were accompanied by burned earth, remnants of hearths, burned and broken animal bones, and even human fossil remains, yet these implements were not generally recognized by the scientific community (Section 5.1.1).

Summarizing his case, Breuil (1910, p. 407) stated: "It is clear that we have here many pseudomorphs that show extreme signs of 'wear.' not only eoliths, but types truly recognized as Paleolithic, such as the marvelous small scraper in figure 67 [our Figure 3.24J. If nature, in exceptional circumstances without doubt, is able to produce objects that resemble advanced industrial types, perfectly defined and discovered in their normal geological position outside all possibility of error, there is thus very good reason to show caution regarding manifestations of the most elementary type of human activity, and to show great care before basing overambitious theories on such problematic findings. All this has been established in a definite manner and with all clarity."



This statement hinges on accepting Breuil's opinion that forces of nature are actually responsible for "types truly recognized as Paleolithic." Nothing in his report demonstrated that this is in fact true. As we have seen, the examples he gave of flints obviously broken in place (Figures 3.20, 3.21) do not compare very well with even the cruder "pseudomorphs" he collected at the Belle-Assise site (Figure 3.22). He also gave no real explanation for the highly organized chipping on the more advanced "pseudomorphs" (Figure 3.25). It would thus seem that Breuil himself was the one who was guilty of constructing overambitious theories on the shaky foundations of problematic findings.

#### 3.4.6 How Scientists Cooperated in Propagating Untruths about Eoliths

Breuil's paper was quite influential and is still cited today as proof that eoliths are natural rather than artificial productions. As an example of how Breuil's study was used shortly after it appeared, we can point to *The Origin and Antiquity of Man* (1912) by G. F. Wright, an American geologist. In a discussion of eoliths, Wright (1912, pp. 338-339) recounted how S. Hazzeldine Warren had shown that cart wheels rolling on gravel roads produced chipped flints like eoliths, and how Marcellin Boule had collected chipped flints resembling eoliths from machinery used for the production of cement. Wright, after lamenting that some scientists, like Rutot, were still promoting eoliths, wrote: "Within the year past, however, Abbe Breuil has apparently been able to give a finishing touch to the evidence discrediting the artificial character of the eoliths. We will content ourselves with quoting the summary of this evidence given by Professor Sollas" (Wright 1912, p. 340).

Wright then quoted from *Ancient Hunters* by W. J. Sollas (1911, pp. 67-69): "These [eoliths] were found by the Abbe Breuil in Lower Eocene sands (Thanetian) at Belle-Assise, Clermont (Oise). M. Breuil shows in the most convincing manner that they all owe their formation to one and the same process, i.e. to movements of the strata while settling under pressure of the soil. The flint nodules crowded together in a single layer are thus squeezed forcibly one against the other, and flaking is the inevitable result. . . . In many cases the flakes are still to be found in connection with the parent nodule, lying apposed to the surface from which they have been detached." Wright published a reproduction of Breuil's drawing of some very crude flakes lying next to parent blocks of flint. Sollas had used the same drawing in his book. As we noted in our previous discussion, the degree of "workmanship" on the flakes pictured in these drawings (Figures 3.20, 3.21) hardly approaches that of even the crudest of eoliths.

The quotation from Sollas (1911) about Breuil's pseudoeoliths continued: "They display just the same forms as other Tertiary 'eoliths,' ranging from the obviously purposeless to those which simulate design and bear bulbs of percussion and marginal retouches. Among the most artificial looking are a few which present an astonishing degree of resemblance to special forms of genuine implements; attention may be directed to two in particular, which are compared by the Abbe Breuil, the one to Azilio-Tardenoisian flakes, and the other to the small burins of Les Eyzies: in their resemblance to artificial forms these simulacra far transcend any 'eoliths' which have been found on other horizons of the Tertiary series" (Wright 1912, p. 341). Sollas implied that Breuil found at Clermont examples such as these last two, with flakes in place. There is, however, a little

dishonesty In this presentation. Sollas should have mentioned that although some pieces of flint were found with flakes lying nearby, these were, although displaying, in some cases, bulbs of percussion and secondary chipping, decidedly nonimplemental in character. Of course, most of the blame lies with Breuil, who wrote the original report.

Sollas concluded: "On the important question of man's first arrival on this planet we may for the present possess our minds In peace, not a trace of unquestionable evidence of his existence having been found in strata admittedly older than the Pleistocene" (Wright 1912, pp. 341-342). This view is still prominent today, although there are hundreds of discoveries, a good many of which are discussed in this book, that invalidate it.

The case of Wright and Sollas shows how researchers who share a certain bias (in this case a prejudice against evidence for Tertiary humans) cooperate by citing a poorly constructed "definitive debunking report" (in this case by Breuil) as absolute truth in the pages of authoritative books and articles in scientific Journals. It Is a very effective propaganda technique. After all, how many people will bother to dig up Breuil's original article, In French, and, applying critical intelligence, see for themselves if what he had to say really made sense?

#### 3.4.7 Breuil Supports Moir

It is Interesting to note that Breuil's "definitive" 1910 report came before most of J. Reid Moir's discoveries in East Anglia. Eventually, when Moir's finds began to attract considerable attention, Breuil, and other scientists, went to England to conduct firsthand evaluations. Surprisingly enough, Breuil backed Moir.

M. C. Burkitt (1956, p. 107) wrote: "Messrs Breuil and Boule, who came over to see the finds, still maintained their skeptical attitude. Mr. Moir, however, was undaunted and continued his researches at new sites until finally at Foxhall, a few miles from Ipswich, he collected a series of specimens of such a nature that an examination of them by M. Breuil caused him to change his ideas completely and to Join the ever-growing company of those prehistorians who believed In the existence of man as early as late tertiary times."

It Is noteworthy that such a conservative and cautious researcher as Breuil should have come out In favor of Moir. During his visit to England, Breuil had specifically searched Moir's sites for any evidence of soil movement and pressure. But he found none. George Grant MacCurdy, director of the American School of Prehistoric Research in Europe, wrote in *Natural History*: "Breuil is authority for the statement that conditions favoring the play of natural forces do not exist in certain . . . deposits of East Anglia, where J. Reid Moir has found worked flints" (MacCurdy 1924b, p. 658).

Some of these deposits are found in the middle of the Red Crag at Foxhall. About Foxhall, Breuil (1922, p. 228) stated: "There is a twin layer in the superior part of the Red Crag, representing without doubt land surfaces that temporarily emerged shortly before the final retreat of the sea during the upper Pliocene." As we have seen, modern authorities still place the Red Crag in the

Late Pliocene (Section 3.3.2). Breuil (1922, p. 228) added: "Here there are no causes of natural mechanical fracturing—no rolling, no scraping, no contusion, no flints found in great quantities of stone. The flints are scattered, not numerous, have sharp angles, and are small in size, just as occurs in a level where the products and byproducts of lithic industry are present. The signs of intentional flaking are very well defined, and one also finds waste products of such flaking. One finds flint cores. Bulbs of percussion are very certain. One finds the same types as at the base of the Red Crag [in the sub-Crag detritus beds]. Furthermore I have noted instances of parallel successive flake removal."

Moir himself (1924, p. 647) informs us that Breuil "definitely accepted the view that the sub-Crag implements were made by man." In 1922, after visiting sub-Crag sites at Thorington Hall and Bramford, Breuil (1922, p. 228) wrote: "the level in which the flints are found represents a land surface that existed prior to the invasion of the Red Crag seas, which occurred in the upper Pliocene, bringing in a fauna adapted to the cold. There certainly does exist cause for mistaken identification of implements, such as intense compression of the soil, which, by means of mechanical action, many times produced examples of flaking and fracturing, including bulbed flakes, with edges showing chipping resembling retouching and signs of utilization. Nevertheless, there are some flint specimens that bear very well-defined bulbs of percussion, manifesting patterns of flaking that could only be obtained by removing successive flakes by repeated blows in the same direction. This flaking oftentimes gives the appearance of retouching, and absolutely resembles flaking of human origin. I am not aware of any action of compression that could produce these results. The mechanical action of rivers or the sea can also be eliminated as causes, as can thermal action. There are some flints that show evidence of having been burned. I reject the majority of rostracarnates [a type of eolith] as not being the product of intentional work, but I do accept as the true product of intentional work an important number of specimens. These are not simply eoliths but are absolutely indistinguishable from classic flint implements. "

Breuil's statement that some of the objects from below the Red Crag were "absolutely indistinguishable from classic flint implements" is highly significant. The sub-Crag formations, which lie between the Late Pliocene Red Crag and the Eocene London Clay, could be anywhere from 2 to 55 million years old. We thus have a situation analogous to that at the Belle-Assise site in France, where Breuil found in Eocene formations two "pseudomorphs" resembling classic Paleolithic implements of the Late Pleistocene. In the case of the sub-Crag implements Breuil stated he was "not aware of any action of compression that could produce these results." This differed from the position he took regarding the two specimens from the Eocene of Belle-Assise, namely, that they were produced by geological compression. Breuil's views about the authenticity of some of Moir's implements nevertheless add considerable weight to the conclusion that the objects found at Belle-Assise were also the product of intentional human work rather than geological compression. One wonders why, if Breuil was prepared to accept the sub-Crag objects were manufactured by humans, he did not change his views about the two objects found at Belle-Assise.

Breuil, once an avid supporter of Moir's finds, apparently became noncommittal later on. In a late edition of *Men of the Old Stone Age*, published posthumously, Breuil and Raymond Lantier (1965, p. 56), in considering the Crag specimens, stated only that "traces of fire and a certain number of flakes might be accepted, though their angle of cut is generally against it." One wonders why there is no mention of the objects Breuil (1922, p. 28) previously said were "not simply eoliths but are absolutely indistinguishable from classic flint implements."

#### 3.4.8 Barnes and the Platform Angle Controversy

Another important element in the eolith controversy was the platform angle test, promoted by Alfred S. Barnes. Barnes, who defended Moir against attacks by Haward and Warren in the 1920s, later became opposed. In 1939, he delivered what many authorities still regard as the death blow to the Red Crag and Cromer Forest Bed tools. But Barnes did not limit his attention to East Anglia. In his study, titled "The Differences Between Natural and Human Flaking on Prehistoric Flint Implements," Barnes (1939, p. 99) considered stone tool industries from France, Portugal, Belgium, and Argentina, as well as those of Moir.

Supporters of the view that implements from the above sites were of human manufacture generally argued that natural forces could not produce the kinds of chipping observed on the objects in question. Barnes admitted that random concussion would not produce effects such as regular, unidirectional chipping along a single edge. He also felt that simple pressure from overlying beds, as proposed by Breuil (Section 3.4.1), was also not a very satisfactory agent, because it did not produce specimens with good striking platforms or clearly marked bulbs of percussion (Barnes 1939, pp. 106-107). But Barnes went on to give some examples of natural forces that, in his opinion, were capable of producing objects resembling eoliths. He called attention to some flints collected from the Blackheath Eocene marine beds at Stanstead in Surrey. At this site, by a process called foundering, flint nodules had descended 20 to 40 feet into cavities eroded in the chalk, where they were crushed by masses of large pebbles from the overlying beds. Some chipped flints were found lying in contact with the parent blocks (Barnes 1939, p. 103).

Besides foundering, another natural force that could, according to Barnes (1939, p. 106) and others, produce eolithlike specimens was solifluction, in which a large mass of frozen gravel thaws and then flows rapidly down a slope.

Barnes admitted that judgements based on simple visual inspection of chipping thought to have been caused by foundering or solifluction were liable to be very subjective. So he proposed that attention should be focused on some measurable feature of the implements that could be objectively evaluated. For this purpose, Barnes chose what he called the "angle platform-scar."

Barnes (1939, p. 107) explained: "It may be said of natural fractures in general that some really good pseudomorphs of human work may be found. but when a number of specimens are examined, examples of aberrant flaking will be present. These aberrant flakes either serve no useful purpose in connection with the supposed tool or occur in positions where they would not be found in human work, or present angles platform-scar which are obtuse. The angle platform-

scar is the angle between the platform or surface on which the blow was struck or the pressure was applied which detached the flake. and the scar left on the tool where the flake has been detached. ”

We find Barnes's description of the angle to be measured somewhat ambiguous. We have spoken with experts in lithic technology at the San Bernardino County Museum, Including Ruth D. Simpson, and they have also been unable to specify exactly what angle Barnes was measuring.

In any case, In the angle platform-scar, Barnes believed he had found the objectively measurable feature by which one could distinguish natural chipping from human work. However. as noted later in this section, modern authorities such as Leland W. Patterson have extensively critiqued Barnes's methodology.

Barnes (1939, p. 109) made these observations: "When we examine the tools of Paleolithic man we find that they are furnished with acute edges (less than 90 degrees) for cutting and scraping, for such edges are more effective for these purposes than edges with obtuse angles (90 degrees and over). There is a further reason why on humanly made tools we find that the majority of angles platform scar are acute and that is because the tool maker must be able to control the flakes he removes. ... In the author's experience of making flint implements he finds that for satisfactory control of the flaking the angles platform-scar lie between 20 degrees and 88 degrees. ”

In order to be effective, the measurement had to be applied not to a single specimen, but to a large sample of specimens from the industry In question. Barnes (1939, p. 111) stated that a sample "may be considered of human origin If not more than 25% of the angles platform-scar are obtuse (90 degrees and over)." Having established this, Barnes (1939, p. III) delivered a devastating conclusion: "None of the eoliths examined by the author . . . (Pre-Crag Suffolk, Kent, PuyCourny, Belgium, etc.) . . . comply with the criterion and therefore they cannot be considered to be of human origin."

Interestingly enough, it appears that Molr himself was aware of the Barnes criterion and believed his specimens were within the required range. In 1935. four years before Barnes came out with his report, Moir analyzed his own specimens in terms of angles. He first noted that flint Implements "are all, of necessity, made upon the same general plan," utilizing "a more or less flat striking-platform in the production of the implements" (Moir 1935, p. 355). He then decided to examine "the angle of the secondary edge-flaking exhibited by a series of pre-Crag Implements, a factor largely under the control of the flint flaker" (Molr 1935, p. 355).

The term "secondary edge-flaking" appears to refer to flakes removed from the edge of a selected piece of naturally broken flint In order to fashion it Into an implement. Although one cannot say so with absolute certainty, the angle of this secondary edge flaking apparently corresponds to the "angle platform-scar" of Barnes. Molr (1935, p. 355) noted "Professor A. S. Barnes was the first to draw attention to the significance of such measurements of flint implements."

Molr (1935, pp. 355-356) then gave the results of his study: "A quantity of pre-Crag implements to the number of 181, composed of 55 specimens of Group No. 1, 55 specimens of Group No. 2, 13 specimens of Group No. 3, 55 specimens of Group No. 4, and 3 specimens of Group No. 5, were measured with the following results. It was found that the average angle of edge-flaking of Group No.1 was 88° degrees, of Group No. 2, 75½ degrees, of Group No.3. 82 degrees, of Group No. 4. 79 degrees and of Group No. 5, 69 degrees."

From these average figures alone we cannot verify that Moir's samples met Barnes's statistical requirement that at most 25 percent of the measured angles in each group exceed 90 degrees. But the angles Moir measured clearly tended to be acute, and he believed his tools satisfied Barnes's requirement.

Nevertheless, Barnes believed he had demolished, in his brief 1939 report, every anomalously old stone tool industry found by scientists over the previous 75 years. For Barnes, and almost everyone else in the scientific community, the controversy was over. But factually speaking, Barnes was beating a dead horse, because the controversy about the eoliths and other Tertiary stone tool industries had long since ceased to be a burning issue. With the discoveries of Java man and Peking man, the scientific community had become increasingly convinced that the key transition from apelike precursors to toolmaking humans (or protohumans) had taken place in the Early to Middle Pleistocene, thus making the lithic evidence for Tertiary humans a sideshow topic of little serious concern. Barnes, however, could be seen as performing the valuable, if menial task, of sweeping away some useless remnants of irrelevant evidence. Thereafter, whenever the topic of very old stone tool industries happened to come up, as it still does from time to time, scientists could conveniently cite Barnes's report. Even today scientists studying stone tools apply the Barnes method.

Barnes's 1939 paper is typical of the definitive debunking report, which can be conveniently cited again and again to completely resolve a controversial question, making any further consideration of the matter superfluous. But on close examination, it appears that Barnes's definitive debunking report may be in need of some debunking itself.

Alan Lyle Bryan, a Canadian anthropologist, recently wrote (1986, p. 6): "The question of how to distinguish naturefacts from artifacts is far from being resolved and demands more research. The way the problem was resolved in England, by application of the Barnes' statistical method of measuring the angles of platform scar, is not generally applicable to all problems of differentiating naturefacts from artifacts." During a phone conversation with one of us on May 28, 1987, Bryan stated that application of the Barnes criterion would, for example, eliminate any blade tools struck from polyhedral cores. He also expressed a cautious belief that Barnes may have gone too far in trying to eliminate all of the anomalous European stone tool industries. Giving attention to more recent discoveries, Bryan said that Peter White has shown there are Late Pleistocene Australian tools that do not conform to Barnes's specifications.

An example of an industry that apparently does not conform with the Barnes criterion is the Oldowan, from the lower levels of the Olduvai Gorge. At site DK at the bottom of Bed I, 242 whole

flakes were recovered. A striking platform angle could be measured on 132 of these. Mary Leakey (1971, p. 39) recorded the following results:

70-89° 90-109° 110-129° 130°+4.6% 47.7% 46.2% 1.5%

As can be seen, over 95 percent of the angles are obtuse. However, It is not clear from Leakey's report exactly which angle was being measured. We discussed this with Ruth D. Simpson and her colleagues at the San Bernardino County Museum of Natural History, near Redlands, California. They were also unable to tell from Mary Leakey's report exactly what angle was being measured. This is a general problem that we have encountered in our review of angle studies on stone tool industries. The vagueness of the descriptions of the angles being measured by various investigators makes it difficult to compare findings and calls into question the scientific usefulness of such reporting.

As far as the implements from Olduvai are concerned, if the angle being measured was the angle used by Barnes, or an equivalent angle, then the Oldowan industry, although universally accepted, does not meet the Barnes criterion. Considering the extremely crude nature of the objects, which Louis Leakey said were comparable to Moir's implements, It is remarkable that they have never been subjected to the slightest challenge by the scientific community. This is probably because the Oldowan industry offers support to the African evolution hypothesis of human origins, which is accepted as dogma.

During the 1950s, the Barnes method was criticized by George F. Carter, who had discovered crude stone implements at various sites in the San Diego area, principally at Texas Street. The tools, mostly pebble choppers and quartzite flakes, were referred to the last interglacial. They were assigned dates of about 100,000 years, which violates the currently accepted idea that humans entered the Americas no more than 30,000 years ago, with most authorities adhering to a more conservative figure of approximately 12,000 years.

Reacting to attempts to dismiss the tools by the same methods used to reject the European eoliths, Carter (1957, p. 323) stated: "Comparison of the San Diego County material with that of Europe has severe limitations placed upon it that seem to have been missed by some people. The lithic materials are extremely different—quartzite and porphyries in California versus glassy rocks of the flint family in Europe. There is no frost action of solifluction or any related phenomenon in the San Diego area now nor was there any during the Pleistocene. There is no limestone area to founder and produce pressures."

Specifically referring to the Barnes method, Carter (1957, p. 329) noted: "Clearly, many of the usual criteria for Judging the human authorship of stonework do not apply to such a tradition. Regrettably this seems to apply especially to the platform- angles method of testing which was so useful in distinguishing between human and natural work in England. Barnes' (1939) platform- angles on a bifacially flaked tool are much lower than 90 degrees. Those on flakes and cores of an industry such as that of Texas Street are normally about 90 degrees. It should not be overlooked that plano-convex tools normally have high platform angles." Plano-convex tools are those that are

flat on one side and convex on the other. So here we have another example of an industry that was accepted (at least by Carter and his supporters) as being of human manufacture and that does not conform to the Barnes criterion.

In the preceding paragraphs, we have reviewed a number of stone tool industries that appear to be exceptions to the criterion proposed by Barnes. If these industries can be considered exceptions, then why not any or all of the various Eolithic industries that Barnes rejected?

Leland W. Patterson, the principal author of a recent study on the stone implements discovered at the Calico site in California, has also examined the application of the Barnes method. At Calico, stone objects believed to be of human manufacture have been found in strata dated by uranium series analysis to about 200,000 years before the present. They are, therefore, like the Texas Street implements, highly anomalous. We shall discuss these and similar finds relating to the human settlement of the Americas more fully in Section 3.8. For now, we shall confine ourselves to studying the application of the Barnes method to the Calico specimens, which are quite similar to Eolithic implements.

Barnes angle measurements were used by L. A. Payen (1982) to dismiss the Calico specimens. But L. Patterson and his coauthors (1987, p. 92) believed that measurement of Barnes's angle was not suitable for this purpose. Patterson defined the Barnes angle, or beta angle (Figure 3.26), as "the angle between the ventral surface and the platform plane" (L. Patterson et al. 1987, p. 92). Patterson, however, preferred to measure the striking platform angle, which he defined as the angle between the dorsal surface of the flake and the platform plane (Figure 3.26).

Patterson observed: 'For general lithic analysis, the striking platform angle is a better attribute than the 'beta' angle . . . because prominent bulbs of force on ventral surfaces of flakes can frequently interfere with 'beta' angle measurement" (L. Patterson 1983, p. 301).

When Patterson and his coworkers measured striking platform angles rather than beta angles, their results differed from Payen's: "Acute platform angles were found on 94.3% of the Calico flakes with intact platforms as compared with 95.5% of the experimental sample. The average platform angle of the Calico flakes was 78.7%, with a standard deviation of 8.3%. This is consistent with the usual products of intentional flaking" (L. Patterson et al. 1987, p. 97).

Figure 3.26. (1) The Barnes, or beta, angle, measured on a stone core. (2) The Barnes, or beta, angle, measured on a flake detached from the stone core. (3) L. Patterson's striking platform angle, measured on a detached flake.

Why such a difference from Payen's findings? Patterson and his coauthors stated: "A question can be raised as to the nature of Payen's sample. Only specimens that are candidates for representation as products of controlled flaking should be subject to analysis of platform



geometiy. A large amount of analytical 'noise' can be introduced by analyzing miscellaneous specimens of broken stone that possibly are not the result of controlled flaking. It is common in many lithic industries to find large quantities of non-diagnostic broken stone that are not the products of controlled flaking" (L. Patterson et al. 1987, p. 92). This might be true of some of the anomalously old European stone tool sites.

From Barnes's report, it appears that he was measuring mainly secondary flake scars on possible implements. He said that he would measure 100 angles, from about 30 tools. Thus he would measure an average of 3.33 angles per object. As far as eoliths are concerned, they are mostly natural flint flakes or blocks that have been subjected to some limited intentional retouching. So they should have both intentional and natural flake scars. If Barnes randomly picked 3 flake scars per eolith for his measurements, it is quite possible that this would introduce enough obtuse angles to violate his requirement that no more than 25 percent of the measured angles should exceed 90 degrees.

Patterson and his coauthors (1987, p. 92) then stated: "Another source of error in the analysis of striking platform geometry is the confusion of secondary planes with true residual striking platforms on flakes." Patterson (1983, p. 301) had earlier pointed out: "In collections both of man-made and naturally fractured stone . . . Barnes identified many specimens with flake-scar angles greater than 90 degrees. These observations must result either from incorrect identification of striking platform geometry or from incorrect angle measurements, if man-made controlled flaking or simulated controlled flaking by nature is being identified. Core flake-scar edge angles, and corresponding 'beta' angles on product flakes, cannot be obtuse in controlled flaking. On a flake, the striking platform and 'beta' angles are most often incorrectly identified when a secondary fracture has removed the true residual surface of the striking platform and has left another flake scar surface which gives the incorrect impression that these angles are obtuse. It must be emphasized that intact examples of controlled flaking will have striking platform and 'beta' angles under 90 degrees. . . . Studies such as that published by R. E. Taylor and L. A. Payen that use

'beta' angles on flakes as the basis for concluding that the sites of Calico and Texas Street do not have man-made specimens are questionable for the reasons given here."

Further emphasizing this fundamental flaw in the Barnes method, Patterson (1983, pp. 301-302) stated: "Previous investigators have obtained the impression that collections of naturally produced lithic flakes have many striking platforms with obtuse angles, but this appears mainly to be a case of incorrect identification of striking platform geometry. . . .

Collections of naturally fractured rock often superficially appear to have a high percentage of flakes with striking platforms that have obtuse angles simply because so many residual striking platforms are missing and secondary fracture planes are incorrectly identified as remnant striking platforms."

Thus even collections of naturally broken stone should satisfy the Barnes criterion, if the original striking platform angles can be properly identified. It would thus appear that the method devised

by Barnes is not appropriate for distinguishing between the effects of natural forces and intentional human work on pieces of stone.

"Probably the greatest problem with the Barnes method," observed Patterson, "is that it considers only a single attribute, and It is very difficult to conclusively demonstrate the presence or absence of human workmanship in that manner" (L. Patterson et al. 1987, p. 92). In another paper, Patterson gave some guidelines for a more suitable method of determining whether or not flaked stone objects are of human manufacture: "Demonstrate the likelihood of human manufacture by combinations of key attributes. Studies of single attributes will always remain unconvincing" (L Patterson 1983, pp. 298-299).

Among the key attributes that Patterson suggested were the presence of clearly marked striking platforms (especially those modified for better flaking), multiple examples of tool types, platform angle measurements, the presence of bulbs of percussion and associated ripple lines, and the geological context. Other attributes that might be considered are the presence of regular retouching, sharp edges (nature tends to produce rounded edges), and signs of parallel flake removal. This balanced approach is typical of the methodology applied by the original discoverers of the stone tool industries discussed in the preceding pages.

Let us now consider in greater detail some of the key attributes identified by Leland W. Patterson and others. Patterson considered the bulb of percussion to be the single most important identifying factor. With regard to Calico, Patterson and his coauthors stated: "Of the 3,336 flakes from five Calico units, 26.1% had force bulbs and were classified as diagnostic flakes. In the experimental knapping project, using hard percussion, 24.3% of the 473 flakes possessed force bulbs and were classified as diagnostic flakes. By comparison, flakes produced by mechanical crushing (pressure force) usually have a very low percentage of distinguishable force bulbs, as shown by samples of flakes from mechanical gravel crushers" (L. Patterson et al. 1987, p. 95).

Patterson (1983, p. 300) also pointed out that percussive fracturing tends to produce prominent ripple lines radiating from the impact point, whereas pressure fracturing produces finer ripple lines. In addition, percussion fracturing can result in the presence of *erraillures*, small chips removed from the ventral surface of the force bulbs.

Patterson stressed the bulb of percussion, force ripples, and *erraillures* as very important in making an identification because stone flaking by humans almost always involves percussive techniques, whereas naturally broken stone is generally the result of pressure flaking. Patterson and his coauthors stated: "To date there is no documented situation where natural forces have produced large concentrations of percussion made flakes" (L. Patterson et al. 1987, p. 96).

In some controversial cases, Patterson has suggested that "the geological context of a lithic collection becomes important in determining if nature would have had the probable capability of fracturing rock, especially in a percussive manner" (L. Patterson 1983, p. 299). He added: "The only published manner that nature can do much percussive fracturing is under high-energy, ocean-beach storm conditions. . . . viscous liquids and slurries inhibit high-velocity percussive interactions

of rocks. Pressure fracturing gives different lithic attributes than the percussive-type flaking used by early man. . . . Another condition in which nature can breakrock is when flint nodules are held in a secure limestone matrix and there is a shift in the mass. Here, it is common to see shear fractures that have none of the key attributes of percussive fracture patterns" (L. Patterson 1983, p. 299). Here we see that Patterson, in common with the original proponents of many early stone tool industries, believed that it is possible to clearly distinguish natural pressure fracturing from that caused by intentional percussion flaking techniques.

In regard to tool type analysis and distribution patterns, Patterson commented: 'Even if nature can produce lithic objects resembling simple man-made items, nature is not likely to do this often. Therefore, the frequency of occurrence at a given location of specimens with similar morphologies is important in demonstrating probable manufacturing patterns. Production of numerous lithic specimens with consistent morphology is certainly not a habit of nature. Quantitative data on amounts of each specimen type should therefore always be presented" (L. Patterson 1983, p. 298).

Patterson warned against the type of purely speculative interpretation often encountered in the writings of critics of anomalous lithic industries. An example would be Warren's suggestion that grounding icebergs were responsible for Moir's specimens. Patterson stated: 'Even the personal opinion of a lithic expert is of little value if explicit technological reasons cannot be given to explain an opinion, either positive or negative. . . . The comments of [C. V.] Haynes on the Calico site lithic collection are a good example of subjective comments, without consideration of specific lithic attributes that could distinguish man-made manufacturing patterns. A list is given of ways that stone could fracture from natural causes, and then an opinion is given that the Calico lithics are the result of natural fractures, without presenting any detailed specific qualitative and quantitative studies of the attributes of the lithic materials in question. This type of subjective discussion should be avoided, as it unduly influences general opinion without any real basis" (L. Patterson 1983, p. 298).

In light of the views presented by Bryan, Carter, and Patterson, it is clear that wholesale rejection of the Eolithic and other early stone tool industries by application of the Barnes criterion is unwarranted. As a rule, the proponents of the anomalously old industries appear to have reached their conclusions by sounder analytic techniques than the opponents of such industries, whose objections mainly take the form of suggesting, with inadequate supporting evidence, various ways in which natural forces, principally pressure flaking, could have produced the objects in question.

So what are we left with? At this stage in our review of ancient stone implements, we find that we have some very credible reports, by reputable scientists, of stone tool industries dating well back into the Tertiary epoch. We should, however, point out that our investigations, although thorough, are by no means complete. In the course of our research, which we can only characterize as a preliminary survey, we have had to leave many leads unpursued (Eolithic Industries from Tunisia, Egypt, etc.). We fully expect that future editions of this book will contain increasing numbers of

authenticated examples of very ancient stone tool industries, as they come to our attention either in the course of our own Investigations or through submissions by others.

### 3.5 Cement Mill Eoliths?

From the late 1800s to the present, some scientists have challenged the human manufacture of eoliths and other crude stone implements, claiming that flakes of flint just like them are produced by machinery at cement factories. Alfred Russell Wallace wrote to Benjamin Harrison on June 8, 1907: "I suppose you know that a considerable number of eoliths have been found recently on the high gravels of the New Forest, near Fordingbridge, by Mr. Westlake and others. But the most important thing recently is the attack on the human origin of eoliths by the production during some process of crushing flints on the Continent of forms which are alleged to be /denial with those of the eoliths in every detail. Opinion seems to be strongly divided, but I have seen no really careful judgement after close comparison. Have you seen them? Can you not get a set of them in exchange for yours, and give us a careful comparison? That would be worth while" (E. Harrison 1928, p. 278).

"Harrison was alive to the challenge to the eoliths arising out of the alleged resemblances of battered mill-made specimens of crude implements," observed Sir Edward R. Harrison (1928, p. 278). "He visited several brickyards and cement works in order to examine the stones that had been struck by the revolving rakes of the machines, and came away convinced that the chipped stones so produced were distinguishable from the typical Kent eoliths."

In one of his notebook entries for the year 1907, Harrison wrote of a visit to a cement mill: "Had over an hour's search on the waste heap, but could find no 'eoliths.' Two bulbed flakes found. One or two stones, having been accidentally rehit near the same place, bore some resemblance to poor eoliths, but still with a difference" (E. Harrison 1928, p. 275).

The charge that stones randomly chipped in mills resembled crude tools was also made in regard to other Eolithic industries in England and elsewhere in Europe. In 1905, Hugo Obermaier, with A. Laville, M. Boule, and E. Cartailhac, visited a chalk mill at Guerville, near Mantes, close to the Seine. Obermaier (1924, p. 11) wrote: "These mills consist of tanks filled with water, in which lumps of chalk with flint nodules embedded in them are rapidly rotated. In order to separate these nodules from the chalk and to pulverize the latter, chalk-lumps and water are subjected by means of turbines to a centrifugal motion of four meters [thirteen feet] per second. . . . The eoliths produced by the chalk mills, equally with those found in river deposits, showed forms with either partial or entire retouch around the edges, notched edges more or less deeply incurved, specimens that might be classed as scrapers, burins, and even planing tools."

But a modern expert, Leland W. Patterson, has pointed out a method for distinguishing between random natural chipping on edges of stone and intentional human chipping. Patterson (1983, p. 304) stated: "Lithic objects in nature are generally free to move or are loosely held by surrounding materials. Randomly applied forces under this condition will tend to be very oblique to the edge of the flake. Fractures then occur transversely to flake edges in the direction of least mass

resistance." This kind of random chipping quickly removes sharp edges from flakes of stone. Furthermore, the chip scars tend to be of various sizes, rather than uniform in size, and tend to be oriented in many directions, rather than in a single direction.

Patterson's own studies of crushed gravel from cement factories demonstrated: "In crushed gravel there are few objects that resemble man-made cores. There are also no long sections of flake edges with uniform, unifacial retouch" (L. Patterson 1983, p. 306). Eoliths and other early stone implements, it may be recalled, are characterized by unifacial retouch— chipping confined to one side of a sharp edge.

It thus seems that a careful student of lithic technology would be able to offer a response to the challenge by Obermaier, who believed running water was a better explanation of eoliths than human action. One might ask if any pieces with sharp edges were found at the chalk mill at Mantes? Obermaier said he saw "sharp edged types, and others in which the edge had been completely worn away." He observed, "The sharp-edged types resulted after remaining in the mill from eight to ten hours, the others after a longer time in the water" (Obermaier 1924, p. 11). This evidence supports Patterson's observation that random natural action tends to quickly wear away sharp edges, making it probable that sharp-edged Eolithic specimens with regular unifacial retouch were manufactured by human beings. Rapidly running water does not produce such effects.

Obermaier, however, tried to overcome this difficulty by proposing mechanisms that would result in only brief random percussive action on flints, a few hours over the course of perhaps millions of years. Here, as many times previously, we find a scientist eager to discredit unwelcome discoveries moving into the realm of extremely improbable special explanations. Obermaier referred to a deposit of Quaternary eoliths discovered by P. Wernert and R. R. Schmidt at Steinheim in the valley of the Stuben, near Württemberg, Germany. Wernert and Schmidt stated: "We were able to show at the site itself how the fragments of flint were borne along by the stream in the principal valley and suddenly drawn into whirlpools caused by the inflow of a tributary stream. By this means the flints were subjected to a strong rotary movement which, however, was limited and intermittent in action, and therefore did not result in such continuous wearing away as would transform the flints into rounded pebbles" (Obermaier 1924, pp. 11-12).

Even if the whirlpool explanation is granted, application of Patterson's method of analyzing edge damage should result in identification of these specimens as the product of random natural forces rather than intentional human work. In fact, Obermaier himself (1924, p. 12), reported that A. Rutot, who discovered a famous series of crude stone implements in Belgium (Section 4.4), visited the German site in 1911 and pronounced the objects found there to be "pseudoeoliths." Even a supporter of eoliths was apparently not as eager to see a human implement in every piece of broken stone as his opponents might have believed. He was able to distinguish a pseudoeolith produced by natural forces from eoliths of human manufacture.

Rutot's own specimens were more sophisticated than Harrison's eoliths. They were, nevertheless, sometimes called eoliths by authors who applied the term to almost any anomalously old and relatively unrefined tools. In the course of the debate about whether or not Rutot's specimens

were made by humans, the Gennan scientist H. Hahne concluded they were distinct from machine-chipped rocks. In his book *Human Origins: A Manual of Prehistory*. George Grant MacCurdy, a professor of prehistoric anthropology at Yale University, wrote (1924a, pp. 91-92): "After a careful comparison of machine-made eoliths from both Mantes and Sassnitz with eoliths from Belgium, Hahne's conclusions are as follows: (1) the chalk-mill flints are all scratched and otherwise marked by the iron teeth of the mill; (2) the sides of all the larger pieces are bedecked with scars from blows that were not properly placed to remove a flake; (3) almost every piece shows more or less of the original chalky crust of the nodule; (4) anything like a systematic chipping of an edge or margin is never found, except for a very short stretch, where one would expect it to be carried along the entire margin; this is quite different from the long retouched margins of most eoliths; (5) the same edge is often rechipped first on one side and then on the other, absolutely without meaning or purpose (the 'reverse working' of true eoliths is quite another thing); (6) In the product, coarse chipping alternates with fine retouches along the same margin, while on the eolith there is a regularity and orderly sequence of chipping; (7) the repeated rechipping of the same edge, while others are left untouched, does not occur in machine-made eoliths; (8) the chief difference is between the haphazard and meaningless on one hand, and the purposeful on the other. The most prominent and easily breakable parts suffer most in passing through the mill. They are often retained intact, or only slightly altered to serve as a handhold on the eolith, and there is a logical relationship between the worked and unworked portion."

During the early decades of the twentieth century, the recurring cement-mill accusations were also leveled against the finds of J. Reid Moir in England. But M. C. Burkitt, a Cambridge archeologist and anthropologist, rejected the various attempts to account for the chipping on crude stone implements by reference to mechanical agencies. In 1905, Marcellin Boule had published a long article about cement-machine chipping that produced pieces of stone resembling eoliths. In his book *The*

*Old Stone Age*, Burkitt (1956, p. 104) noted: "It is certainly true that specimens showing a remarkable series of chipplings are produced by such machines, but no mechanical machine or natural force can chip a flint, dealing the blows from only two or three directions, more or less at right angles to one another." Burkitt (1956, p. 104) believed that some of Moir's flint specimens met that criterion and pointed out that "a number of serious students believe that the Kent specimens [of B. Harrison] are really the result of human workmanship."

It therefore appears that in no case were opponents of anomalously old crude stone tool industries able to conclusively demonstrate that implements representative of these industries could be duplicated by the action of cement and chalk mills. Thus they failed to show that the implements were in fact the product of purely natural forces rather than intentional human work. Instead, various researchers, from the late nineteenth century to the present, have presented criteria by which crude stone implements can be distinguished from the products of random battering of lithic materials, and have shown that the stone tool industries under consideration satisfied these criteria.

### 3.6 Impact of the English Eolithic Industries on Modern Ideas of Human Evolution

If scientists were to resurrect the eoliths of the Kent Plateau and East Anglia, at least granting them some serious consideration, then how would they fit into the current scenario of human evolution?

#### 3.6.1 Eoliths of the Kent Plateau

First let us consider the implements discovered by Benjamin Harrison on the Kent Plateau. For the sake of the discussion that follows, let us set aside all the evidence for stone tool industries in the Miocene and earlier geological periods (see for example, Sections 4.1-4, 4.7, 5.5), and let us consider just the Kent Plateau implements. The reasoning behind this approach is as follows. If we take seriously the evidence for the presence of toolmaking beings in Europe during the Miocene period, then the whole story of human evolution currently accepted, with the Homo line originating in Africa and migrating to Europe and Asia during the Early Pleistocene, must be completely wrong. For the present, we just want to consider why the Eolithic implements of England, by themselves, present problems for advocates of the currently accepted doctrines of human evolution.

We have seen that the eoliths of the Kent Plateau may be referred to the Pliocene period in England. The end of the Pliocene is generally placed at about 2 million years ago, although some place it at about 1.6 million years ago (Gowlett 1984, p. 200). Hugo Obermaier, one of the important scientists working in the field of paleoanthropology during the early twentieth century, wrote of "the eoliths from the chalk plateau of Kent in southern England, which belong to the Middle Pliocene" (1924, p. 8).

A Middle Pliocene date would make the eoliths of Kent 3-4 million years old. Most paleoanthropologists now put the origin of Homo sapiens of the fully modern type (technically known as Homo sapiens sapiens) at a maximum of 100,000 years before the present. The immediate forerunner of Homo sapiens sapiens, technically known as archaic Homo sapiens or early Homo sapiens, would date back only 200,000-300,000 years. Homo erectus, the supposed ancestor of early Homo sapiens, dates back roughly 1.5 million years in Africa (Oohanson and Edey 1981, p. 283), and Homo habilis, the supposed ancestor of Homo erectus, dates back only 2 million years. According to the standard account, the hominids of the Late and Middle Pliocene would have been very primitive australopithecines, none of which are thought to have been makers of stone tools.

Just for the sake of argument, let us suppose that the eoliths of the Kent Plateau can be referred to the very latest Pliocene, at about 2 million years b.p. This is, of course, too early for Homo sapiens. It is also too early for Homo erectus. Even if we push the first appearance of Homo erectus back further than 1.5 million years, the 2-million-year minimum age for the Eolithic implements of the Kent Plateau still causes some problems. According to the most widely accepted scenario of human evolution, Homo erectus was the first hominid to leave Africa, and did

not do so any earlier than about a million years ago. Thus even an Early Pleistocene date for the Harrison implements from the Kent Plateau would be problematic.

Up to now, we have been speaking of the standard evolutionary account of human origins, with the major transitions taking place in Africa. But there is a second, less widely held version of the human evolutionary process. According to this account, the transition to *Homo erectus* and *Homo sapiens* took place not in Africa alone but across a wider geographical range (Gowlett 1984). This means that the precursors of *Homo erectus*, creatures like *Homo habilis*, must have already been existing outside Africa, perhaps as much as 2 million years ago. According to some scientists, *Homo habilis* made the very primitive stone tools found in the lower levels of Olduvai Gorge, tools very much like eoliths. It is therefore within the realm of theoretical possibility (for some paleoanthropologists) that a creature like *Homo habilis* may have made the eoliths found by Benjamin Hamson in England.

One would thus have to make relatively few changes in current theory to accommodate the Harrison eoliths. But once such evidence has been condemned, it must apparently remain so perpetually, with no chance of rehabilitation. Even scientists whose theories the tainted evidence might support ignore it. Why? Perhaps because if some relatively benign evidence of this kind were to be resurrected, then more threatening evidence might also emerge from the crypt.

### 3.6.2 East Anglian Tools and the African Origins Hypothesis

The implements discovered by J. Reid Moir pose a similar set of problems, which were, interestingly enough, recognized by a modern researcher (Coles 1968). Some of Moir's discoveries in the Cromer Forest Bed were referred to the Middle Pleistocene. Others, from the Red Crag, were referred to the Early Pleistocene or Late Pliocene. For the purposes of this discussion, we shall set aside implements from the detritus bed below the Red Crag, which could be dated anywhere from the Pliocene to the Eocene.

J. M. Coles (1968, p. 30) summarized his review of Moir's East Anglian discoveries by stating: "in view of the evidence of early man in North Africa and in Southern Europe, there is nothing basically startling about the presence of human industries in East Anglia at the beginning of the Middle Pleistocene. The axe from Sidestrand, if it is, in fact, a paleolithic tool and not a neolithic roughout in an erosion pocket, suggests that man was present during the Cromerian interglacial period, or early in Mindel times. This would not be out of step with the evidence of man's presence in Europe . . . during these periods, but the character of the handaxe is rather surprising. But even more surprising would be the existence of a handaxe tradition encompassing the Whitlingham axe in the Norwich Crag phase, or pre-Giinzian age, which at the moment would seem radically out of step with our evidence for early man and early industries, in both Africa and Europe. The evidence for humanly-struck flints at Foxhall, certainly the most puzzling of all the East Anglian sites, if accepted, would extend back to the earliest Villafranchian, and would indicate that an enormous gap in our evidence for early man existed, if we were to maintain our belief in an African origin."



In suggesting, however obliquely, that the belief in an African origin might be open to question, Coles is, in the light of the most widely accepted view, verging on heresy. The early Villafranchian stage, in which Coles placed the Foxhall tools, belongs to the Late Pliocene, extending from 2.0 to 3.5 million years ago (Section 1.7). According to our conservative estimate, the Foxhall site would most likely fall toward the latter part of the early Villafranchian stage, between 2.0 and 2.5 million years before the present (Section 3.3.4). One would not expect to find toolmaking humans present in England at that time. According to the African origins story, one should find during that period, in Africa alone, Just apeline Australopithecus, who is not thought to have been a toolmaker.

In Coles we see a modern establishment scientist approaching the point of giving serious consideration to one of the conclusions warranted by the evidence presented in this book, namely, that an African origin for the Homo line is a myth. Coles found himself confronted with a spectrum of anomalous evidence. Some of it was mildly surprising to him, some of it more surprising. This is to be expected—that there should be, in the range of evidence ignored by the scientific establishment, a certain number of cases that approach the borderlines of acceptability. However, in light of the evidence we have thus far considered, and evidence we shall consider in coming chapters, it is clear that the Late Pliocene discoveries Coles found most surprising are Just the tip of an iceberg of anomalous evidence that extends into the depths of the Tertiary and beyond.

We suggest it is the threatening nature of this vast body of anomalous evidence that might cause establishment science to steadfastly refuse to consider even the borderline evidence. One thing leads to another. If the borderline evidence is admitted, then the more surprising evidence comes one step closer to acceptance. And then what very quickly happens, as Coles hinted, is that the African origins hypothesis evaporates. And then where would paleoanthropology be? Lost in a raging sea of evidence suggesting all kinds of impossible things. A strong sense of vertigo is bound to arise, because there is a lot of evidence, every bit as good as Moir's discoveries, that puts human beings back as far as the Miocene (Sections 4.1-3), Oligocene (Section 4.4), and Eocene (Section 5.5). At that point, not only the idea of an African origin but also the whole concept of an evolutionary origin of the human species becomes untenable. And if scientists are forced to give up an evolutionary explanation of human origins, what does that say about the whole theory of evolution?

Those who have staked their prestige on the slogan 'evolution is a fact not a theory' might counter that the evidence for evolution in general is "overwhelming." There are, of course, millions of species that might be considered, but here we are focusing on one, the human species, and testing the hypothesis of its evolutionary origin. In this defined area of investigation, we have documented overwhelming evidence contradicting the proposal that the modern human type evolved from more apeline predecessors. Trying to avoid the implications of this thought-provoking evidence by bringing in ex cathedra claims of evolutionary progressions in the fossil histories of myriad other species is inappropriate.

### 3.6.3 Recent Pakistan Finds (Plio- Pleistocene Boundary)

Resistance to the idea that representatives of the Homo line may have been present outside Africa around 2 million years ago is apparent in reactions to some recent discoveries in Pakistan. These were reported in a New York Times News Service story appearing in the San Diego Union edition of August 30, 1987. The story told of "reports from British archaeologists working in northern Pakistan that they have found 2-million-year-old chopping tools believed to have been made by early humans." The reports were from the British journal New Scientist. The news article continued: "If such a significantly earlier time of migration is established, it would presumably mean that a more primitive species in the human lineage, Homo habilis, was the first to leave Africa and did so soon after learning to make stone tools. The prevailing view now is that the later Homo erectus, which had a considerably larger brain capacity, initiated the human migration about a million years ago." To those accepting the prevailing view, the English tools, discovered in the nineteenth century, and the new Pakistani stone tools, both at least 2 million years old, present a problem.

The article went on to explain how mainstream scientists considering the Pakistan tools dealt with this problem—they tried to discredit the discovery. "Sally McBrearty, an anthropologist at William and Mary College who has done research in Pakistan, complains that the discoverers 'have not supplied enough evidence that the specimens are that old and that they are of human manufacture. Our review of anomalous stone implements should make us suspicious of this sort of claim. As we have seen, it is fairly typical procedure for scientists to demand higher levels of proof for anomalous finds than for evidence that fits within the established ideas about human evolution.

The New York Times News Service article then stated: "Like many experts, McBrearty was skeptical of the 2-million-year date because the discovery was made in a river plain, which is 'not a good stratigraphical context.' The sediment layers there have been so mixed up by flowing water over time that geologists have a hard time determining whether artifacts are embedded in their original sediments." As previously noted, if this standard were to be applied uniformly, then there should be similar skepticism regarding many important paleoanthropological finds, which were also made in river plains and other places, such as caves, with poor stratigraphy. One good example is the famous Java man, the first bones of which were taken from a flood plain directly on the edge of a river.

Finally, the news service article stated: "Anthropologists also noted that pebbles fracture easily as they roll through flowing water, resulting in shapes that can be mistaken for artifacts." Do these anthropologists think that the British scientists who discovered the implements in Pakistan were unaware of this problem, which has been the object of serious study for over a century? As we have seen earlier in this chapter (Sections 3.2.3, 3.2.5, and 3.2.11), authorities ranging from Sir John Prestwich (1892, p. 256; 1895, and p. 625) to Leland W. Patterson (1983, p. 108) have

pointed out that fortuitous damage to stones in stream beds can be clearly distinguished from intentional human work.

Now let us look at the report on the discovery of the Pakistani tools published in New Scientist, and see how it matches up with the newspaper statements of scientists critical of the find. In the New York Times New Service story, Sally McBrearty strongly suggested that the reported 2-million-year date for the Pakistani implements was very uncertain, but New Scientist stated: "These artefacts are surprisingly old, but the date is convincing" (Bunney 1987, p. 36). McBrearty also claimed that the stratigraphic context was not good, hinting that if the objects were tools, they did not belong to the beds where they were found.

But the New Scientist stated: "Such doubts do not apply in the case of the stone pieces from the Soan Valley southeast of Rawalpindi, argues Robin Dennell, the field director of the Paleolithic Project of the British Archaeological Mission and the University of Sheffield. He and his colleague Helen Rendell, a geologist at the University of Sussex, report that the stone pieces, all of quartzite, were so firmly embedded in a deposit of conglomerate and gritstone called the Upper Siwalik series, that they had to chisel them out" (Bunney 1987, p. 36). According to the New Scientist, the dating was accomplished using a combination of paleomagnetic and stratigraphic studies.

The New York Times News Service article left the reader with the strong impression that the objects in question were quite probably formed by random concussion in stream beds, and it did not mention any of the evidence in favor of their human manufacture. However, the New Scientist gave its readers with a more balanced treatment: "Of the pieces that they extracted, eight, Dennell believes are definite artefacts. In Dennell's view, the least equivocal artefact is a piece of quartzite that a hominid individual supposedly struck in three directions with a hammer stone, removing seven flakes from it [Figure 3.27]. This multifaceted flaking together with the fresh appearance of the scars left on the remaining 'core' make a very convincing case for human involvement, Dennell told New Scientist" (Bunney 1987, p. 36). So what is going on with the find in Pakistan? It appears we may have a recent example of scientists being unable to objectively evaluate evidence that contradicts their preconceptions about the progress of human evolution.

Figure 3.27. A stone tool discovered in the Upper Siwalik formation in Pakistan (Bunney 1987, p. 36). British scientists estimated its age at about 2 million years.

In this case, we find that scientists holding the view that *Homo erectus* was the first representative of the *Homo* line to leave Africa, and did so about a million years ago, were apparently quite determined to discredit stone tools found in Pakistan, about 2 million years old, rather than

modify their ideas. We can just imagine how such scientists would react to stone tools found in Miocene contexts.

### 3.6.4 Siberia and India (Early Pleistocene to Late Pliocene)

Many other discoveries of stone implements around 2 million years old have been made at other Asian sites, in Siberia and northwestern India. Turning first to Siberia, let us consider what A. P. Okladinov and L. A. Ragozin called the riddle of Ulalinka. These two scientists reported in 1984: "Quite recently it was thought that the Siberia Paleolithic was not more than 20-25,000 years ago. Everything changed after a Paleolithic site, bearing no similarities with any site known before, was discovered in 1961 on the slopes of the steep bank of the Ulalinka River, at the edge of the city of Gorno-Altaysk, the capital of the autonomous oblast. Stone tools of primeval man were found here in the form of cobble stones only partially worked over by a coarse chipping. Half or even two-thirds of such a stone retained its original pebbly surface, a kind of scale, which had been removed only at the working end of the tool, at its cutting edge. A person not acquainted with the technology of those remote times would have tossed this stone away, seeing nothing striking in it. But the stone from Ulalinka can tell an archaeologist, a specialist in such things, a great deal" (Okladinov and Ragozin 1984, p. 5). Six hundred such tools were found at Ulalinka.

After the discovery of the implements, geologists dated the Ulalinka site at 40,000 years. This dating poses no particular problems for modern ideas about human evolution. The tools could have been made by anatomically modern *Homo sapiens*, or perhaps by some late survivals of a Neanderthal population in Siberia. But subsequent studies put the Ulalinka site in the late Middle Pleistocene, giving ages that range from 150,000 to 400,000 years (Okladinov and Ragozin 1984, pp. 5-6). Then, in 1977, Okladinov and Ragozin conducted new excavations and determined that the implement-bearing stratum was much older than scientists previously thought. They stated: "the pebble tools belong to the middle part of the Kochkov horizon, the Podpusk-Leblazh'e layers, formed roughly 250,000 to 1.5 million years ago. This conclusion was confirmed by thermoluminescent analysis done by A. I. Shliukov, Director of the Geochronology Group of the Faculty of Geography of the Moscow State University. ... It was found that the cultural layer with the Ulalinka pebble tools was more than 1.5 million years old" (Okladinov and Ragozin 1984, pp. 11-12). The faunal remains at the site were comparable to the middle Villafranchian (Early Pleistocene or Late Pliocene) of Europe (Okladinov and Ragozin 1984, p. 12).

Okladinov and Ragozin (1984, p. 12) also reported: "Similar pebble tools were found in China, together with two knives made of hominid incisor teeth. This is the so-called Yuanmou man. His age, according to paleomagnetic data, is from 1.5 to 3.1 million years; the accepted date is 1.7 million years."

Okladinov and Ragozin (1984, p. 14) then posed a question: "was the Ulalinka man an aborigine or did he come in from somewhere else?" It was possible, they stated, that the ancestors of Ulalinka man had migrated from Africa. If so, the migration must have occurred well over 1.5 million years ago, and the being that migrated would therefore have been *Homo*

habiiis.

But the Russian scientists apparently had some patriotic impulse, and favored the idea that the ancestors of the Ulalinka hominid had not migrated from elsewhere. Okladinov and Ragozin (1984, pp. 15-19) therefore proposed an extensive search for skeletal remains of a possible ancestor of Ulalinka man in Siberia, hinting that Siberia, not Africa, might very well have been the cradle of humanity. In a paleoanthropological reflection of the wider Sino-Soviet conflict, Okladinov and Ragozin (1984, p. 18) proposed: "It is not impossible that *Sinanthropus* [Peking man] stems from the Ulalinka hominids." In other words, China man came from Russia man. The Chinese, however, believed the reverse to be true.

Okladinov and Ragozin were not the first scientists to broach the idea that human beings evolved within the borders of the former Soviet Union. Alexander Mongait, an archeologist, wrote (1959, p. 64): 'today it may be surmised that Transcaucasia was within the vast zone where man first appeared. . . . In 1939, the remains of an anthropoid ape, which lived at the end of the Tertiary period, was found in Eastern Georgia in a locality called Udabno. It was named *Udabnopithec*. This find confirmed the possibility that mankind originated in Trans-Caucasia (in addition to other regions embracing South Asia, South Europe, and Northeast Africa). But in order to substantiate this hypothesis, science needed the chief link—if not the remains of primitive man himself, then at least the most ancient implements of labor. In 1946-48, S. M. Sardaryan and M. Z. Panichkina, while surveying Satani-dar (Mount Satan), which is situated close to Mount Bogutlu in Armenia, found crude obsidian implements of the most ancient forms dating from the Chellean period: to date, these implements are the most ancient of the archaeological finds in the U.S.S.R. and make up yet another link in the chain of facts proving that the southern areas of the Soviet Union were part of the region where man grew out of the animal state."

Another scientist, Yuri Mochanov, discovered stone tools resembling the European eoliths at a site overlooking the Lena River at DiringYurlakh, Siberia. The formations from which these implements were recovered were dated by potassium-argon and magnetic methods to 1.8 million years before the present. Mochanov, leaving aside the standard African origins concept, proposed the simultaneous emergence of man in Siberia and Africa during the very early Pleistocene. Mochanov stated: "I couldn't believe my eye, at first. After all, I had always argued against finding such primitive pebble tools in this part of Siberia" (Daniloff and Kopf 1986). Some have argued that Siberia was too cold for human habitation. But Pavel Melnikov, director of the Permafrost Institute at Yakutsk, stated that "paleobotanists, studying pollens and seeds in ancient layers, have concluded that the Siberian climate a million years ago was much like today and could have supported people" (Daniloff and Kopf 1986). There is no reason to rule out the possibility that these toolmaking people might have been very much like modern *Homo sapiens*. And here is something else to consider—if the climate was like that of today these ancient Siberians surely would have needed clothing, indicating an advanced level of culture.

Recent evidence from India also takes us back about 2 million years. Many discoveries of stone tools have been made in the Siwalik Hills region of northwestern India. The Siwaliks derive their

name from the demigod Shiva (Sanskrit Siva). the lord of the forces of universal destruction. Roop Narain Vasishat, an anthropologist at Punjab University, objected strongly to the idea that "the Siwalik hominoids did not evolve into hominids and the prehistoric stone tool making man in this region was an intruder from outside" (1985, pp. xiv-xv). Some Indian scientists, like the Russians and Chinese, believe that the key steps in human evolution took place within their nation.

In 1981, Anek Ram Sankhyan, of the Anthropological Survey of India, North Western Region, reported: "the author recovered a Palaeolithic implement from the Upper Siwalik horizon, about 8 kms [5 miles] east of Haritalyangar village" (1981, p. 358). Sankhyan offered this description of the implement: 'The stone tool under reference is a typical Bifacial Chopper made on a large darkcoloured quartzite cobble, 12.5 cm [4.9 inches] in length, 9.3 cm [3.7 inches] in breadth, and 6.5 cm [2.6 inches] in maximum thickness at the butt end. The core exhibits multiple flaking scars on nearly half of its surface on both sides forming a sharp and broad cutting edge. One surface is smoothly flaked and tapering whereas the other carried a large and deep flake scar, besides other smaller flake scars near the edge. The butt end is unworked and rounded for a comfortable grasp" (1981, pp. 358-359).

On the age of the implement, Sankhyan (1981, p. 358) reported: "The stone tool was recovered from a thin band of pebbles distributed in patches over a grey shale horizon. . . . Prasad (1971) assigns these beds to the Tatrot Formation (Upper Pliocene)." Sankhyan subsequently discovered many more stone tools apparently from the same Tatrot horizon (1983, pp. 126-127). Other Indian researchers have made similar finds in the same area.

The above-mentioned Siberian and Indian discoveries, at 1.5-2.5 million years old, do not agree very well with the standard view that *Homo erectus* was the first representative of the *Homo* line to emigrate from Africa, doing so about a million years ago. But, as previously mentioned, they might agree with the view that creatures like *Homo habilis* migrated from Africa about 2 million years ago. One prominent scientist expressing this view is John Gowlett of Oxford. Gowlett wrote (1984 p. 59): "Although it is sometimes suggested that human occupation of the East only started with the migration of *Homo [erectus]* from Africa at the beginning of the Pleistocene, this seems unlikely. Some of the very first fossil hominid remains ever found are those of *Homo erectus* from Java, which can hardly have been the first stop on a migration route. In addition to these historic finds made by Eugene Dubois in 1891 near the Solo River, other more primitive specimens have since been discovered in the older Djertis beds." The Djertis beds were given a potassium-argon date of 1.9 million years Oacob 1972; Gowlett 1984, p. 59). But subsequent tests (Bartstra 1978; Nilsson 1983, p. 329) gave the Djertis beds a far younger date of less than 1 million years. In Chapter 7, we shall see that the Java *Homo erectus* discoveries are, however, all highly questionable, because they are practically all surface discoveries. This means that the stratigraphic context, and consequently the dates, are not firmly established.

In any case, Gowlett (1984, p. 58) proposed: "Human evolution is likely to have taken place across a continuous band of the tropics and subtropics. . . . our only certain evidence comes from a thin scattering of archaeological sites and human remains. These testify directly to the early occupation

of large areas, including southern Africa and the Far East, from 2 or 3 million years ago." Gowlett did not offer very much further in the way of detail, but from the whole of his discussion it would appear he was suggesting that *Homo habilis* and perhaps even the australopithecines were spread widely throughout this region 2-3 million years ago. In this case, why did Gowlett not mention the Eolithic implements of England, also 2-3 million years old? It would seem they would have lent support to his hypothesis.

There come to mind at least three reasons why Gowlett did not mention the English eoliths in connection with his hypotheses about human evolution: (1) he was aware of the discoveries of Harrison, Moir, and others, but accepted the verdict of Barnes and others that they were products of natural forces; (2) he was aware of the Early Pleistocene and Late Pliocene eoliths of England but hesitated to mention them because of their embarrassing connection with older eoliths from the Early Pliocene, Miocene, and earlier periods; (3) he was unaware of the discoveries.

Many modern students of paleoanthropology are in fact completely unaware of reports of crude stone tool industries from the Tertiary and early Quaternary.

Why? The eolith evidence was buried decades ago by skeptical scientists, at a time when it did not fit in so well with then current theories of human evolution. During the 1930s, the oldest human ancestors completely accepted by science were the Java *Homo erectus* and Peking *Homo erectus*, which dated back to the Middle Pleistocene, about a half million years ago. This did not leave any place for a toolmaking being in England during the Early Pleistocene, 1-2 million years ago or Late Pliocene, 2-3 million years ago. Now, the understanding of human evolution has changed, and there are some versions with which the English eolith evidence seems somewhat compatible. But hardly any scientists are now familiar with the discoveries of Harrison or Moir. So here is a good argument for not burying controversial evidence so deeply that it is hardly remembered—it may become relevant in light of future developments.

Again, it should be kept clearly in mind that in discussing how the English eoliths relate to modern evolutionary scenarios centering on a Late Pleistocene origin of the human species, we are deliberately excluding from consideration the extensive evidence (in the form of incised and broken animal bones, stone implements, and modern human skeletal remains) that places humans of the modern type in the Early Pliocene, the Miocene, and even more distant geological periods. When this evidence is admitted into the discussion, as we believe it should be, the discovery of stone implements in the Pliocene in England or anywhere else poses no particular problems.

Where has all of the preceding discussion left us? The main conclusion is that most modern paleoanthropologists are unable to cope with stone tools from periods and places that even slightly deviate from entrenched ideas about the time for the migration of the *Homo* line out of its Africa homeland. Evidence is submitted to intense negative criticism for no other reason than that it conflicts with established views. If this is true of evidence that lies on the very borderline of acceptability, then what kind of treatment can one expect for otherwise good evidence that happens to lie completely beyond the range of current expectations, such as the Miocene

implements discovered in France and Portugal (Sections 4.1-3)? Silence and ridicule are the receptions most likely to be encountered.

Of course, even after having heard all of the arguments for eoliths being of human manufacture, arguments which <sup>11</sup> certainly prove convincing to many, some might still legitimately maintain a degree of doubt. Could such a person, it might be asked, be forgiven for not accepting the eoliths? The answer to that question is a qualified yes. The qualification is that one should then reject other stone tool industries of a similar nature. This would mean the rejection of large amounts of currently accepted lithic evidence, including for example, the Oldowan industries of East Africa and the crude stone tool industry of Zhoukoudian (Choukoutien) in China.

### 3.7 Acceptable Eoliths: The Stone Tools of Zhoukoudian and Olduvai Gorge

We shall now examine some stone tools broadly similar to but in some cases even more primitive than European eoliths such as those found by Benjamin Harrison and J. Reid Moir. Unlike the European eoliths, these implements are unquestioningly accepted by modern paleoanthropologists. It would seem, however, that if tools comparable to the European eoliths are considered genuine, then to be consistent, the European eoliths should also be accepted as genuine.

#### 3.7.1 Accepted Implements from Zhoukoudian (Middle Pleistocene)

One industry similar to the European Eolithic industries is that found at Zhoukoudian, the site of the Peking man discoveries. The Zhoukoudian tools, comprising natural flakes modified with unifacial chipping, compare favorably with the European eoliths. In fact, the crudeness of the tools at Zhoukoudian (Figure 3.28) was unexpected. Peking man was classified as *Homo erectus*, who in Europe and Africa was usually associated with the more advanced bifacially flaked Acheulean implements. Anthropologist Alan Lyle Bryan (1986, p. 7) stated: "less than 2% of the 100,000 artifacts recovered from the living floors at Zhoukoudian Locality I exhibit bifacial edge retouch."

Zhang Shensui of China described the implements from the lower levels of Locality 1 at Zhoukoudian: "Tools fashioned from cores, pebbles and small chunks of stone outnumber those made on flake blanks. This assemblage is typologically simple, consisting primarily of choppers and scrapers. Points and gravers occur only rarely and are very crudely retouched" (Zhang 1985, p. 168). When illustrations of the eoliths found on the Kent Plateau and in East Anglia (Figures 3.3, p. 95; 3.6, p. 121; 3.12, and p. 136) are set alongside those of tools from Zhoukoudian, we do not notice much of a difference in workmanship.



Figure 3.28. These tools from the Zhoukoudian cave seem cruder than the anomalously old Pliocene and Miocene eoliths of Europe (Black et. al. 1933, pp. 115, 131, 132).

### 3.7.2 The Oldowan Industry (Early Pleistocene)

A second industry very much like the European eoliths is the Oldowan industry, initially discovered by Mary and Louis Leakey in Beds I and II of Olduvai Gorge, Tanzania, during the 1930s. Many of the Oldowan implements were described by Mary Leakey in the third volume of *Olduvai Gorge*, published by the Cambridge University Press in 1971.

From the published reports, which is all we really have to go on, it is not possible to easily distinguish European eoliths, such as those collected by Harrison on the Kent Plateau, from some of the Oldowan tools. This is readily seen in the illustrations in Mary Leakey's book, which show the apparent identity between the two types. Although made of different kinds of stone, they look remarkably alike. Furthermore, Leakey's verbal descriptions could just as well be applied to eoliths. One might say that there are subtle distinctions not revealed in the reports, but then what does that say about the quality of scientific reporting on stone tool industries?

Mary Leakey stated that the Oldowan industry was found in locations ranging from upper Bed I to the base of Bed II at Olduvai Gorge. Describing the primary Oldowan industry, she stated: "It is characterised by choppers of various forms, polyhedrons, discoids, scrapers, occasional hammer stones, utilised cobbles and light-duty utilised flakes" (M. Leakey 1971, p. 1). In Bed II, Leakey found an industry she called Developed Oldowan, which contained more spheroid types than the Oldowan. Bed II also yielded a second industry, Developed Oldowan B, which contained some bifacially flaked tools (less than 40 percent of the assemblage). Bifacially flaked tools are those with chipping on both surfaces of the edges. In the upper part of Middle Bed II, there occurred Acheulean assemblages, in which more than 40 percent of the tools were bifacially flaked. Even these were still quite crude. According to Leakey, "The Acheulean appears to be an early form in which the bifaces exhibit minimal flaking and considerable individual variation" (M. Leakey 1971, p. 2). The Acheulean type of Olduvai appears to correspond with the Paleolithic Implements described by Harrison and Prestwich, while the Oldowan type, especially its unifacially flaked specimens, appears to roughly correspond with the flint implements described as eoliths. We shall mainly concern ourselves with the Oldowan industry.

The majority of the Oldowan tools were classified as "choppers," made of volcanic cobblestone and also of quartz and quartzite. Leakey stated: "These are essentially jagged and lack secondary trimming, although utilisation has often resulted in the edges having been chipped and blunted" (M. Leakey 1971, p. 1). In other words, these are even cruder than the eoliths of the Kent Plateau, most of which display some form of intentional secondary trimming. Careful searching, however, has failed to reveal a single published challenge to the authenticity of the Oldowan specimens as genuine human artifacts.

One might argue that hominid fossils have been recovered at Olduvai Gorge, while none were found on the Kent Plateau. It should, however, be noted that crude stone tools were being

excavated at Olduvai Gorge by Louis and Mary Leakey for decades before any currently accepted hominid fossil remains were recovered. In 1959, the Leakeys discovered the first fossil bones of a new primitive apelike hominid, which they regarded as humanlike and named *Zinjanthropus* (Section

11. 4.1). They initially attributed the stone tools of Olduvai Gorge to *Zinjanthropus*. Not long thereafter, however, the bones of a more advanced hominid, *Homo habilis*, were found nearby (Section 11.4.2). *Zinjanthropus* was demoted from his status as toolmaker, and *Homo habilis* replaced him.

But although the designation of the toolmaker was changed, the tools themselves remained unquestioned. The principal reason why the implements discovered in Olduvai Gorge have not been subjected to the same sorts of challenges directed at the eoliths discovered in Europe is hinted at in the following statement made by Mary Leakey (1971, p. 280): "evidence for the manufacture of tools by means of using one tool as an instrument to make another is one of the most important criteria in deciding whether any particular taxon has reached the status of man. ... If evidence of toolmaking is not counted as a decisive factor for the human status it is difficult to see what alternative can be used for determining at what point it had been reached. Evolutionary changes must have been so gradual that it will never be possible for the threshold to be recognised on the evidence of fossil bones alone. This would be true even if a far more complete evolutionary sequence of material were available for study: with the scanty and often incomplete material that has survived it is clearly out of the question. An arbitrary definition based on cranial capacity is also of doubtful value, since the significance of cranial capacity is closely linked with stature or body size, of which we have little precise information in respect of early hominids."

Scientists almost unanimously accept the idea that the genus *Homo* arose in Africa, developing from the australopithecine hominids around 2 million years ago. The strong need for stone tools as corroborating evidence of humanlike status may thus explain, at least in part, the extremely lenient treatment of the Oldowan industry. If they were not accepted as tools, that would greatly detract from the status of the African hominids as human ancestors.

In her report on Olduvai Gorge, Mary Leakey identified, besides the choppers previously mentioned, several other types of implements, which, from her descriptions, appear to correspond to the eoliths found in Europe. She described "various fragments of no particular form but generally angular, which bear a minimum of flaking and some evidence of utilisation" (M. Leakey 1971, p. 6).

Another category of Oldowan tools was scrapers of various types. Leakey described the heavy-duty scrapers of Bed II, which were fashioned from quartzite flakes, as follows: "Many of the heavy-duty scrapers are impossible to assign to any particular type and consist merely of amorphous pieces of lava, quartz, or quartzite, with at least one flat surface from which steep trimming has been carried out along one edge" (M. Leakey 1971, p. 6). About "discoidal scrapers," Leakey wrote: "the tools are seldom entirely symmetrical and they are usually trimmed on only

part of the circumference" (M. Leakey 1971, p. 6). These scrapers conform to the descriptions of the eoliths discovered on the Kent Plateau of England.

Another type similar to a common variety of eolith was the nosed scraper. About this type of tool, Leakey stated: 'There is a median projection on the working edge, either bluntly pointed, rounded, or occasionally spatulate, flanked on either side by a trimmed notch or, morerarely, by straight convergent trimmed edges" (M. Leakey 1971, p. 6). Hollow scrapers, with a broad curved indentation on one side of the stone fanning the working edge, are another type common both to the Eolithic and Oldowan assemblages. Leakey described this type as follows: 'Specimens in which the notch is unquestionably prepared are relatively scarce in both the heavy and light-duty groups, although light-duty flakes and other fragments with notches apparently caused by utilisation are common" (M. Leakey 1971, p. 6). In other words, on these Oldowan specimens, as in the case of eoliths, the working edge of the stone had simply been modified by slight chipping or use.

One of the more remarkable coincidences of form may be found in the presence of tools called awls or borers in both Eolithic and Oldowan assemblages. Of the awls in the Developed Oldowan, Mary Leakey (1971, p. 7) stated: 'They are characterized by short, rather thick, pointed projections, generally at the distal ends of flakes, but sometimes on a lateral edge. In the majority, the points are formed by a trimmed notch, on either one or both sides, but occasionally by straight convergent trimmed edges. The points are often blunted by use and have sometimes been snapped off at the base." This description perfectly applies to the awls collected and displayed by both Harrison and Moir. The identity of the Oldowan and English specimens is very much evident in Figure 3.5 (p. 96).

About the above-mentioned light-duty flakes and fragments, Leakey wrote: "Flakes and other small fragments with chipping and blunting on the edges occur in both the Oldowan and developed Oldowan but are more common in the latter. They fall into three groups: (a) with straight edges; (b) with concave or notched edges; (c) with convex edges. There is also a miscellaneous group with indeterminate chipping. In specimens with straight edges, chipping is usually evident on both sides, while in the notched and convex series it is usually only present on one face" (M. Leakey 1971, pp. 7-8). Leakey also described "light-duty utilised flakes" (Figure 3.29). Of these, she stated: 'The utilised edges are sharp, with 'nibbled' one-directional flaking, which is sometimes present on two of the edges" (M. Leakey 1971, p. 37). The above descriptions could also apply to many of the European eoliths.

### 3.7.3 Who Made the Eolithic and Oldowan Implements?

Now comes a crucial question: to what sort of being should the manufacture of the quite similar Oldowan and Eolithic tool types be assigned? Most of the tools in both the Oldowan and Eolithic assemblages are very crude. Scientists are prepared to accept practically without question that the Oldowan implements were made by *Homo habilis*, a primitive hominid species which, according to modern paleoanthropological thought, marks the initial transition from the australopithecine hominids to the genus *Homo*. It should not, therefore, be completely unthinkable for scientists to entertain the possibility that a creature like *Homo habilis* might also have made the eoliths from

East Anglia and the Kent Plateau, some of which are roughly comparable in age to the Oldowan tools.

Even in the absence of *Homo sapiens* remains, the advanced nature of some of the Oldowan tools raises questions about the correctness of attributing their manufacture to a creature as primitive as *Homo habilis*. The Leakeys found in Bed I of Olduvai Gorge bola stones and an apparent leather-working tool that might have been used to fashion leather cords for the bolas (Section 5.3.2). Using bola stones to capture game would seem to require a degree of intelligence and dexterity beyond that possessed by *Homo habilis*. This concern is heightened by the recent discovery of a relatively complete skeleton of *Homo habilis*, which shows this hominid to have been far more apelike than scientists previously imagined (Section 11.7).

It should be kept in mind that *Homo sapiens* fossils are quite rare even at Late Pleistocene sites where, according to conventional views, they should be expected to be found. Marcellin Boule (Boule and Vallois 1957, p. 145) noted that scientists searching for human fossils in the Prince's

Cave at Grimaldi in southern Europe sifted through four thousand cubic yards of deposits without finding a single human bone. Nevertheless, stone tools and animal remains were both abundant in the cave. Thus the absence of *Homo sapiens* fossils at a particular site does not eliminate *Homo sapiens* as the maker of stone tools found there.

Furthermore, as described in Chapters 6 and 11, fossil skeletal remains of human beings of the fully modern type have been discovered by scientists in strata at least as old as the lower levels of Olduvai Gorge, Tanzania. Among them may be numbered the fossil human skeleton discovered in 1913 by Dr. Hans Reck in Bed II of Olduvai Gorge (Section 11.1), and some fossil human femurs discovered by Richard Leakey at Lake Turkana, Kenya, in a formation slightly older than Bed I at Olduvai (Section 11.3). Bed I is now dated at approximately

1.75 million years, and the top of bed II is dated at about 0.7-1.0 million years (M. Leakey 1971, pp. 14-15).

It is, therefore, not correct to say that there is no fossil evidence whatsoever for a fully human presence in the lower levels of Olduvai Gorge. In addition to fossil evidence, we have a report from Mary Leakey (1971, p. 24) about a controversial circular formation of stones at the DK site in lower Bed I: "On the north side, where the circle was best preserved, there were groups of stones piled up into small heaps. It is possible to identify six of these piles which rise to a height of 6-9 in. and are spaced at intervals of 2-2.5 ft., suggesting that they may have been placed as supports for branches or poles stuck into the ground to form a windbreak or rough shelter."

Leakey then continued: "In general appearance the circle resembles temporary structures often made by present-day nomadic peoples who build a low stone wall round their dwellings to serve either as a windbreak or as a base to support upright branches which are bent over and covered with either skins or grass" (M. Leakey 1971, p. 24). For the purpose of illustration, Mary Leakey provided a photograph of such a temporary shelter made by the Okombambi tribe of South West Africa (now Namibia).

Not everyone agreed with Leakey's interpretation of the stone circle. But accepting Leakey's version, the obvious question

may be raised: if she believed the structure resembled those made by 'present-day nomadic peoples' like the Okombambi, then why could she not assume that anatomically modern humans made the Olduvai stone circle 1.75 million years ago?

The same assumption might easily be made about even the crudest stone tools. Leakey stated in her book: "An interesting present-day example of unretouched flakes used as cutting tools has recently been recorded in South West Africa and may be mentioned briefly. An expedition from the State Museum, Windhoek, discovered two stone-using groups of the Ova Tjimba people who not only make choppers for breaking open bones and for other heavy work, but also employ simple flakes, un-retouched and un-hafted, for cutting and skinning" (M. Leakey 1971, p. 269). Nothing, therefore, prevents one from entertaining the possibility that anatomically modern

humans might have been responsible for even the crudest stone tools found at Olduvai Gorge and the European eolith sites.

At present, we find that humans manufacture stone tools of various levels of sophistication, from primitive to advanced. We also find evidence of the same variety of tools in the Pleistocene, Pliocene, Miocene, and even as far back as the Eocene. There are examples of relatively crude stone tools, such as those found by Ribeiro in Miocene formations in Portugal (Section 4.1). And there are also advanced stone tools, similar to those used by modern Indians in North America, from formations of Eocene antiquity in California (Section 5.5).

The simplest explanation is that anatomically modern humans, who make such a spectrum of tools today, also made them in the past. Continuity of tool types suggests continuity of toolmakers. We might call this the hypothesis of stasis. Alternatively, the evolutionary hypothesis requires us to reject all advanced stone tool industries from periods earlier than the Late Pleistocene. As for the remaining crude stone tools, we must reject the ones found in geological contexts older than the earliest Pleistocene or the latest Pliocene. We must then propose that various grades of subhumans made crude stone tools in the Late Pliocene and Early Pleistocene, and then when modern humans came along in the Middle and Late Pleistocene, they also made identical crude tools along with more advanced ones.

All in all, the hypothesis of stasis allows us to account for all the reported evidence in a more straightforward fashion. The only anomaly in this account of stasis is the absence of evidence for advanced civilization, with its intricate metallic productions and complex stone architecture, in very ancient times. Abundant evidence for such civilization appears to extend back only a few thousand years. There are, however, intriguing hints of the existence of advanced civilization millions of years ago. This evidence, reported in Appendix 2, is, however, not very extensive.

Granting the stasis hypothesis, we must therefore ask the following question. Why are there so many scientific reports of stone tools and cut bones indicating the presence of anatomically modern humans tens of millions of years ago yet so little evidence of more advanced civilization for the same time periods?

Here is one possible explanation. Although the scientists who reported much of the evidence contained in this book were prepared to find signs of a human presence in times far more ancient than allowed by current evolutionary theory, these scientists were themselves evolutionists. As such, they believed that in the past culture was more primitive than today. Therefore, they probably would not have given serious consideration to any evidence of advanced culture in very ancient times.

Did they encounter such evidence but refuse to report it? We cannot say for certain. What we do know is that evidence for advanced civilizations in very ancient times has been reported, but not often by scientists. Many of the reports have come from miners. Such reports are far more likely to turn up in old newspapers than scientific journals. We suspect that many finds suggestive of advanced civilizations in very ancient times have not been reported at all.

It is thus possible that our data base for the study of human origins and antiquity is quite incomplete. But what evidence we do have suggests that anatomically modern humans have been manufacturing stone tools of various degrees of sophistication since the Miocene and earlier.

To further complicate the picture, one could imagine *Homo sapiens* coexisting millions of years ago with species of humanlike apes, unrelated to human beings in any evolutionary sense. These humanlike creatures may have also been able to manufacture very crude stone implements. There are in fact reports from Central Asia of a living ape-man-like creature, the Almas, which is said to break stones for use as tools (Section 10.8), Just like modern humans. Indeed, this is what the unedited record of skeletal remains and stone implements actually suggests—that human beings of modern type and more primitive creatures have been coexisting since time immemorial and manufacturing a whole array of tool types, from the crudest to the most advanced.

### 3.8 Recent Examples of Eolithic Implements from the Americas

Several anomalously old crude stone tool industries of Eolithic type have been discovered in the Americas. A careful study of the debates about these industries will add to our understanding of why and how the stone tools from Pliocene and Miocene sites in Europe have largely disappeared from view, as far as modern science is concerned.

#### 3.8.1 Standard Views on the Entry of Humans Into North America

The debates about various anomalous stone tool industries discovered in the Americas takes place in the context of the standard theory of the entry of humans into the New World. According to this theory, Siberian hunters crossed over the Bering Strait into Alaska on a land bridge that existed when the last glaciation lowered sea levels. During this glacial period, the Canadian ice sheet blocked southward migration until about 12,000 years ago, when the first American immigrants followed an ice free passage to what is now the United States. These people were the so-called Clovis hunters, famous for their characteristic doubly fluted spearpoints. These would correspond to the highly evolved stone implements of the later Paleolithic in Europe.

According to Jared Diamond (1987), these Clovis hunters quickly multiplied and peopled the entire habitable region of North and South America. Because a site in Patagonia, in the southernmost part of South America, is now dated at 10,500 years, the immigrants must have gone from the arctic, to the tropics, and on to the near antarctic regions of South America in little more than a thousand years. In their long march, these Clovis hunters exterminated over 70 percent of the large mammalian genera of the New World in an orgy of rapacious exploitation rivaled only by the European heirs of the territory they conquered (Diamond 1987, pp. 82-88).

The following arguments in favor of this theory were published in the popular science magazine, *Discover*, in June of 1987: "at excavated Clovis sites, conclusive evidence for artifacts made by other peoples has been found above but not below the level with Clovis tools: and there are no irrefutable human remains with irrefutable pre-Clovis dates anywhere in the New World south of the former Canadian ice sheet. Mind you, there are dozens of claims of sites with pre-Clovis



human evidence, but all are marred by serious questions about whether the material used for radiocarbon dating was contaminated by older carbon, or whether the dated material was really associated with human remains, or whether the tools supposedly made by hand were just naturally shaped rocks. In contrast, the evidence for Clovis is undeniable, widely distributed, and accepted by archaeologists" (Diamond 1987, pp. 84, 86).

To put this theory into perspective, we should note that before World War II, anthropological authorities insisted that human beings first entered America just 4,000 years ago. Their initial reaction to the Clovis hunter theory was summed up by the anthropologist John Alloszetai-Petheo (1986, pp. 18-19): "For . . . decades, American archaeologists would labor under the view of man's relative recency in the New World, while the mere mention of the possibility of greater antiquity was tantamount to professional suicide. Given this orientation, it is not surprising that when the evidence of the antiquity of man in America was finally reported from Folsom, Clovis, and other High Plains sites, it was rejected out of hand by established authorities despite the clear nature of the evidence at multiple locations, uncovered by different researchers, and seen and attested to by a large variety of professional visitor/observers. . . . The mind set of conservatives of the day left no room for acceptance."

Alloszetai-Petheo argued that the history of the rejection of the Folsom and Clovis discoveries is now being repeated as conservative archaeologists of the present day staunchly reject evidence for pre-Clovis man in America. Certainly, there are now many cases of archaeological excavations using modern methods that have yielded dates as great as 30,000 years for humans in America.

For example, geological, archaeological, and paleontological research at El Cedral, in the state of Sinaloa, northern Mexico, revealed human artifacts along with bones of extinct animals in 'undisturbed stratified deposits on horizons radiocarbon- dated at 33,000 b.p., 31,850 b.p., 21,960±540 b.p., and older than 15,000 b.p." (Lorenzo and Mirambell 1986, p. 107). The date of 31,850 b.p. corresponds to a hearth found in situ and consisting of "a circle of proboscidean tarsal bones surrounding a zone of charcoal about 30 cm [a foot] in diameter and 2 cm [almost an inch] thick" (Lorenzo and Mirambell 1986, p. 111). Proboscideans are elephants of various kinds. Tarsal bones come from the ankle region.

Another case involves a fire pit found on California's Santa Rosa Island, off Santa Barbara, and investigated by archaeologist Rainer Berger of UCLA. Laboratory testing showed that charcoal samples taken from the pit contained no measurable carbon 14. They are thus older than the 40,000-year limit imposed by the conventional radiocarbon dating method. The find is significant, since the fire pit contained crude chopping tools along with the bones of a bull-sized species of mammoth (ScienceNews 1977 a, p. 196).

Yet another interesting excavation took place in northeastern Brazil. At the rock-shelter of Boquira do Sitio da Pedra Furada, a Joint French-Brazilian team of archaeologists dug through a stratified 3-meter [10-foot] deposit of sediment that was found to contain human occupational debris at all levels. The lowest levels included big circular hearths with large quantities of charcoal and ash. There were pebble tools, denticulates, burins, retouched flakes, and double-edged flakes,

all made from local quartz and quartzite. There were also painted fragments of rock spalled or broken from the cave walls, which suggests that the tradition of rock painting well known in this part of Brazil may have existed during the earliest occupational period (Guidon and Delibrias 1986, pp. 769-771).

Charcoal from the lowest hearth in the deposit yielded carbon 14 dates of  $31,700 \pm 830$  years and  $32,160 \pm 1,000$  years. In addition, carbon 14 dates were obtained at a series of levels running throughout the entire deposit. These dates formed the following consistent series in years b.p.: 6,160, 7,750, 7,640, 8,050, 8,450, 11,000, 17,000, 21,400, 23,500, 25,000, 25,000, 25,200, 26,300, 26,400, 27,000, 29,860, 31,700, and 32,160 (Guidon and Delibrias 1986).

This excavation is of particular interest because it involved a controlled study of stratified cave deposits yielding hearths, artifacts, and a series of radiocarbon dates. These are some of the criteria often insisted upon by defenders of orthodox archaeological theories. However, one can always point to flaws in unwanted evidence, and thereby adopt a double standard.

Of course, a small but increasing number of archaeologists are now accepting that humans may have been living in South America as long as 30,000 years ago. It might therefore be argued that the resistance to new findings exhibited by successive schools of archaeologists is simply a healthy and unavoidable part of the scientific process. By applying the braking action of skepticism, science can make slow but steady progress, while avoiding wild, speculative excesses.

One answer to this is that by sticking to conservative viewpoints in anthropology one certainly does not avoid extreme speculation. The theory that Clovis hunters marched from northern Canada to the Tierra del Fuego in a few centuries is certainly speculative. And the sweeping denial of certain possibilities—such as the existence of humans in America at a certain date—can be just as much a speculative excess as their uncritical affirmation. In addition, it may happen that evidence suppressed as a result of such policies of denial is permanently lost, and important advancements in understanding will be delayed until similar evidence manages to surmount the barriers to acceptance in the future.

An alternative approach would be to recognize that in fields such as archaeology, most empirical evidence is of a doubtful nature, whether it corroborates our views or contradicts them. Therefore, it would be best (though difficult in practice) to maintain all relevant evidence in a readily accessible form, without giving absolute credence to any current positive or negative interpretations. If this cannot be done, one should at least recognize that one may be aware of only a fraction of the evidence that has already been seriously studied—what to speak of the evidence that may be uncovered in the future.

The present method of rendering final judgement on controversial evidence by how well it fits with currently established theories does not seem to be scientifically healthy, and it can be argued that it may do irreparable damage not only to the progress of scientific knowledge, but also to the reputations of persons who happen to find controversial evidence. This is especially true when politics and intrigue enter into the scientific process. Such considerations appear to have played a

major role in the negative treatment of evidence suggesting that human beings were living in the New World long before both the 12,000-year limit still favored by a majority of paleoanthropologists and the 30,000-year limit currently accepted by a growing minority. We shall now discuss a few recent examples of this evidence, in the form of anomalously old crude stone tool Industries, with the aim of shedding more light on the social processes of acceptance and rejection of evidence in the scientific world.

### 3.8.2 Texas Street, San Diego (Early Late Pleistocene to Late Middle Pleistocene)

A good example of a controversial American early stone tool Industry reminiscent of the European eoliths is the one discovered by George Carter (1957) in the 1950s at the Texas Street excavation in San Diego. At this site, Carter (1957) claimed to have found hearths and crude stone tools at levels corresponding to the last interglacial period, some 80,000-

90,000 years ago. Critics scoffed at these claims, referring to Carter's alleged tools as products of nature, or "cartifacts", and Carter was later publicly defamed in a Harvard course on "Fantastic Archeology" (Williams 1986, p. 41). However, Carter gave clear criteria for distinguishing between his tools and naturally broken rocks, and lithic experts such as John Witthoft (1955) have endorsed his claims.

In 1973, Carter conducted more extensive excavations at Texas Street and invited numerous archeologists to come and view the site firsthand. Almost none responded. Carter (1980, p. 63) stated: "San Diego State University adamantly refused to look at work in its own backyard."

Carter found evidence for a human presence during the last interglacial period at several other sites in San Diego and elsewhere in the southwestern United States. But he found it difficult to get his findings published in standard scientific journals. In 1960, an editor of *Science*, the journal of the American Academy for the Advancement of Science, asked Carter to submit an article about early humans in America. Carter did so, but the article was rejected. The editor wrote to Carter on February 1, 1960: "It was good of you to prepare a paper 'On the Antiquity of Man in America' for possible publication in our Current Problems in Research series in *Science*. In view of the fact that I invited you to prepare the paper for us, I especially regret to say that your paper, although it is interesting and deals with an important subject, is too controversial for publication in a general scientific magazine such as ours. I sought the advice of two highly competent advisers and they were in essential agreement with each other in their recommendations. They both thought that the paper was unsuitable for *Science*" (T. E. Lee 1977, p. 3).

Carter replied in a letter to the editor, dated February 2, 1960: "I must assume now that you had no idea of the intensity of feeling that reigns in the field. It is nearly hopeless to try to convey some idea of the status of the field of Early Man in America at the moment. But just for fun: I have a correspondent whose name I cannot use, for though he thinks that I am right, he could lose his job for saying so. I have another anonymous correspondent who as a graduate student found evidence that would tend to prove me right. He and his fellow student buried the evidence. They were certain that to bring it in would cost them their chance for their PhD's. At a meeting, a young

professional approached me to say, 'I hope you really pour it on them. I would say If I dared, but it would cost me my Job.' At another meeting, a young man sidled up to say, 'In dig xthey found core tools like yours at the bottom but Just didn't publish them'" (T. E. Lee 1977, p. 4).

The inhibiting effect of negative propaganda on the evaluation of Carter's discoveries is suggested in the following statement by archeologist Brian Reeves: 'Were actual artifacts uncovered at Texas Street, and is the site really Last Interglacial in age? . . . Because of the weight of critical 'evidence' presented by established archaeologists, the senior author [Reeves], like most other archaeologists, accepted the position of the skeptics uncritically, dismissing the sites and the objects as natural phenomena" (Reeves et al. 1986, p. 66).

But when he took the trouble to look at the evidence himself, Reeves changed his mind. He wrote: 'While visiting San Diego in 1976 the senior author had the opportunity to view some of George Carter's . . . collections from Texas Street ... In Mission Valley. Among the fractured quartzite cobbles were many objects that appeared to Reeves and R. S. MacNeish to be culturally produced, modified, and utilized quartzite cobble artifacts" (Reeves et al. 1986, p. 66). Ten years later Reeves conducted several onsite investigations near Texas Street.

Many of the specimens he studied, although made from quartzite rather than flint, appear to be Eolithic: "In summary, the Mission Ridge quartzite cobble complex includes naturally produced sharp-pointed and edged bipolar cores, blocky quartzite pieces and irregular-shaped sharp-edged flakes. These fragments were not only utilized by man, but also modified into more formed flakes and tools (the horseshoe chopper, for example) as well as culturally manufactured, unifacially retouched and utilized flakes" (Reeves et al. 1986, p. 78).

Reeves concluded: "The bulk of the fractured quartzites recovered from Mission Ridge were, in our opinion, naturally broken but collected elsewhere and brought to the site by man for use primarily as ready-made expediency tools" (Reeves et al. 1986, p. 78). In light of Reeves's change of heart about Carter's tools, one wonders what would result from an open minded review of the European eoliths.

Reeves then made the following commentary on the unfair treatment professional scientists gave to the San Diego Implements: "The fractured quartzite complex, as first claimed by Carter, is part of a Late to Middle Pleistocene quartzite cobble core/unifacial flake tradition of Pacific coastal-adapted people. . . . Had Carter's claims been taken seriously enough by professional archaeologists to undertake detailed field studies instead of simply dismissing them, we would have had a major body of data on Late Pleistocene North American coastal settlement" (Reeves et al. 1986, pp. 78-79). Reeves believed some of Carter's implements to be 120,000 years old.

Over several decades, many ancient human occupation sites were investigated around San Diego, and Carter (1957, pp. 370-371) constructed a tentative history of stone tool usage in this region over the last 90,000 years. After the Texas Street phase, characterized by crude stone tools, came the following developments: (I) The period of 55,000 to 80,000 years ago, represented by 'strongly weathered manos and metates from basal positions in alluvium over interglacial beaches at

Scripps campus and about La Jolla and Point Lorna.” There were also biface and plano-convex cobble core tools, and used flakes.

Manos and metates are grinding tools. Plano-convex tools are flat on one side and rounded, or convex, on the other. (2) The period of 30,000 to 55,000 years ago, with large, crude, percussively flaked, ovate knives, tending to be unifacial. (3) The period of 15,000 years to 30,000 ago with small, slender, leaf-shaped, double-convex knives, broad-stemmed knives, and abundant fine plano-convex tools. (4) Then came the recent San Dieguito and Yuman cultures.

According to standard views, practically all of the variegated lithic forms in this list would have to be either (1) incorrectly dated, or (2) products of human imagination applied to naturally broken stone. The manos and metates are especially interesting, since these grinding tools are generally associated with Neolithic, or very late Stone Age, culture. The oldest accepted examples, from Egypt, are thought to be only 17,000 years old (Gowlett 1984, p. 152).

### 3.8.3 Louis Leakey and the Calico Site in California (Middle Pleistocene)

As we have several times seen in previous chapters (and will see again in later chapters), some famous scientists have occasionally nurtured heretical ideas, despite the personal risks involved in opposing prevailing academic views. One example is Louis Leakey, world renowned for his discoveries in Africa. He began to have radical ideas about the antiquity of humans in America at a time when the entry date for the Siberian hunters was thought to be no greater than some 5,000 years ago. Eventually, Leakey journeyed to America and discovered a crude stone tool industry, of Eolith type, at Calico, in southern California. The site was dated at over 200,000 years.

Leakey recalled: ‘Back in 1929-1930 when I was teaching students at the University of Cambridge, I began to look into the question of the antiquity of man in the Americas. Although there was no concrete evidence to indicate a remote age, I was so impressed by the circumstantial evidence that I began to tell my students that man must have been in the New World at least 15,000 years. I shall never forget when Ales Hrdlicka, that great man from the Smithsonian Institution, happened to be at Cambridge, and he was told by my professor (I was only a student supervisor) that Dr. Leakey was telling students that man must have been in America 15,000 or more years ago. He burst into my rooms—he didn’t even wait to shake hands— and said, ‘Leakey, what’s this I hear? Are you preaching heresy?’” Leakey said, "No, Sir!" Hrdlicka replied, “You are! You are telling students that man was in America 15,000 years ago. What evidence have you?” Leakey replied, "No positive evidence. Purely circumstantial evidence. But with man from Alaska to Cape Horn, with many different languages and at least two civilizations, it is not possible that he was present only the few thousands of years that you at present allow" (L Leakey 1979, p. 91).

Leakey continued to harbor unorthodox views on this matter, and in 1964 he made an effort to collect some definite evidence by initiating an excavation at a site known as Calico in the Mojave Desert of California. This site is situated near the shore of now-vanished Pleistocene Lake Manix, on the eroded remains of an alluvial fan of sediments washed down from the nearby Calico mountains. Over a period of eighteen years of excavation, some 11,400 artifacts were recovered

from a number of levels. The oldest artifact-bearing level has been dated by the uranium series method to about 200,000 years b.p. (Budinger 1983).

There is general agreement among geologists about the great age of the Calico site, and ages as great as 500,000 years have been seriously proposed. However, as happened with Texas Street, mainstream archeologists have tended to reject the artifacts discovered at Calico as products of nature, and the Calico site tends to be passed over in silence in popular accounts of archeology. Indeed, It seemed that the Iconoclastic Leakey, famous for so many revolutionary archeological discoveries, had committed a grave error in judgement In his foray into the New World. Leakey's biographer Sonia Cole (1975, p. 351) said, "For many colleagues who felt admiration and affection for Louis and his family, the Calico years were an embarrassment and a sadness."

Yet the artifacts of Calico also have their defenders, who give elaborate arguments showing that they were human artifacts, not "geofacts" resulting from natural processes. These archeologists include Phillip Tobias, the well-known associate of Raymond Dart, discoverer of *Australopithecus*. Tobias (1979, p. 97) declared: "when Dr. Leakey first showed me a small collection of pieces from Calico . . . I was at once convinced that some, though not all, of the small samples showed

unequivocal signs of human authorship." Tobias went on to point out that the presence of naturally broken stones is to be expected, and does not detract from the validity of artifacts that are mixed in with them.

The arguments presented are reminiscent of the controversy over eoliths in Europe. Detractors such as archeologist C. Vance Haynes (1973, pp. 305-310) claimed that the natural banging together of stones in streams and shifting earth can simulate all the alleged Calico stone tools. On the other hand, defenders pointed out that these alleged natural processes did not occur at sites such as Calico, and could not have produced the observed, systematic patterns of lithic flaking even if they did occur (L. Patterson et al. 1987, pp. 91-105).

Geological evidence indicates that the Calico implements lie in an ancient mud flow context. In this regard, Ruth D. Simpson stated: "Natural forces in a mud flow would be expected to give mainly bidirectional random damage to flake edges. It would be difficult for nature to produce many specimens resembling man-made unifacial tools, with completely unidirectional edge retouch done in a uniform, directed manner. The Calico site has yielded many completely unifacial stone tools with uniform edge retouch. These include end scrapers, side scrapers, and graters. Some graters have bifacial retouch on points, which can be expected in even unifacial flake tool industries" (Simpson et al. 1986, p. 96). Flake tools with unifacial, unidirectional chipping, like those found at Calico, are typical of the European eoliths. Examples are also found among the Oldowan industries of East Africa. Among the best tools that turned up at Calico was an excellent beaked grater (Figure 3.30). Bola stones have also been reported (Minshall 1989, p. 110).

At an international archeological conference held in Mexico City, Mexico, in 1981, three of the defenders of Calico listed 17 criteria for human flaking which, according to them, were met by the artifacts discovered at the Calico site. Some of these criteria were (1) the presence of ripple lines

and force bulbs with bulb scars, (2) striking platform angles under 90 degrees, (3) crushing of striking platforms, (4) no remaining cortex on either striking platforms or dorsal surfaces, (5) prismatic flakes and blades, (6) unifacial edge retouch, (7) flaking on certain edges and not others, (8) well-defined bifacial objects, and (9) specific workshop areas with evidence of stone working (Simpson et al. 1981).

Figure 3.30. Abeaked graver—a stone tool from Calico in southern California, dated at about 200,000 years (Bryan 1979, p. 77).

Herbert L. Minshall stated that in 1985 several of the best small Calico Implements were displayed at the annual meeting of the Society for American Archaeology in Denver, Colorado. Minshall wrote (1989, p. 111): "The tools were finally accepted as manmade, but now the objection was that they could not possibly have so great an age, even though

200,000 years was modest compared to many estimates for the age of the fan sediments. . . . One highly respected archaeologist actually suggested that the tools he was shown must have somehow fallen into the excavation from the surface,"

In 1986, George Carter and Fred Budinger discovered an additional site at Calico. Minshall (1989, p. 111) stated that Carter and Budinger found 'small stone specimens apparently worked by man and more than 20 feet below the dated volcanic ash stratum at the foot of the Calico/Mule Canyon fan" near the main Calico site. Fossils of typical Pleistocene mammals such as the sabertooth tiger, camel, horse, and mammoth were also found beneath the ash, which yielded a potassium-argon date of 185,000 years.

In general, however, the Calico discoveries have met with silence, ridicule, and opposition in the ranks of mainstream paleoanthropology. Ruth Simpson nevertheless stated: "The data base for very early man in the New World is growing rapidly, and can no longer simply be ignored, because it does not fit current models of prehistory in the New World. With the present data gaps that exist in our knowledge of the prehistory of man in the New World, any current proposed 'final' solutions to the early origins, migrations, and cultures of Pleistocene man in the New World are premature. At the present state of knowledge in early man research, there is a need for flexibility in thinking to assure unbiased peer reviews" (Simpson et al. 1986, p. 104). The same might also be said of the larger question of human evolution.

#### 3.8.4 Toca da Esperanga, Brazil (Middle Pleistocene)

Support for the authenticity of the Calico tools has come from a find in Brazil. In 1982, Maria Beltrao found a series of caves with wall paintings in the state of Bahia. In 1985, a trench was cut in the Toca da Esperança (Cave of Hope), and excavations in 1986 and 1987 'yielded stone tools associated with Quaternary fauna in a defined stratigraphic context' (de Lumley et al. 1988, p. 241).

There were four layers in the cave. The first layer was a hard carbonate crust, 20 to 60 centimeters (about 8 to 24 inches) thick. Beneath this were 3 layers of sand and sandy clay. In the lowest, Layer 4, stone implements were discovered along with abundant mammalian fossils. De Lumley et al. (1988, p. 241) commented: 'Three bones . . . were dated by the uranium-thorium method using alpha and gamma-ray spectrometries, [giving] ages between 204,000 and 295,000 years.' These tests were performed at three different laboratories— Gif-sur-Yvette, France; the University of California at Los Angeles; and the laboratory of the U.S. Geological Survey at Menlo Park, California (de Lumley et al. 1988, p. 243).

The tools were fashioned from quartz pebbles and were somewhat crude, like those from Olduvai Gorge. The implements included 'a chopper with cutting edge trimmed by three adjacent removals' (de Lumley et al. 1988, p. 243). The report pointed out that the nearest source of quartz pebbles is about 10 kilometers from the cave.

De Lumley et al. (1988, p. 242) stated: 'the evidence seems to indicate that Early Man entered into the American continent much before previously thought.' They went on to say: 'In light of the discoveries at the Toca da Esperança, it is much easier to interpret the lithic industry of the Calico site, in the Mojave Desert, near Yermo, San Bernardino County, California, which is dated at between 150,000 and 200,000 years' (de Lumley et al. 1988, p. 245).

According to de Lumley and his associates, humans and protohumans entered the Americas from northern Asia several times during the Pleistocene. The early migrants, who manufactured the tools in the Brazilian cave, were *Homo erectus* (de Lumley et al. 1988, p. 242). While this view is in harmony with the consensus on human evolution, there is no reason why the tools in the Toca da Esperança could not have been made by anatomically modern humans. As we have several times mentioned, such tools are still being manufactured by humans in various parts of the world.

Toca da Esperança provides a clear example of how the scientific community hesitates to change deeply held convictions. The discovery was made by a team headed by a famous French scientist, respected in his field. The site was systematically excavated according to strict principles. The implements were discovered in situ, in a defined stratigraphic context. They were clearly intentionally manufactured. They were found in conjunction with a typical Middle Pleistocene fauna, with many extinct species. The researchers admitted that it was not possible to assign a direct age to the cave on the basis of the biostratigraphic evidence (the Middle Pleistocene goes from about 100,000 to 1,000,000 years ago), but multiple uranium series tests gave ages of between 204,000 and 295,000 years. Of course, the uranium series dates could be wrong. But if these are wrong, then every uranium series date, including the ones used to buttress more acceptable finds, could also be wrong. Altogether, it is hard to see what more one could desire in



the way of empirical evidence that would confirm the presence of intelligent toolmaking beings in the Americas in the Middle Pleistocene. Yet the consensus that humans entered the Americas fairly recently remains intact.

### 3.8.5 Alabama Pebble Tools

The crude stone tools of Bed I in Olduvai Gorge, Tanzania, are also paralleled, interestingly enough, by pebble tools from Alabama, U.S.A. that are almost identical in form (Figure 3.31, p. 208). These stone tools, reported by archaeologist Daniel Josselyn (1966), can be found in great numbers in certain surface sites, where they are mixed in with artifacts from a variety of native American cultures.

Pebble tools are usually associated with very primitive levels of culture not thought to have ever existed in America. Thus, when Josselyn tried to acquaint other American archaeologists with his finds, he did not receive an encouraging reaction. "Rather," as he put it, 'to my horror, I learned that Pavlov could have studied 'conditioned reflexes' about as well in archaeologists as in dogs. Please, please, believe that I say this with no critical rancor" (Josselyn 1966, p. 25). It was apparently "known" by some that no pebble tools were made in the New World.

Figure 3.31. Pebble chopper. from Alabama, U.S.A., undated (Josselyn 1966). Such tools usually imply very primitive cultures not thought to have existed in America.

Josselyn said that since the Alabama tools were not from stratified sites, they could not be dated, and he had no suggestion about their age. They could thus be quite recent, posing no threat to dominant views about the arrival of humans in the Americas. The problem here seems to be a fixation on the questionable idea that pebble tools must have been made by protohumans such as *Homo habilis* or *Homo erectus*. But human beings have used pebble tools in Asia and Africa in historic times.

### 3.8.6 Monte Verde, Chile (Late Pleistocene)

Another archaeological site that has bearing on the evaluation of crude stone tools is the Monte Verde site in south central Chile. According to a report in Mammoth Trumpel (1984), this site was first surveyed by archaeologist Tom Dillehay in 1976. Although the date of 12,500 to 13,500 years b.p. for the site is not highly anomalous, the archaeological finds uncovered there challenge the standard Clovis hunter theory. The culture of the Monte Verde people was completely distinct from that of the Clovis hunters. Although these people made some bifacial implements, their lithic technology was based mainly on minimally modified pebble tools. Indeed, to a large extent, they

obtained stone tools by selecting naturally occurring split pebbles. Some of these show signs of nothing more than usage; others show signs of deliberate retouching of a working edge. This is strongly reminiscent of the descriptions of the European eoliths.

In this case, the vexing question of artifacts versus geofacts was resolved by a fortunate circumstance: the site is located in a boggy area in which perishable plant and animal matter has been almost indefinitely preserved. Thus two pebble tools were found hafted to wooden handles. Twelve "architectural foundations" were found, made of cut wooden planks and small tree trunks staked in place. There were large communal hearths, as well as small charcoal ovens lined with clay. Some of the stored clay bore the footprint of a child 8 to 10 years old. Three crude wooden mortars were also found, held in place by wooden stakes. Grinding stones (metates) were uncovered, along with the remains of wild potatoes, medicinal plants, and sea coast plants with a high salt content. All in all, the Monte Verde site sheds an interesting light on the kind of creatures who might have made use of "crude pebble tools" during the Pliocene and Miocene in Europe or at the Plio-Pleistocene boundary in Africa. In this case, the culture was well equipped with domestic amenities made from perishable materials. Far from being subhuman, the cultural level was what we might expect of anatomically modern humans in a simple village setting even today.

By an accident of preservation, we thus see at Monte Verde artifacts representing an advanced culture accompanying the crudest kinds of stone tools. At sites millions of years older, we see only the stone tools, although perishable artifacts of the kind found at Monte Verde may have once accompanied them.

Finally, we note that Tom Dillehay found in the deepest stratum at Monte Verde a split basalt pebble, some wood fragments, two modified stones, and some charcoal dated at about 33,000 years b.p. (Bray 1986, p. 726).

### 3.8.7 Early Humans in America and the Eolith Question

The arguments about American sites tens and hundreds of thousands of years old are similar to those that took place among European scientists when the first evidence for prehistoric humans was coming to light. This was noted by anthropologist Alan Lyle Bryan, who wrote (1986, p. 5): "The present controversy over Early Man in America is analogous to that in Europe more than a century ago because the intellectual climate has been dominated for over 50 years by a particular paradigm which has seemed to fit most of the evidence but which fails to explain an increasing body of data. Rather than considering a new paradigm which might make the evidence sensible, skeptics have demanded that all evidence for 'pre-Clovis' be judged by more rigid standards of evidence and argument than are applied to later sites. . . . Arbitrary application of such rigid criteria to later sites, including Clovis sites, would relegate nearly all archaeological evidence to the 'not proven' category." It should, however, be noted that the European controversy of the nineteenth century, the full dimensions of which Bryan was probably unaware, is, like the debate on the antiquity of humans in the Americas, still very much an open question. The seriousness with which a modern paleoanthropologist might consider reports of stone tools apparently made

by humans in the European Pliocene and earlier is likely to vary in inverse proportion to his commitment to the now-accepted views on human evolution.

Eoliths have been found not only in America and Europe but in Australia (R. A. Gould et al. 1971). They have been described as featuring "the casual use of available materials; the lack of emphasis on technological sophistication; the regular discarding of tools after a specific job had been completed; and an attitude which de-emphasizes symmetry, refinement, and systematic continuity in tool types, but instead focuses on the most convenient means of accomplishing the job at hand" (Alsoszatai-Pettheo 1986, p. 22).

The human manufacture of the Australian specimens has been widely recognized in the scientific community. So why are not similar tools found in America granted equal recognition? Alan Lyle Bryan (1986, pp. 7-8) stated: "some definitely shaped tool (preferably something 'diagnostic') must be present in order to have acceptable 'proof' for the presence of Early Man. Anything less is now being labelled a 'myth,' and believers of myths cannot be scientific archaeologists. But if the Australian archaeologists had adhered to such strict criteria they would not have searched for and thereby recovered evidence for Pleistocene man on that continent. ... It was realized that the only 'diagnostic' artifact categories may be simple flakes and cores. It was realized that simple retouched flakes are adequate to demonstrate the presence of early man, if they are recovered from datable stratigraphic contexts. ... It is illogical to require the presence of diagnostic shaped tools in America and not to require their presence in Australia in order to prove that that continent was populated at least 40,000 years ago." But if simple retouched flakes are adequate to prove the existence of humans 40,000 years ago in Australia, and

200,000 years ago in America, why are they not adequate to prove the existence of toolmaking hominids 2 million years ago in England and even earlier elsewhere?

Obviously, the great mass of evidence for a human presence in the Pliocene and earlier, as presented in this book, does not fit within the narrow limits of current ideas on human evolution. Many will therefore hesitate to even consider such evidence. This being the case, it can be said that evolutionary preconceptions impose unreasonable constraints on what evidence may be introduced into discussions of human origins and antiquity. Evidence is excluded for no other reason than that it violates evolutionary expectations. If one were, however, to give even-handed treatment to all of the available evidence, then it would become impossible to coherently set forth any temporally sequential and physiologically progressive path of hominid development. Only a ruthlessly selective editing of the totality of paleoanthropological evidence allows an evolutionary picture of human origins to be sustained.

### 3.9 A Recent Eolith Discovery from India (Miocene)

We shall conclude our discussion of very crude stone tools, from as far back as the Eocene, with a recent example that shows the relevance of the issues raised in this chapter to modern paleoanthropological research.

K. N. Prasad (1982, p. 101) of the Geological Survey of India wrote in an abstract of his report: "A crude unifacial hand-axe pebble tool recovered from the late Mio-Pliocene (9-10 m.y. b.p.) at Haritalyangar, Himachal Pradesh, India is described. This crude flaked tool is assigned to Ramapithecus. The occurrence of this pebble tool in such ancient sediments indicates that early hominids such as Ramapithecus fashioned tools, were bipedal with erect posture, and probably utilized the implements for hunting." Prasad (1982, p. 102) added: "The implement was recovered in situ, during remeasuring of the geological succession to assess the thickness of the beds. Care was taken to confirm the exact provenance of the material, in order to rule out any possibility of its derivation from younger horizons." He also pointed out that Ramapithecus jaw fragments and teeth were found in the same horizon, the Nagri formation of the Middle Siwaliks.

Describing the tool itself, Prasad (1982, p. 102) stated: "The quartz artefact, heart-shaped (90 mm x 70 mm) [3.6 inches x 2.8 inches] was obviously fabricated from a rolled pebble, the dorsal side of which shows signs of rough flaking. . . . On the ventral side much of the marginal cortex is present at the distal end. Crude flaking has been attempted for fashioning a cutting edge. Marginal flaking at the lateral edge on the ventral side is visible." Prasad reminded his readers that another Indian scientist had recovered stone tools from the lower part of the Pinjor formation, corresponding to the Villafranchian stage of the European Late Pliocene. He then stated: "It is not improbable that fashioning tools commenced even as early as the later Miocene and evolved in a time-stratigraphic period embracing the Astian-Villafranchian" (Prasad 1982, p. 103). We agree, but the real question is the identity of the toolmaker. As we shall see, Ramapithecus has not remained a viable candidate.

Ramapithecus first came to the attention of scientists in the 1930s. This creature, initially regarded as a fossil ape, was named after Rama, an incarnation of God described in the Vedas. In 1964 Ramapithecus achieved worldwide fame when Elwyn Simons and David Pilbeam reconstructed an upper jaw from two fragments, giving it a characteristically human parabolic shape. Simons and Pilbeam pronounced Ramapithecus to be a hominid, an erect, bipedal primate. In 1964, Elwyn Simons wrote in *Anthropology*: "Ramapithecus punjabicus is almost certainly man's forerunner of 15 million years ago. This determination increases tenfold the approximate time period during which human origins can now be traced with some confidence" (Fix 1984, p. 20). This was a bit of an overstatement, because between *Australopithecus* and *Ramapithecus* there was, and still is, a gap of several million years in the hominid fossil record.

In any case, *Ramapithecus* quickly received acclaim, in textbooks and journal articles, as the earliest human ancestor. As Richard Leakey and Roger Lewin wrote in 1977: "*Ramapithecus* ... as far as one can say at the moment ... is the first representative of the human family—the hominids" (Fix 1984, p. 20).

Others, however, maintained a more cautious attitude. In 1972, Maitland A. Edey wrote in *The Missing Link*: "On grounds of pure logic, it is tempting to regard *Ramapithecus* as a sort of proto-*Australopithecine*; after all, the *Australopithecines* had to start somewhere. But, however tempting such an idea may be, it is premature. We have no knowledge whatsoever of the nature

of the rest of Ramapithecus's body. We do not know what its skull was shaped like or how large its brain was. We know nothing about its hand or foot. We do not know if it stood upright" (Fix 1984, p. 21). Herbert Wendt also expressed some doubts in *Ape to Adam*: "Whether Ramapithecus, which some experts think does not really belong to the race of hominids in the narrow sense of the term, was already a tool-maker we do not know" (Fix 1984, p. 21).

In 1979, information confirming the doubtfulness of Ramapithecus appeared in the journal *Natural History*. A. L. Zihlman and J. M. Lowenstein stated that a complete lower jaw of Ramapithecus, the first ever found, was V-shaped, unlike either the human jaw, which has a parabolic shape, or the ape jaw, which has parallel sides (Fix 1984, p. 21). In response, Pilbeam modified his position on Ramapithecus, placing it in a separate category related neither to apes nor humans. But three years later, Ramapithecus's status changed again. William R. Fix (1984, pp. 21-22) wrote: "the February 6, 1982 issue of *Science News* added a new twist to the Ramapithecus story. Compiling information from an article in *Nature* (January 21, 1982) and a telephone interview with Pilbeam, *Science News* now has Ramapithecus as 'part of the orangutan lineage.'" This newly defined Ramapithecus was definitely not a maker of stone tools.

As late as 1981, however, A. R. Sankhyan of the Anthropological Survey of India was writing (1981, pp. 358-359): "The Sivalik Group of rocks exposed in Haritalyangar area of district Bilaspur is famous for the well known Mio-Pliocene Hominoidea—*Dryopithecus*, *Gautopithecus*, and *Ramapithecus*, the last of which is considered as the earliest hominoid ancestor of man and also believed to be an ad hoc toolmaker. "

But a short time later this view was history. R. N. Vasishat, an anthropologist at Punjab University, wrote (1985, p. xiv): "Until the year 1982, scientists all over the world had unanimously been considering the genus *Ramapithecus* to be the earliest known hominid in the world and [it] was also presumed to be ancestral to *Australopithecus* and *Homo*. When this species was taken out of the family Hominidae, the Siwaliks became devoid of any evidence for the antecedents of Early Man. But the author is very sure, the void thus created is very temporary and there is no reason for us to believe that the Siwaliks will never yield fossil evidence or physical evidence of Early Man in the future."

It is interesting to note that with *Ramapithecus* demoted, the Siwaliks became "devoid of any evidence" for Early Man. But what about the above-mentioned stone tools reported by Prasad and Verma? Were these not still evidence? Here is yet another example of the curious manner in which scientists treat anomalous discoveries. Prasad's discovery of a Miocene implement is particularly significant in that it shows that evidence of the type reported by nineteenth-century scientists is still turning up and still being subjected to the same unfair treatment.

#### Crude Paleolithic Stone Tools

In the previous chapter, we considered anomalous stone tools of the crudest type, the eoliths. We shall now turn our attention to other stone tools, which, although also crude when compared with

the sophisticated implements of the conventional Late Stone Age, represent an advance over the eoliths. These we have chosen to designate as crude paleoliths.

For some researchers, the terms eolith and paleolith represent a chronological succession, but we use these terms principally to make a distinction in the morphology of tool types. Eoliths, it may be recalled, are naturally broken pieces of stone that are used as tools with little or no further modification. A working edge might be retouched and show signs of wear. Paleoliths, however, are often deliberately flaked from stone cores and then more extensively modified.

As we have previously mentioned, arriving at clear-cut distinctions between eoliths and crude paleoliths is not always possible. Furthermore, a particular group of discoveries often includes implements of various levels of sophistication. In making decisions about what industries to put in this chapter, we have been guided by statements of scientists who favorably compared individual implements, and groups of implements, to recognized tools from much later periods. Anomalously old stone tool industries containing a good many implements comparable to the cruder kinds of classical Paleolithic implements have been selected for inclusion.

#### 4.1 The Finds of Carlos Ribeiro in Portugal ( Miocene)

We first turn our attention to Carlos Ribeiro's discoveries in the Miocene of Portugal. The first hint of Ribeiro's work came to our attention quite accidentally. While going through the writings of the nineteenth-century American geologist J. D. Whitney, who reported evidence for Tertiary human beings in California, we encountered a sentence or two about Ribeiro having discovered flint implements in Miocene formations near Lisbon. We found more brief mentions in the works of S. Laing, a popular English science writer of the late nineteenth century. Curious, we searched libraries, but turned up no works under Ribeiro's name and found ourselves at a dead end. Sometime later, Ribeiro's name turned up again, this time in the 1957 English edition of *Fossil Men* by Boule and Vallois, who rather curtly dismissed the work of the nineteenth-century Portuguese geologist. We were, however, led by Boule and Vallois to the 1883 edition of *Le Préhistorique*, by de Mortillet, who gave a favorable report of Ribeiro's discoveries, in French. By tracing out the references mentioned in de Mortillet's footnotes, we gradually uncovered a wealth of remarkably convincing original reports in French Journals of archeology and anthropology from the latter part of the nineteenth century. The search for this buried evidence was very illuminating, demonstrating how the scientific establishment treats reports of facts that no longer conform to accepted views. Keep in mind that for most current students of paleoanthropology, Ribeiro and his discoveries simply do not exist. You have to go back to textbooks printed over 30 years ago to find even a mention of him. Did Ribeiro's work really deserve to be buried and forgotten? We shall present the facts and allow readers to form their own conclusions.

##### 4.1.1 A Summary History of Ribeiro's Discoveries

Carlos Ribeiro was not an amateur. In 1857, he was named to head the Geological Survey of Portugal, and he would also be elected to the Portuguese Academy of Sciences. During the years 1860- 63, he conducted studies of stone implements found in Portugal's Quaternary strata.

Nineteenth-century geologists generally divided the geological periods into four main groups: (1) the Primary, encompassing the periods from the Precambrian through the Permian; (2) the Secondary, encompassing the periods from the Triassic through the Cretaceous; (3) the Tertiary, encompassing the periods from the Paleocene through the Pliocene; and (4) the Quaternary, encompassing the Pleistocene and Recent periods. During the course of his investigations, Ribeiro learned that flints bearing signs of human work were being found in Tertiary beds between Canergado and Alemquer, two villages in the basin of the Tagus River, about 35- 40 kilometers (22- 25 miles) northeast of Lisbon.

Ribeiro immediately began his own investigations, and in many localities found "flakes of worked flint and quartzite in the interior of the beds." Ribeiro (1873a, p. 97) said: "I was greatly surprised when I forcefully extracted, with my own hand, worked flints, from deep inside a bed of limestone which had been inclined at an angle of 30-50 degrees from the horizontal. " The geology of the region indicated the limestone bed was of Tertiary age, yet the presence of the stone implements, so obviously the work of humans, placed Ribeiro in a dilemma. The discovery of the implements "deep inside" the beds seemed to rule out the possibility that they had been artificially introduced at some later period. So if he accepted the beds as Tertiary, then humans must have existed during that time. But Ribeiro felt he must submit to the prevailing scientific dogma that human beings were not older than the Quaternary. To this very day authorities hold that humans of the modern type did not appear until the very latest part of the Pleistocene. So Ribeiro looked for and found a way to designate the limestone formation as Quaternary. He remained troubled at heart, however, for the geological facts he himself had observed were leading him to the forbidden conclusion that humans had existed in times more ancient than the Quaternary (Ribeiro 1873a, p. 97).

In 1866, on the official geological maps of Portugal, Ribeiro reluctantly assigned Quaternary ages to certain of the implement-bearing strata. Upon seeing the maps, the French geologist de Verneuil took issue with Ribeiro's judgement, pointing out that the so-called Quaternary beds were, according to geological evidence, certainly Pliocene or Miocene.

Meanwhile, in France, the Abbe Louis Bourgeois, a reputable investigator, had reported finding stone implements in Tertiary beds, and some authorities had supported him. Thus, under the twin influences of de Verneuil's criticism and the discoveries of Bourgeois, Ribeiro resolved his inner conflict and decided that the geological and paleontological facts could no longer be ignored. He began openly reporting that implements of human manufacture were being found in Pliocene and Miocene formations in Portugal (Ribeiro 1873a, p. 98).

From the standpoint of modern geology, Ribeiro's assessment of the age of the formations in the Tagus River valley near Lisbon is generally correct. Modern authorities have observed seven Miocene cycles of sedimentation and one Pliocene cycle (Antunes et al. 1980, p. 136). The Late Tertiary (including the Pliocene and Miocene) is sometimes called the Neogene. In a study focusing on the Neogene formations of Europe, Ivan Chicha (1970, p. 50) said about Portugal: "The Neogene beds are known from the basin situated in the lower reach of the river Tejo [Tagus], in

the environs of Lisbon. The Oligocene beds, prevalently of freshwater continental origin . . . are overlain by beds . . . which are placed in the oldest Miocene—Aquitainian.” According to Chicha, these Aquitainian beds are surmounted by limestones and claystones that ascend to the Tortonian stage of the Late Miocene. Another recent study (Antunes et al. 1980, p. 138) included a chart showing the lithostratigraphic units in the Tagus basin. Limestones, such as those in which Ribeiro found stone tools, occur in the Middle and Early Miocene.

In considering stone implements, three questions must be answered: (1) is the specimen really of human manufacture? (2) has the age of the stratum in which it was discovered been properly determined? (3) was the implement incorporated into the stratum at the time the stratum was laid down, or was the implement introduced at a later date? As far as Ribeiro was concerned, he was convinced that he had satisfactorily answered all three questions. The toollike flint objects he studied were of human manufacture, they were found in strata mostly of Miocene age, and many appeared to be in primary position, although some of his specimens were found on the surface.

In 1871, Ribeiro presented to the members of the Portuguese Academy of Science at Lisbon a collection of flint and quartzite implements, including those gathered from the Tertiary formations of the Tagus valley. In 1872, at the International Congress of Prehistoric Anthropology and Archeology meeting in Brussels, Ribeiro gave a similar report on his discoveries and displayed more specimens, mostly pointed flakes. At that time, Bourgeois said that none appeared to be of human manufacture. Upon a new examination of Ribeiro's specimens, Bourgeois found one flint that he thought displayed signs of human work, but unfortunately it had not been found in situ. He therefore suspended judgement (de Mortillet 1883, p. 95). The English authority, AW. Franks, who served as Conservator of National Antiquities and Ethnography at the British Museum, gave a more positive opinion. An expert in cultural remains, including tools, Franks stated that some of the specimens did appear to be the product of intentional work, but he reserved judgement on the age of the strata in which they had been found (Ribeiro 1873a, p. 99).

Ribeiro himself (1873 b, p. 100) then addressed the Congress on the question of “the exact geological situation of the beds in which he had found worked flint flakes, the authenticity of which has been recognized by Mr. Franks and other members of the Congress.” Ribeiro reported that one of the flints had been found in the reddish-yellow Pliocene sandstone on the left bank of the Tagus, to the south of Lisbon. He noted that these beds cover Miocene marine deposits (Ribeiro 1873b, p. 101). Modern authorities (Antunes et al. 1980, pp. 136-138) still show this basic sequence—Miocene marine deposits surmounted by Pliocene sandstone formations—in the Lisbon region.

"Concerning the other flints which Mr. Franks has declared bear evident traces of human workmanship," said Ribeiro (1873b, p. 102), "they were found in Miocene strata." He explained that on the way north from Lisbon to Caldas da Rainha, between the towns of Otta and Cercal, one comes to the steep hill of Espinhac;o de Cao. According to Ribeiro (1873b, p. 102), it was in the sandstone beds of this hill, which lie under marine Miocene strata, that he found 'flints worked by the hand of man before they were buried in the deposits.' This would indicate the presence of



human beings in Portugal at least 5 million years ago and perhaps as much as 25 million years ago. Figure 4.1 shows an implement from Espinhaço de Cao.

Ribeiro's Miocene flints made an impressive debut at Brussels, but remained controversial. At the Paris Exposition of 1878, Ribeiro displayed 95 specimens of Tertiary flint tools in the gallery of anthropological science. De Mortillet visited Ribeiro's exhibit and, in the course of examining the specimens carefully, found that 22 had indubitable signs of human work. This was quite an admission for de Mortillet, for, as described in Chapter 2, he habitually rejected all evidence for human work on incised and broken bones from the Tertiary.

Figure 4.1. Implement found by Carlos Ribeiro, of the Geological Survey of Portugal, in a Miocene layer at Espinhaço de Cao (G. de Mortillet and A. de Mortillet 1881, plate 3). The ventral surface shows: (1) a striking platform, (2) bulb of percussion, and (3) *écaillage*.

Gabriel de Mortillet, along with his friend and colleague Emile Cartailhac, enthusiastically brought other paleoanthropologists to see Ribeiro's specimens, and they were all of the same opinion—a good many of the flints were definitely made by humans. Cartailhac then photographed the specimens, and de Mortillet later presented the pictures in his *Musée Préhistorique* (1881).

De Mortillet (1883, p. 99) wrote: "The intentional work is very well established, not only by the general shape, which can be deceptive, but much more conclusively by the presence of clearly evident striking platforms and strongly developed bulbs of percussion." The bulbs of percussion also sometimes had *écaillures*, small chips removed by the force of impact. In addition to the striking platform, bulb of percussion, and *écaillure*, some of Ribeiro's specimens had several long, vertical flakes removed in parallel, something not likely to occur in the course of random battering by the forces of nature.

De Mortillet's method of analysis is comparable to that employed by modern experts in lithic technology, who, like de Mortillet, emphasize that the toollike shape of a flint does not in itself establish human work. Leland W. Patterson, a contemporary expert in distinguishing artifacts from naturefacts, believes that the bulb of percussion is the most important sign of intentional work on a flint flake. If the flake also shows the remnants of a striking platform, then one can be even more certain that one is confronted with a flake struck deliberately from a flint core and not a piece of naturally broken flint resembling a tool or weapon.

"There can be no doubt," wrote de Mortillet (1883, p. 99) about Ribeiro's stone implements. "The diverse specimens are formed from big flakes, almost all of them triangular and without retouch, some in flint, some in quartzite. In looking at the collection, one believes oneself to be seeing Mousterian tools, only somewhat coarser than usual." Mousterian is the name given to the type of stone tool usually considered to have been made by the Neanderthals (*Homo sapiens neanderthalensis*), who are thought to have lived in the latter part of the Pleistocene. By making the comparison with the Late Pleistocene

Mousterian Implements, de Mortillet was pointing out that Ribeiro's specimens almost exactly resemble those that are universally acknowledged as being of human manufacture. Figure 4.2 shows one of Ribeiro's Miocene tools from Portugal and for comparison an accepted stone tool from the Mousterian cultural stage of the European Late Pleistocene. They share the typical features of intentional human work on stone: the striking platform, bulb of percussion, *écaillage*, and parallel removal of flakes.

De Mortillet (1883, pp. 99-100) further observed: "Many of the specimens, on the same side as the bulb of percussion, have hollows with traces and fragments of sandstone adhering to them, a fact which establishes their original position in the strata. The sandstone is inserted among strata of clays and limestones in the valley of the Tagus, together comprising a formation that attains in some places a depth of 400 meters [over 1,300 feet]. The beds have been dislocated and are in some places now resting almost in a vertical position. It is very evidently Tertiary terrain. Of the 22 worked specimens, 9 are indicated by Ribeiro to be Miocene. The others are Pliocene."

Plate 3 in de Mortillet's publication *Musée Préhistorique* (G. de Mortillet and A. de Mortillet 1881) featured illustrations of Ribeiro's Miocene and Pliocene discoveries. We have selected two for reproduction. Figure 4.3 depicts both sides of a flint flake recovered from a Tertiary formation at the base of Monte Redondo. This formation is said to belong to the Tortonian stage of the Late Miocene (de Mortillet 1883, p. 102). The ventral surface of the flint flake shows "a large striking platform, bulb of percussion, and *écaillage*" (G. de Mortillet and A. de Mortillet 1881, plate 3). The dorsal surface of the flake bore proof that it was found in the Tertiary sandstones of Ota. Sandstone, just like that found at the base of Monte Redondo, adhered to the surface.

Figure 4.2. Left: Dorsal and ventral views of a stone tool recovered from a Tertiary formation in Portugal (de Mortillet 1883, p. 98). It would be over 2 million years old. Right: An accepted stone tool, less than 100,000 years old, from the Mousterian cultural stage of the European Late Pleistocene (de Mortillet 1883, p. 81). Both implements clearly display the following features of intentional human work: (1) striking platforms, (2) *écaillures*, (3) bulbs of percussion, and (4) parallel flake removal.

Figure 4.3. Ventral and dorsal surfaces of a flint tool found in a Late Miocene formation at Monte Redondo, Portugal (G. de Mortillet and A. de Mortillet 1881, plate 3).

The quartzite flake shown in Figure 4.4 was found in a Pliocene formation at Barquinha, 103 kilometers (about 64 miles) northeast of Lisbon, Portugal. The ventral surface of the flake displays a striking platform, bulb of percussion, and enlure (G. de Mortillet and A. de Mortillet 1881, plate 3). While this flake was still attached to the quartzite core, another flake was struck from it, as shown by a negative bulb of percussion on the dorsal surface of the flake.

In a report published in 1879, Cartailhac <sup>1 2</sup> said about some of Ribeiro's specimens: "One would believe himself to be

viewing a series of Mousterian stone implements, though somewhat cruder. The bulbs of percussion are 3 generally quite prominent. \_ . These pieces bear the proof that they were not found on the surface,

Figure 4.4. Quartzite tool found in a Pliocene formation at Barqwnha, Portugal (G. de Mortillet and A. de Mortillet 1881, plate 3). The ventral surface (left), shows (1) a striking platform. (2) bulb of percussion, and (3) enlèvement.

Figure 4.5. An implement found in a Miocene formation at Carregado, Portugal (Cartailhac 1879, plate 8).

On the faces of the flakes and in the hollows are found fragments of the sandstone which had encased them" (Cartailhac 1879, p. 439). One of the pieces (Figure 4.5) was found at Carregado in a Miocene formation and was described by Cartailhac as displaying "a bulb of percussion and retouch." Retouching. In the form of regular chipping along the edges of a flint flake. Is a good Indicator of Intentional work.

#### 4.1.2 An International Committee Vindicates Ribeiro

At the 1880 meeting of the International Congress of Prehistoric Anthropology and Archeology, which was held in Lisbon, Portugal, Ribeiro, now on his home ground, delivered another report and displayed more specimens that were "extracted from Miocene beds" (1884, p. 86). In his report ("L'homme Tertiaire en Portugal"), Ribeiro (1884, p. 88) stated: "The conditions in which the worked flints were found in the beds are as follows: (1) They were found as integral parts of the beds themselves. (2) They had sharp, well-preserved edges, showing that they had not been subject to transport for any great distance. (3) They had a patina similar in color to the rocks in the strata of which they formed a part."

The second point is especially important. Some geologists claimed that the flint implements had been introduced into Miocene beds by the floods and torrents that periodically washed over this terrain. According to this view, Quaternary flint implements may have entered into the interior of the Miocene beds through fissures and been cemented there, acquiring over a long period of time the coloration of the beds (de Quatrefages 1884, p. 95). But if the flints had been subjected to

such transport, then the sharp edges would most probably have been damaged, and this was not the case.

The Congress assigned a special commission of scientists the task of directly inspecting the implements and the sites from which they had been gathered. In addition to Ribeiro himself, the commission included G. Bellucci of the Italian Society for Anthropology and Geography; G. Capellini, from the Royal University of Bologna, Italy, and known to us from Chapter 2 for his discoveries of incised Pliocene whale bones; E. Cartailhac, of the French Ministry of Public Instruction; Sir John Evans, an English geologist; Gabriel de Mortillet, professor of prehistoric anthropology at the College of Anthropology, Paris; and Rudolph Virchow, a German anthropologist. The other members were the scientists Choffat, Cotteau, Villanova, and Cazalis de Fondouce.

On September 22, 1880, at six in the morning, the gentlemen of the commission boarded a special train and proceeded north from Lisbon. During the rail journey, they gazed at the old forts topping the hilltops, and pointed out to each other the Jurassic, Cretaceous, and Tertiary terrains as they moved through the valley of the Tagus River. They stepped off the train at Carregado. It is on a line from Carregado north to Cercal that Ribeiro discovered most of his flints. They then proceeded to nearby Ota and two kilometers (just over a mile) from Ota arrived at the hill of Monte Redondo. At that point, the scientists dispersed into various ravines in search of flints.

Paul Choffat, a member of the commission and its secretary, later reported to the Congress: "Of the many flint flakes and apparent cores taken from the midst of the strata under the eyes of the commission members, one was judged as leaving no doubt about the intentional character of the work" (1884a, p. 63). This was the specimen found in situ by the Italian naturalist Bellucci (Figure 4.6). Choffat then noted that Bellucci had found on the surface other flints with incontestable signs of work. Some thought they were Miocene implements that had been removed from the Miocene conglomerates by atmospheric agencies, such as rain and wind, while others thought that the implements were of a much more recent date.

Figure 4.6. Flint implement found by G. Bellucci in an Early Miocene formation at Ota, Portugal (Choffat 1884b, figure I). It was judged by a commission of scientists to be identical to Late Pleistocene implements of similar type.

In his book *Le Préhistorique*, Gabriel de Mortillet gave an informative account of the events that took place at the Congress at Lisbon: "While the printer was preparing the first pages of this book," wrote de Mortillet (1883, p. 100), "I went to the meeting of the International Congress of Prehistoric Anthropology and Archeology In Lisbon, one of my purposes being to complete the table of strata containing evidence for the presence of humans. I was able to confirm in a very exact and positive manner the actuality of Ribeiro's discoveries, including the precise geological position of certain of his worked flints." De Mortillet (1883, pp. 100 -101) then proceeded to describe the scientists' excursion to Otta and Bellucci's remarkable discovery: "The members of the Congress arrived at Otta, in the middle of a great freshwater formation. It was the bottom of an ancient lake, with sand and clay in the center, and sand and rocks on the edges. It is on the shores that intelligent beings would have left their tools, and it is on the shores of the lake that once bathed Monte Redondo that the search was made. It was crowned with success. The able Investigator of Umbria [Italy], Mr. Bellucci, discovered in situ a flint bearing incontestable signs of intentional work. Before detaching it, he showed it to a number of his colleagues. The flint was strongly encased in the rock. He had to use a hammer to extract it. It is definitely of the same age as the deposit. Instead of lying flat on a surface onto which it could have been secondarily recemented at a much later date, it was found firmly in place on the under side of a ledge extending over a region removed by erosion [ Figure 4.7]. It is impossible to desire a more complete demonstration attesting to a flint's position in its strata." All that was needed was to determine the age of the strata. Study of the fauna and flora in the region around the Monte Redondo site showed that the formations present there can be assigned to the Tortonian stage of the Late Miocene period (de Mortillet 1883, p. 102).

"Therefore," concluded de Mortillet "during the Tortonian epoch there existed in Portugal an intelligent being who chipped flint just like Quaternary humans" (1883, p. 102). Some modern authorities consider the Otta conglomerates to be from the Burdigalian stage of the Early Miocene (Antunes et al. 1980, p. 139).

Figure 4.7. Stratigraphy of the site at the base of Monte Redondo hill in Otta, Portugal, where G. Bellucci found the implement pictured in Figure 4.6: (1) sandstone; (2) Miocene sandstone conglomerate with flints; (3) surface deposit of eroded flints. The arrow marked "X" indicates the position of the implement (de Mortillet 1883, p.

101).

Choffat (1884b, pp. 92- 93) presented, in the form of answers to four questions, the conclusions of the commission members, who had not only examined the specimens Ribeiro exhibited at the

Congress but also journeyed to Otta to conduct field investigations. The first two questions and answers dealt with the flints themselves: "(1) Are there bulbs of percussion on the flints on exhibition and on those found during the excursion? The commission declares unanimously that there are bulbs of percussion. and some pieces have several. (2) Are bulbs of percussion proof of Intentional work? There are different opinions. They may be summarized as follows: de Mortillet considers that just one bulb of percussion is sufficient proof of Intentional work, while Evans believes that even several bulbs on one piece do not give certitude of Intentional chipping but only a great probability of such." Here it may once more be noted that modern authorities such as Leland W. Patterson (1983) consider one or more bulbs of percussion to be very good indicators of Intentional work.

The remaining two questions concerned the positions in which the flints were found: "(3) Are the worked flints found at Otta from the interior of the beds or the surface? There are diverse opinions. Mr. Cotteau believes all are from the surface, and that those found embedded within the strata came down through crevasses in the beds. Mr. Capellini, however, believes that pieces found on the surface were eroded from the interior of the beds. De Mortillet, Evans, and Cartailhac believe there are two time periods to which the flints may be referred, the first being the Tertiary, the other being the Old and New Stone Ages of the Quaternary. The flints of the two periods are easy to distinguish by their form and patina. (4) What is the age of the strata of the worked flints? After only a moment's discussion the members declared they were in perfect accord with Ribeiro." In other words, the strata were Miocene, although some members of the commission believed that the flints found lying on the surface had not weathered out of the Miocene rock but instead had been dropped there in fairly recent times.

In the discussion that followed the presentation of Choffat's report, Capellini said: "I believe these flints to be the product of Intentional work. If you do not admit that, then you must also doubt the flints of the later Stone Ages" (Choffat 1884b, pp. 97-98). According to Capellini, Ribeiro's Miocene specimens were almost identical to undoubted Quaternary flint implements. Capellini's remarks strike at one of the central issues in the treatment of scientific evidence—the application of a double standard in determining what evidence is to be accepted and what is to be rejected. If the standards used by the scientific establishment to reject finds such as Ribeiro's were applied in the same manner to conventionally accepted finds, then the accepted finds would also have to be rejected. And this would deprive the theory of human evolution of a substantial portion of its evidential foundation.

The next speaker, Villanova, provided a good example of the double standard treatment. Villanova was very doubtful, even about the Bellucci find. He said that in order to remove all cause for suspicion one would have to discover an unmistakably genuine implement firmly embedded not in a Miocene conglomerate but in the middle of undifferentiated Miocene limestone and alongside characteristic fossils (Choffat 1884b, p. 99). A conglomerate is a mass of rock composed of rounded stones of various sizes cemented together in a matrix of sandstone or hardened clay. Apparently, Villanova felt there was some reason to doubt the age of a stone tool found in a conglomerate—there was perhaps a chance it had entered recently and been cemented in with

other stones. Or perhaps he doubted the age of the conglomerate at Otta, but the majority opinion was that this conglomerate was in fact Miocene.

Maybe it would have been better if the flint had been found in an undifferentiated stratum. The crucial point, however, is this: If Villanova's criterion were to be applied in all cases, this would wipe out most of the paleoanthropological evidence now accepted. The number of human fossils found in undifferentiated strata directly alongside characteristic fossils is rather small. For example, as we shall see in Chapter 9, the initial Java *Homo erectus* discovery was made in strata that had undergone considerable mixing, and almost all of the later Java *Homo erectus* finds were made on or quite near the surface. Beijing *Homo erectus* was found in cave deposits. Another point that will emerge in our discussion is this: sometimes anomalous finds are made in undifferentiated strata alongside characteristic fossils, and then some other means will be found to discredit them. Indeed, as previously mentioned, in his report to the International Congress of Prehistoric Anthropology and Archeology at Brussels in 1872, Ribeiro (1873a, p. 97) did tell of finding flint implements "deep inside" undifferentiated Miocene limestone beds.

Following Villanova, Cartailhac spoke. He said that if the question of the Miocene age of the implements were to be decided on the grounds of actual scientific evidence, the answer would have to be affirmative. Cartailhac believed that the coloration of many of the surface finds indicated they were eroded from Miocene beds, and he pointed out that some specimens had remnants of Miocene sediments adhering to them.

Cartailhac then asked the members to consider a particular specimen from Ribeiro's collection, which he had previously studied at the anthropological exposition in Paris. He stated: "I have seen on it two bulbs of percussion, and possibly a third, and a point that seems to truly be the result of intentional work. It has on its surface not a coloration that could be removed by washing but rather a surface incrustation of Miocene sandstone tightly adhering to it. A chemist would not permit us to say that such a deposit could form and attach itself to a flint lying, for whatever amount of time, on a sandstone surface" (Choffat 1884b, p. 100). In other words, the flint must have been lying within the Miocene bed itself, when it was formed. Cartailhac admitted that natural action might in rare occasions produce a bulb of percussion, but to have two on the same piece would be an absolute miracle. He believed that the many very good specimens discovered on the Miocene surface, where there was absolutely no trace of any other deposit, were really Miocene implements that had weathered out of the rock.

One may certainly disagree with Cartailhac. But then we may here note that in more recent times, the famous Lucy australopithecine fossils were found by D. C. Johanson on the surface of Pliocene deposits in Ethiopia, from which they were presumed to have weathered out (Johanson and Edey 1981, pp. 16-18). As we shall see in Chapter 9, the same is also true of many of the Java *Homo erectus* finds. Are these discoveries also to be doubted? Perhaps, but the real point is that the application of standards should be consistent. Unfortunately, as we shall see throughout this book, standards tend to be applied selectively, in conformity with the biases and expectations of the researcher.



After Cartailhac finished his remarks, Bellucci recounted his own noteworthy discovery of an Implement (Figure 4.6, p. 221) in the Miocene conglomerate at Otta (Choffat 1884b, pp. 101-102). Before extracting it, he had shown it to many members of the commission, who saw that it was firmly integrated into the stratum (Figure 4.7, p. 222). It had been so firmly fixed in the Miocene sandstone conglomerate that he had not been able to remove it with his wooden tool, and had needed to use Cartailhac's iron pick to break the sandstone. Bellucci stated that the inner surface of the implement, the one adhering to the conglomerate, had not only the same reddish color as the conglomerate but also incrustations of tiny grains of quartzite that could not be detached even by vigorous washing.

Bellucci further pointed out that the elements composing the intact conglomerate corresponded perfectly with those found loose on the surface. This led Bellucci to conclude that the loose stones found on the surface at Otta were the result of weathering of the conglomerate. This indicated that flint implements found on the surface might also have come from the conglomerate, which was of Miocene age (Choffat 1884b, p. 103). By itself, this was, however, a fairly weak argument. Although the flints on the surface may have weathered out of the Miocene conglomerate, they also could have been dropped on the surface during the Late Pleistocene. But the fact that the implements had incrustations of Miocene sediments on them, and were the same color as the Miocene conglomerate, strongly supported the conclusion that the implements were themselves Miocene.

As for the signs of intentional work on the piece found in situ, Bellucci noted: "Mr. Evans says he believes in bulbs of percussion. Well look. This piece was detached from the surface of a flint core, and it not only has a magnificent bulb of percussion, but also one of its surfaces presents marks showing that another flake had been previously detached, in the same direction, when the implement had been still part of the flint nucleus" (Choffat 1884b, p. 104).

The last feature described by Bellucci, successive parallel flake removal from a core, is recognized today by experts in lithic technology as one of the surest signs of intentional work. The striking of two successive flakes from a flint core requires a considerable degree of expertise, and is quite beyond what might be expected from random shocks by purely natural forces. Patterson stated: "Humans will often strike multiple flakes in series from a single core, usually resulting in the production of some flakes with multiple facets on the dorsal face. In contrast, the removal of a few flakes from cores by random natural forces would not be expected to occur often by serial removals. ... It is characteristic in human lithic manufacturing processes to use the same striking platform for multiple flake removals" (L. Patterson et al. 1987, p. 98).

When Cotteau's turn to speak came, he argued, like Villanova, that, in order to be accepted, finds of implements should be made only in undifferentiated, intact strata (Choffat 1884b, pp. 105 - 106). Cotteau observed that unless finds were made in undifferentiated, intact strata, the possibility always existed that the implements might have been washed in through fissures from the surface and cemented in place. In time, the fissure might be filled in, hiding its existence to researchers. It should, however, be noted, that Cotteau did not specifically address the conditions

of Bellucci's discovery. Was there in fact a filled-in fissure near the place where Bellucci found the flint implement? Cotteau does not say. Furthermore, the position in which Bellucci found his implement, firmly in place on the underside of an overhanging section of the Miocene formation, argues against Cotteau's hypothesis. In general, Bellucci's opponents at the Congress offered only vague hypothetical objections.

Altogether, there seems little reason why Ribeiro's discoveries should not be receiving some serious attention, even today. Here we have a professional geologist, the head of Portugal's geological survey, making discoveries of flint implements in Miocene strata. In appearance the implements resembled accepted types, and they displayed characteristics that modern experts in lithic technology accept as signs of human manufacture. To resolve controversial questions, a congress of Europe's leading archeologists and anthropologists deputed a committee to conduct a firsthand investigation of one of the sites of Ribeiro's discoveries of Miocene implements. There a scientist discovered in situ an implement in a Miocene bed, a fact witnessed by several other members of the committee. Of course, objections were raised, but upon reviewing them, it does not appear to us that they were conclusive enough to cause an unbiased observer to categorically reject Bellucci's find in particular or Ribeiro's finds in general.

#### 4.2 The Finds of The Abbe Bourgeois at Thenay, France (Miocene)

We now turn our attention to the discoveries of the Abbe L. Bourgeois, rector of the seminary at Pontlevoy, Loire-et-Cher, France. On August 19, 1867, In Paris, Bourgeois presented to the International Congress for Prehistoric Anthropology and Archeology a report on flint Implements he had found in Early Miocene beds at Thenay, in north central France, near Orleans (de Mortillet 1883, p. 85). Bourgeois, who had conducted research near Thenay for over twenty years, said that although the instruments were crudely made, they resembled the types of Quaternary implements (scrapers, borers, blades, etc.) he had found on the surface in the same region. He found on almost all of the Miocene specimens the standard indications of human work: fine retouching, symmetrical chipping, and traces of use. He also noted multiple examples of particular forms. Some of the flints, naturally translucent, were opaque, a sign that they had been burned. By performing experiments with fire and flint, Bourgeois had been able to reproduce the exact effect. The signs of fire on the flints were another strong indication that humans had made and used them.

The flint implements of Thenay were recovered from below the Calcaire de Beauce, a well-known Early Miocene limestone formation. Bourgeois recognized that the presence of stone tools in this geological position was indeed remarkable, having serious implications with regard to human antiquity. Yet, for him, the facts, uncomfortable though they might be to contemplate, spoke for themselves. De Mortillet (1883, p. 86) said that the layers of clay in which the flints were found were of Early Miocene or even Oligocene age. This would push back the presence of human beings in France to around 20-25 million years before the present. If this sounds Impossible, one should ask oneself why. If the answer is that modern science's Ideas about human evolution prevent one from seriously considering such a thing, one should honestly admit that one is allowing

preconceived notions to unduly influence one's perception of facts and that this is unscientific. One with faith in the scientific method should maintain a willingness to change one's notions, even the most dearly held, in the face of facts that contradict them.

Modern geologists still agree with the determination that the deposits at Thenay are Miocene. As stated above, the Implement-bearing layers lie below the Calcaire de Beauce. This limestone formation is now referred to the Aquitanian stage (Pomerol and Feugeur 1974, p. 142), which lies within the Early Miocene (Romer 1966, p. 334). Some French authorities (Klein 1973, p. 566) put the deposits of Thenay at the base of the Helvetian stage. The Helvetian stage is placed in the Middle Miocene (Romer 1966, p. 334). The base of the Helvetian would thus mark the boundary between the Middle and Early Miocene.

#### 4.2.1 Debates About the Discoveries at Thenay

Bourgeois displayed his specimens at the house of the Marquis de Vibraye, and the members of the Paris congress of 1867 were allowed to examine them at their leisure. Although the form and appearance of the flints had been sufficient to convince Bourgeois they were of human manufacture, most of the visitors were hesitant to acknowledge this. De Mortillet (1883, p. 86) stated that "the ancient age of the strata in which they were found involuntarily indisposed the geologists and paleontologists." Here again we find a clear case of preconceptions (of what could and should be) dominating a decision whether or not to accept evidence.

Thus the flints from the Miocene of Thenay did not win much approval at their Paris debut. Only a few scientists, prominent among them the Danish naturalist Worsaae, admitted they were actual artifacts. Undeterred, Bourgeois continued his work, finding more and more specimens, and convincing individual paleontologists and geologists they were the result of intentional work. De Mortillet said he was one of the first to be so convinced. He and other scientists not only examined the collection of Bourgeois at Pontlevoy but also carefully studied the site at Thenay.

Some scientists questioned the stratigraphic position in which the flints had been found. The first specimens collected by Bourgeois, many of which showed signs of burning by fire, came from the slopes of rocky debris along the sides of a small valley cutting through the plateau at Thenay. Geologists such as Sir John Prestwich objected that these were essentially surface finds. In response, Bourgeois dug a trench in the valley and found flints showing the same signs of human work (de Mortillet 1883, p. 94).

Still unsatisfied, critics proposed that the flints found in the trench had come to their positions through fissures leading from the top of the plateau, where Quaternary Implements were often found. To meet this objection, Bourgeois, in 1869, sank a pit into the top of the plateau (de Mortillet 1883, p. 95). In the course of the excavation, he came to a layer of limestone 32 centimeters (about one foot) thick, with no fissures through which Quaternary stone tools might have slipped to lower levels.

Deeper In his pit, at a depth of 4.23 meters (13.88 feet) in Early Miocene strata of the Aquitanian stage, Bourgeois discovered many flint tools. De Mortillet (1883, pp. 95-96) stated in *Le Prérhistorique*: "There was no further doubt about their antiquity or their geological position." In the layer of Early Miocene clay containing the flint Implements, Bourgeois found a hammer stone bearing evident signs of percussion. Hammer stones are primarily used to strike flakes from flint cores. In his collection, Bourgeois (1873, p. 90) had several other examples of hammer stones.

Despite the clear demonstration provided by the pit sunk in the middle of the plateau at Thenay, many scientists retained their doubts. A showdown came in Brussels, at the 1872 meeting of the International Congress of Prehistoric Anthropology and Archeology. There Bourgeois

Figure 4.8. A pointed implement from a Miocene formation at Thenay, France (Bourgeois 1873, plate I).

delivered a report summarizing the history of his discoveries. In addition, he presented many specimens, figures of which were included in the published proceedings of the Congress. Describing a pointed specimen (Figure 4.8), Bourgeois (1873, p. 89) stated: "Here is an awllike specimen, on a broad base. The point in the middle has been obtained by regular retouching. This is a type common to all epochs. On the opposite side is a bulb of percussion. which although rare in the Tertiary flints of The-nay, here shows itself very well."

Bourgeois described another implement: "A very regularly shaped fragment of a flake that deserves the designation knife or cutter." He continued: "The edges have regular retouching, and the opposite side presents a bulb of percussion" (Bourgeois 1873, p. 49). On many of his specimens, noted Bourgeois, the edges on the part of the tool that might be grasped by the hand remained unworn, while those on the cutting surfaces showed extensive wear and polishing.

Another specimen (Figure 4.9). was characterized by Bourgeois (1873, p. 89) as a projectile point or an awl.

Figure 4.9. A pointed artifact from Miocene strata at Thenay, France, with retouching near the point (Bourgeois 1873, plate 2).

He noted the presence of retouching on the edges, obviously intended to make a sharp point. Bourgeois (1873, p. 89) also saw among the objects he collected "a core with the two extremities retouched with the aim of being utilized for some purpose." He observed: "The most prominent edge has been chipped down by a series of artificial blows, probably to prevent discomfort to the hand grasping the implement. The other edges remain sharp, which shows this flaking is not due

to rolling action” (Bourgeois 1873, p. 89). If the flint had been subjected to transport by water or another natural agency, one would expect that the resultant random chipping and fracturing should have damaged all the edges, and not just one. For the sake of comparison, we show in Figure 4.10 the Implement from the Early Miocene of Thenay alongside a similar accepted Implement from the Late Pleistocene.

Then Bourgeois (1873, p. 90) described a final specimen: "A short scraper, with numerous and well-marked retouchings, in all respects resembling the Quaternary types found every day on the surface. On the other side, it presents . . . a bulb of percussion. "

Bourgeois did not specify the exact places from which the above-mentioned specimens were taken—that is, from the exposed sections in the valley, from the valley trench, or from the pit sunk in the top of the plateau. But his reports suggest that implements recovered from all three places were quite similar.

Figure 4.10. Top: A Late Pleistocene flint implement (Laing 1894, p. 366). Bottom: An implement from Early Miocene strata at Thenay, France (Bourgeois 1873, plate 2).

In order to resolve any controversy, the Congress of Prehistoric Anthropology and Archeology nominated a fifteen-member commission to judge the discoveries of Bourgeois. A majority of eight members, including de Quatrefages and Capellini, voted that the flints were of human manufacture (de Mortillet 1883, p. 87). An additional member voted in favor of Bourgeois, but with some reservations. Only five of the fifteen found no trace of human work in the specimens from Thenay. One member expressed no opinion.

De Mortillet stated that if instead of considering just the numbers of votes that were cast, one considered their scientific merit, then the victory of Bourgeois was even greater. De Mortillet pointed out that among those voting in support of Bourgeois were the scientists who had especially devoted themselves to the study of flint tools, while among the dissenters were the scientists who had little or no experience in this area. Indeed, one of them, Dr. Fraas, of Germany, claimed at the Congress that the handaxes of the Quaternary gravels of the Somme region of France, accepted by almost all authorities as genuine human artifacts, were "an invention of French chauvinism" (de Mortillet 1883, p. 88).

Bourgeois gave a choice collection of flint tools from Thenay to the national museum of antiquities at St. Germain and also exhibited his best specimens at the exposition of anthropological science held in 1878. After his death, specimens were given to the museum of the School of Anthropology in Paris.

Many of the flints of Thenay have finely cracked surfaces indicating exposure to fire. Others, much more altered, have surfaces pitted with irregular holes. Was the cracking and pitting caused by weathering? De Mortillet (1883, p. 90) said that cracking resulting from fire and weathering could be very easily distinguished. Significantly, the normally translucent flints had become opaque. Experiments showed that it took a great deal of heat to discolor flints as much as those found at Thenay. The heat of the sun could not have done it. But if fire was the cause, was it fire used by humans or some kind of accidental fire?

In considering the possible causes of accidental fire, de Mortillet suggested that the three most likely possibilities were volcanic action, spontaneous vegetable combustion, or vegetation ignited by lightning. De Mortillet pointed out, however, that there were no volcanoes in the region and no layers of combustible plant material such as peat. Furthermore, the burned flints were found scattered at many locations throughout diverse levels in the same general area. This indicated to de Mortillet that the signs of burning were not the result of fires ignited by lightning. He appears to have reasoned as follows.

The many localized signs of fire at numerous levels indicated continuous intentional use of small fires over a long period of time rather than occasional general conflagrations, such as might have occurred when grass, brush, or forest was ignited by lightning. The evidence strongly suggested that humans had regularly used fire to help fracture the flints.

Bulbs of percussion were rare on the Early Miocene flints of Thenay, but most displayed fine retouching of the edges. De Mortillet (1883, p. 92) stated that even though there were not many bulbs of percussion, retouching alone was a good sign of intentional work. The retouching tended to be concentrated on just one side of an edge, while the other side remained untouched; this is called unifacial flaking. De Mortillet, like modern authorities, believed that in almost all cases unifacial flaking is not the result of chance impacts but of deliberate work. Some researchers have suggested that in special instances unifacial flaking might result from natural forces that press one side of a flint against a hard surface, taking small chips off the edge (Section 3.4.1). De Mortillet (1883, pp. 92-93) admitted that this sometimes occurred; however the resultant chipping was generally very crude and irregular. In his book *Musee Prehistorique*, de Mortillet included reproductions of some Thenay flints that displayed very regular unifacial retouching—flakes removed in the same direction along one side of an edge (Figure 4.11). Some of the critics of Bourgeois commented that among all the Early Miocene flint pieces he collected at Thenay, there were only a very few good specimens, about thirty. But de Mortillet (1883, p. 93) stated: ‘ Even one incontestable specimen would be enough, and they have thirty!’

A modern expert on stone implements, Leland W. Patterson, has stated (1983, p. 303): "Unifacially retouched stone tools are generally an important class of tools on archeological sites, and

comprise a major portion of lithic artifacts of early man sites. This group can include well-known types of stone tools such as graters, perforators, scrapers, notched tools, and some types of knives, choppers, and denticulates. " The Thenay implements conformed to this description.

Figure 4.11. Unifacially retouched implements from the Early Miocene at Thenay, France (G. de Mortillet and A. de Mortillet 1881, plate 1).

Figure 4.12. Left: A flint implement from an Early Miocene formation at Thenay, France (G. de Mortillet and A. de Mortillet 1881, plate 1). Right: An accepted implement from the lower middle part of Bed II, Olduvai Gorge, Africa (M. Leakey 1971, p. 113). The lower edges of both specimens show roughly parallel flake scars, satisfying the requirements of L. Patterson (1983) for recognition as objects of human manufacture.

According to L. Patterson (1983, p. 303): "Completely unifacial tool shapes would be one of the most difficult items for nature to reproduce by random forces. It would be difficult for random forces unidirectionally to fracture flake edges only on one face. It would be even more difficult for fortuitous forces to create the long, uniform, parallel flake scars characteristic of purposefully made unifacial tools. ... It follows, then, that it would be extremely difficult to conceive of nature fortuitously creating an entire group of various well-made unifacial tools, with multiple examples of each tool type, that is the usual demonstration of a kit of man-made stone tools." Patterson (1983, p. 303) added: "Any experienced lithic analyst with a 10-power magnifier can distinguish fortuitously shaped flakes from unifacial tools."

Illustrations of the flints from the Early Miocene of Thenay show the parallel flake scars of approximately the same size that, according to Patterson, are indicative of intentional human work. Figure 4.12 shows a unifacial implement from Thenay along with a similar accepted unifacial implement from Olduvai Gorge.

Through the writings of S. Laing, knowledge of the Thenay tools from the Early Miocene reached the intelligent reading public of the English-speaking countries. Because we desire to make this

work a sourcebook of primary and contemporary secondary reports about anomalous evidence relating to human antiquity, we will include relevant passages from Laing's works.

Laing (1893, p. 113) wrote of the tools found at Thenay: "When these were first produced, the opinion of the best authorities was very equally divided as to their being the work of human hands, but subsequent discoveries have produced specimens as to which it is impossible to entertain any doubt, especially



Figure 4.13. A scraper or borer (top) and a flint knife (bottom) from an Early Miocene formation at Thenay, France, reproduced by S. Laing (1894, pp. 364-365) from a book by A. de Quatrefages.

the flint knife and two small scrapers [the knife and one scraper appear in Figure 4.13] figured by M. Quatrefages at p. 92 of his recent work on *Races humaines*. They present all the characteristic features by which human design is inferred In other cases, viz.: the bulb of percussion and repeated chipping by small blows all in the same direction, round the edge which was intended for use."

Laing (1893, pp. 113 -115) continued his review: 'The human origin of these implements has been greatly confirmed by the discovery that the MIncoplcs ofthe Andaman Islands manufacture whetstones or scrapers almost identical with those of Thenay. and by the same process ofusing fire to splitthe stones into the requisite size and shape. These MIncoplcs are not acquainted with the art of chipping stone into celts or arrowheads, but use fragments of large shells, of whichthey have a great abundance, or of bone or hard wood. and the scrapers are employed In bringing these to a sharper point or finer edge. The main objection, therefore, at first raised to the authenticity of these relics of Miocene man, that they did not afford conclusive proof of design, may be considered as removed, and the objectors have to fall back on the assumption. either that the Implements were fabricated by some exceptionally intelligent *Dryopithecus*. or that the Abbe Bourgeois may have been deceived by workmen, and mistaken in supposing that flints, which really came from overlying Quaternary strata, were found in the Miocene deposit. This hardly seems probable in the case of such an experienced observer. and had it been so, the implements might have been expected to show the usual Quaternary types of celts. knives, and arrow-heads, fashioned by percussion, whereas the specimens found all bear a distinct type, being scrapers and borers of small size, and partly fashioned by fire. ... On the whole. the evidence for these Miocene implements seems to be very conclusive. and the objections to have hardly any other ground than the reluctance to admit the great antiquity of man. " Here we may note that collections of Quaternary implements often include scrapers and borers of the type found at Thenay, in addition to the more sophisticated projectile points and handaxes.

As an example of popular science writing, Laing's work is satisfactory. His mode of expression was reasonable and lucid. He did not oversimplify. The evidence he cited was faithfully reproduced from original scientific reports and was presented in an honest fashion. Especially strong was his report that the Andaman islanders made tools similar to those of Thenay by using fire to flake the stone. Modern authorities regard studies of present-day lithic technologies as useful in recognizing intentional human work on stone materials gathered from ancient sites.

In his book *Human Origins*, Laing (1894, p. 363) again wrote of the flint implements of Thenay: "The general form might be the result of accident, but fractures from frost or collisions simulating chipping could hardly be all in the same direction, and confined to one part of the stone. The inference is strengthened if the specimen shows bulbs of percussion, where the blows had been struck to fashion the implement, and if the microscope discloses parallel striae and other signs of use on the chipped edge, such as would be made by scraping bones or skins, while nothing of the sort is seen on the other natural edges." As we have seen, some of the flint objects from Thenay do have bulbs of percussion and signs of wear confined to working edges, in addition to regular unifacial retouching. Laing also mentioned that the Thenay specimens closely resembled later implements of undoubted human manufacture.

Laing (1894, p. 356) listed the flint implements found in the Early Miocene at Thenay as one of many cases "in which the preponderance of evidence and authority in support of Tertiary man seems so decisive, that nothing but a preconceived bias against the antiquity of the human race can refuse to accept it."

Laing (1894, pp. 363-364) told the history of the finds: "When specimens of the flints from Thenay were first submitted to the Anthropological Congress at Brussels in 1867, their human origin was admitted by MM. Worsae, de Vibraye, de Mortillet, and Schmidt, and rejected by MM. Nilson, Hebert, and others, while M. Quatrefages reserved his opinion, thinking a strong case made out, but not being entirely satisfied. M. Bourgeois himself was partly responsible for these doubts, for, like Boucher de Perthes, he had injured his case by overstating it, and including a number of small flints, which might have been, and probably were, merely natural specimens. But the whole collection having been transferred to the Archeological Museum at St. Germain, its director, M. Mortillet, selected those which appeared most demonstrative of human origin, and placed them in a glass case, side by side with similar types of undoubted Quaternary implements. This removed a great many doubts, and later discoveries of still better specimens of the type of scrapers have, in the words of Quatrefages, 'dispelled his last doubts,' while not a single instance has occurred of any convert in the opposite direction, or of any opponent who has adduced facts contradicting the conclusions of Quatrefages, Mortillet, and Hamy, after an equally careful and minute investigation."

Laing (1894, p. 370) then went on to say: "The scraper of the Esquimaux and the Andaman islanders is but an enlarged and improved edition of the Miocene scraper, and in the latter cases the stones seem to have been split by the same agency, viz. that of fire. The early knowledge of fire is also confirmed by the discovery, reported by M. Bourgeois in the Orleans Sand at Thenay, with bones of mastodon and dinotherium, of a stony fragment mixed with carbon, in a sort of hardened paste, which . . . must be the remnant of a hearth on which there had been a fire."

In any case, the evidence that an intelligent being of the human type produced the flints of Thenay around 20 million years ago in the Early Miocene seems overwhelming. But some authorities believed the being was not of the modern human type, but rather a more primitive ancestor, as required by evolutionary theory. The controversy was vehement. As this question will come up

again and again in our review of evidence for the presence of humans in Tertiary times, we shall now give this matter some detailed consideration.

#### 4.2.2 Evolution and the Nature of Tertiary Man

In his book *Hommes Fossiles et Hommes Sauvages*, A. de Quatrefages (1884, p. 80) noted: 'The problem of Tertiary man is singularly obscured by the fact that solutions are too often dictated by opinions held a priori, deriving from extremely opposing theories.' The opposing theories and opinions were those of the Darwinists and the Biblical creationists. Uncomfortable with the views of both these groups, de Quatrefages (1884, p. 80) went on to say: "The elements of a conviction based on purely scientific and rational grounds are not numerous. It is easy to see that men of equal intelligence and experience can have different opinions or hesitate to give any opinion whatsoever. But Darwinian doctrines and dogmatic religious convictions have obviously influenced scientific discussion on this matter. "

As of the late nineteenth century, the only fossil remains relating to human origins yet discovered were those of the Neanderthals and Cro-Magnon man. As previously mentioned (Section 1.2), scientists favoring evolution thought that the Neanderthals, although somewhat primitive, were too humanlike to qualify as a missing link with the Miocene apes; and Cro-Magnon man, of course, was fully human. But Cro-Magnon man did put the fully human type well back into the Quaternary, contemporary with ice age mammals such as the mammoth and woolly rhinoceros.

This naturally led Darwinists to place the origin of the human species from apelike ancestors much further back in time. De Quatrefages (1884, pp. 80-81) noted: "Haeckel was the first to make a proposal. He put his *Homo alatus* (speechless man) and *Homo pithecanthropus* (ape-man) in the Pliocene, or late Tertiary. Darwin, taking after his German disciple, proposed that the initial transition from ancient apes to the precursors of modern humans, as signified by the loss of the ape's primitive coat of hair, occurred as early as the Eocene. Wallace cautiously suggested the middle Tertiary as the time during which an unspecified variety of ape attained the human form after a prolonged process of morphological evolution. "

At this time, however, the visions of ape-men propounded by Darwin and Haeckel were purely hypothetical. No fossils of creatures truly transitional between the early Tertiary apes and Cro-Magnon man had been found. But what about the stone tools discovered in Miocene formations by Ribeiro in Portugal and by Bourgeois in France?

Anatole Roujou, a French evolutionist, reacted in an interesting fashion to the stone tools found at Thenay. Roujou said: "Being convinced of the transformation of species, I did not have to wait for the discovery of Miocene flints to demonstrate the existence of Tertiary man, because his existence is a necessary consequence of transformation, as currently understood, and an indispensable corollary to the ideas I hold about the morphological affinities of the mammals and their mode of descent" (de Quatrefages 1884, p. 81).

De Quatrefages (1884, p. 81) observed: "Roujou traced back to Tertiary man, whose existence he accepted on purely theoretical grounds, the several distinct present races of humans which, he believed, have existed since the Quaternary. Roujou saw no reason to suppose that humans like those presently existing could not have existed at the time the flint implements of Thenay were being made."

This is quite an interesting admission from an evolutionist. Today, evolutionists put the emergence of anatomically modern humans in the Late Pleistocene. Nevertheless, even from the standpoint of current evolutionary theory, there is, strictly speaking, no reason to rule out in advance the existence of modern human beings, or a closely related species, in the Miocene. After all, advocates of punctuated equilibrium no longer envision an uninterrupted process of gradual change from one species to another. The paleontological evidence, they say, shows that species remain static for long periods of time, millions of years, and that new species appear quite abruptly in the fossil record (Gould and Eldredge

1977). Accepting this point of view, we should not necessarily expect our ancestors to become progressively more primitive and apelike as we trace them back further and further. After all, there are many present-day creatures, turtles and alligators to name a couple, that have not changed substantially for tens of millions of years.

De Mortillet, also a Darwinist, took a somewhat different approach than Roujou. "He tries to accommodate the ideas of Darwin with the paleontological facts," wrote de Quatrefages (1884, p. 81). De Mortillet himself said: "The mammalian fauna has been replaced several times, at least thrice, since the implementbearing deposits at Thenay were laid down. . . . Can human beings, who display one of the most complex levels of biological organization, have escaped from that law of transformation?" (de Quatrefages 1884, p. 81).

But from the standpoint of modern theory, species may change at different rates. Even if it is agreed that some mammalian species have been replaced several times since the Miocene, there is no reason to reject evidence that suggests the human species might not have been replaced. According to current thinking, speciation is a relatively abrupt and unpredictable occurrence rather than the result of an ongoing process of gradual, progressive change.

As can be seen from the different conclusions of Roujou and de Mortillet, evolutionary theory is quite flexible, perhaps too flexible. It seems almost any piece of paleoanthropological evidence can be accommodated within the elastic evolutionary framework.

De Mortillet went on to make the following observation. "If we see in the flint objects found at Thenay signs of intentional work, we can only conclude that it was the work not of anatomically modern human beings but of another human species, probably representative of a genus of human precursors that fills the gap between humans and animals" (de Quatrefages 1884, pp. 81-82).

De Mortillet called this precursor genus *Anthropopithecus*, existing in three species, the oldest, that of Thenay, being the link with the apes. The other two species were the makers of flint tools found by Ribeiro in Portugal (Section 4.1) and by Rames at Aurillac in southern France (Section 4.3.2).

"For de Mortillet," stated de Quatrefages (1884, pp. 82-83), "the existence of the anthropopithecus in Tertiary times is a necessary consequence of Darwinist doctrines. Their successive appearances and disappearances are equally indispensable for maintaining the accord between the progressive development of the human type and that of mammalian fauna. Encountering in the ancient layers of the earth flints bearing signs of intentional work, it was natural for him to interpret them as the first manifestations of primitive industry by a precursor of modern humans." De Mortillet's objections to anatomically modern humans in the Tertiary were, it seems, primarily theoretical. based on his Darwinian preconceptions.

Looking back on this formative era of modern paleoanthropology, one should carefully note the great strength of de Mortillet's faith in the existence of an apelike precursor of modern human beings. Darwinists were awaiting the appearance of the missing link just as expectantly as others awaited the coming of the Messiah. We may well ask: was it perhaps this strong faith and conviction, more than any other factor, that motivated later paleoanthropologists to designate certain apelike fossil creatures as the biological ancestors of the modern human type?

De Quatrefages (1884, p. 83) then continued: "De Mortillet is the first to admit that no one has as yet found the slightest remains of the anthropopithecus: and he combats the theory of Mr. Gaudry, who is disposed to attribute the worked flints of Thenay to the Miocene ape *Dryopithecus fontani*. But it remains for de Mortillet to reveal to us the exact character of that being, which evidently has, except in his own eyes, nothing but a completely theoretical existence. Others, however, are more daring. Haeckel and Darwin, on the basis of diverse considerations, have indicated some characteristics which would, in their opinion, enable us to recognize their apemen. Finally Hovelacque, carrying to extremes the theory of transformationism, has compared point for point the corresponding traits of the highest anthropoid apes with those of the lowest forms of humanity; from this exercise, he has derived an intermediate form and believes he is able to trace a fairly complete portrait of the being that immediately preceded the first human of the modern type."

Such speculative visualization continues even today. Whereas Hovelacque had not a single fossil bone to work with, paleoanthropologists of later years had at least some starting point. But even so, the few fragments of bone they came to possess were, as we shall see in later chapters, quite insufficient to justify the countless elaborate technicolor visions of body types and lifestyles that to this day decorate museum exhibits and the pages of popular science publications. The main point to be gathered, however, is that the existence of apelike precursors of modern humans was, as de Quatrefages so perceptively noted, more a matter of dogmatic assertion than scientific fact. If this is kept in mind, the subsequent developments in paleoanthropology can be seen in a new light. Were the later "discoveries" of fossil apelike human ancestors the product of unbiased

scientific investigation or of a fanciful prophetic quest that ended in true believers seeing in broken iron cups the holy grail?

"The majority of the authors responsible for the evolutionary views I have discussed speak very loudly in the name of free thought," stated de Quatrefages (1884, p. 83). The term "free thought." in this context, refers not to the modern constitutional guarantee of freedom of conscience but to the atheistic and deistic philosophies that arose in Europe during the eighteenth and nineteenth centuries, in opposition to established churches and their doctrines.

After commenting on the views of the Darwinist free thinkers, de Quatrefages (1884, p. 83) observed: "It is very curious to see how other authors arrive at the very similar conclusions starting from a quite different position. namely, the Mosaic tenets shared by the Christian faiths." De Quatrefages then went on to discuss the beliefs of Boucher de Perthes, the discoverer of the flints of Abbeville, who from Christianity derived the idea of pre-Flood humans, very different from present humans. Some Christian thinkers believed that the time before the Flood was of inestimable length and that the earth had once been inhabited by pre-Adamite humans, who were "rough sketches" of the present species. For such thinkers, including Boucher de Perthes, it was these primitive humans who made the crude stone tools of Tertiary times. Boucher de Perthes suggested that the fossil bones of the antediluvian race had already been found but had perhaps been mistaken for those of anthropoid apes. The pre-Adamite race of apelike humans, constitutionally incapable of understanding and worshipping God, was thought to have been destroyed by an inundation (not the Flood of Noah's time). After this catastrophe, and others, came the six days of the new creation during which the modern race of humans, capable of worshipping God, was brought into being, starting with Adam and Eve (de Quatrefages 1903, p. 31; 1884, pp. 84-88). The new human species was completely distinct from the old, with no connection by descent.

"On the other hand, for de Mortillet and Darwin and his disciples," observed de Quatrefages (1884, p. 89). "the successive creations are continuous. The present human being is connected to the ancient arthropitheque by an uninterrupted line of descent. His form has been somewhat modified, the intelligence increased; but we are nothing else than, in the accepted physiological sense of the word, his great grandson. I will not here combat this last opinion. Everyone already knows the negative nature of my views toward the doctrine of transformationism. So likewise with the religious theories just reviewed." The question of Tertiary humans, in de Quatrefages's view (1884, p. 89), had become "as so much else which should have remained exclusively scientific, a theater of conflict between religious dogmatism and free thought." The same is still true today, as demonstrated by the ongoing debates between advocates of Darwinian evolution and Biblical creationism, particularly in the United States.

We share the views of de Quatrefages, in the sense that we are not satisfied with the dogmatic accounts of human origins given by either the Darwinian evolutionists or the Biblical creationists. The available empirical evidence appears to be at variance with both, which suggests that it would be advisable to seriously consider other theoretical systems. In a forthcoming book, we shall

present an alternative account of human origins that agrees with all the facts more completely than the accounts given by either of the traditional opponents in the long-running debate on human origins.

#### 4.2.3 Who Made the Flints of Thenay?

So the question remains: who made the flint implements of Thenay? Even if one assumes the presence of some primitive ape-man, how can one rule out the presence of human beings of the modern type in the same period? If you can bring *Homo habilis* or *Homo erectus* back to the Miocene, why not *Homo sapiens*?

Laing (1894, p. 370) said of the flints of Thenay: "their type continues, with no change except that of slight successive improvements, through the Pliocene, Quaternary, and even down to the present day. The scraper of the Esquimaux and the Andaman islanders is but an enlarged and improved edition of the Miocene scraper." If humans make such scrapers today, it is certainly possible, if not probable, that identical beings made similar scrapers back in the Miocene period. And, as we shall see in coming chapters, scientists did in fact uncover skeletal remains of human beings indistinguishable from *Homo sapiens* in Tertiary strata.

It thus becomes clearer why we no longer hear of the flints of Thenay. At one point in the history of paleoanthropology, several scientists who believed in evolution actually accepted the Thenay Miocene tools, but attributed them to a precursor of the human type. Evolutionary theory convinced them such a precursor existed, but no fossils had been found. When the expected fossils were found in 1891, in Java, they occurred in a formation now regarded as Middle Pleistocene. That certainly placed any supporters of Miocene ape-men in a dilemma. The human precursor, the creature transitional between fossil apes and modern humans, had been found not in the Early Miocene, 20 million years ago by current estimate, but in the Middle Pleistocene, less than 1 million years ago (Nilsson 1983, pp. 329-330). Therefore, the flints of Thenay, and all the other evidences for the existence of Tertiary humans (or toolmaking Tertiary ape-men), were quietly, and apparently quite thoroughly, removed from active consideration and then forgotten.

The alternative to burying the evidence from Thenay and elsewhere was uncomfortable—perhaps anatomically modern humans had coexisted with dryopithecine apes. This would have meant discarding the emerging evolutionary picture of human origins or revising it to such an extent as to make it appear far less credible. What to speak of anatomically modern humans, any kind of toolmaking hominids would have been, after the discovery of Java man, quite out of place in the Early Miocene of France.

Of course, this scenario about the treatment of evidence is somewhat hypothetical, but it would appear that something like this actually did occur within the scientific community, over the course of several decades in the late nineteenth and early twentieth centuries. The extensive evidence for the presence of toolmaking hominids in the Tertiary was in fact buried, and the stability of the entire edifice of modern paleoanthropology depends upon it remaining buried. If even one single piece of evidence for the existence of toolmakers in the Miocene or Early Pliocene were to be

accepted. the whole picture of human evolution, built up so carefully in this century, would begin to disintegrate. Late Pliocene and Early Pleistocene tools found outside Africa also present difficulties. According to currently dominant ideas, *Homo erectus* was the first hominid to leave Africa and did so about one million years ago.

#### 4.3 Implements From the Late Miocene of Aurillac, France

##### 4.3.1 A Find by Tardy

Further discoveries of Tertiary stone tools were made at two principal sites (Puy Courny and Puy de Boudieu) near the town of Aurillac in the department of Cantal in south central France. In 1870, Anatole Roujou reported that Charles Tardy, a geologist well known for his Quaternary research, had removed a flint knife [Figure 4.14] from the exposed surface of a Late Miocene conglomerate at Aurillac. To describe the removal, Roujou (1870) used the word *arrache*, which means the flint had to be extracted with some force. According to Roujou, the stratum was proven to be Late Miocene

in age by a characteristic fauna, including *Dinotherium giganteum* and *Machairodus latidens* ( de Mortillet 1883, p. 97). De Mortillet, who thought the signs of intentional work on the flint were incontestable, declared that the object resembled undoubted Quaternary tools. Yet de Mortillet (1883, p. 97) believed Tardy's flint tool had only recently been cemented onto the surface of the Late Miocene conglomerate and therefore chose to assign it a Quaternary date.

Figure 4.14. The first stone tool found at Aurillac, France (Verworn 1905, p. 9).

##### 4.3.2 Further Discoveries by Rames

The French geologist J. B. Rames was doubtful that the object found by Tardy was actually of human manufacture, but in 1877 Rames made his own discoveries of flint implements in the same region, at Puy Courny. De Mortillet stated that the flints collected by Rames were found in beds of white quartzite sand and whitish clay containing fossils of *Hipparion*, *Mastodon angustidens*, and other species of Late Miocene (Tortonian) age. Instead of being split by the action of fire, like the flints of Thenay, the specimens from Puy Courny were obviously chipped by percussion (de Mortillet 1883, p. 97).

S. Laing (1894, p. 357) provides a good review of the positive case for the implements found by Rames at Puy Courny: "The first question is as to the geological age of the deposits in which these chipped implements have been found. In the case of Puy Courny this is beyond dispute. In the central region of Auvergne there have been two series of volcanic eruptions, the latest towards the close of the Pliocene or commencement of the Quaternary period, and an older one, which from its position and fossils, is clearly of the Upper Miocene. The gravels in which the chipped flints were discovered by M. Rames, a very competent geologist, were interstratified with tuffs and lavas of these older volcanoes, and no doubt as to their geological age was raised by the Congress of French archaeologists to whom they were submitted. The whole question turns



therefore on the sufficiency of the proofs of human origin, as to which the same Congress expressed themselves as fully satisfied."

Modern geologists still refer the fossiliferous sands of Puy Courny to the Miocene (Peterlongo 1972, pp. 134-135). The fauna (*Dinothenium giganteum*, *Mastodon longirostris*, *Rhinoceros schleiermacheri*, *Hipparion gracile*, etc.) is said to be reminiscent of that of Pikermi, Greece, and is judged to be characteristic of the end of the Pontian (Peterlongo 1972, p. 135). In the past, the Pontian was equated with the Early Pliocene, but Nilsson (1983, p. 19) stated that modern radiometric dating methods indicate that "the whole Pontian stage should be assigned to the latest Miocene time." According to French authorities also, the Pontian marks the end of the Miocene, and can be given a quantitative date of about 7-9 million years (Klein 1973, table 6).

Laing (1894, p. 358) then gave a detailed description of the signs of human manufacture that Rames had observed on the flints: "The specimens consist of several well-known palaeolithic types, celts, scrapers, arrow-heads, and flakes, only ruder and smaller than those of later periods. They were found at three different localities in the same stratum of gravel, and comply with all the tests by which the genuineness of Quaternary implements is ascertained, such as bulbs of percussion, conchoidal fractures, and above all, intentional chipping in a determinate direction. It is evident that a series of small parallel chips or trimmings, often confined to one side only of the flint and which have the effect of bringing it into a shape which is known from Quaternary and recent implements to be adapted for human use, imply intelligent design, and could not have been produced by the casual collisions of pebbles rolled down by an impetuous torrent."

According to Laing, de Quatrefages noted fine parallel scratches on the chipped edges of many specimens, indicating usage. These use marks were not present on other unchipped edges. The flint implements of Puy Courny were accepted as genuine at a congress of scientists in Grenoble (Laing 1893, p. 118).

In conclusion, Laing (1894, pp. 358-359) repeated another very important point that was made by de Quatrefages: "The chipped flints from Puy Courny also afford another very conclusive proof of intelligent design. The gravelly deposit in which they are found contains five different varieties of flints, and of these all that look like human implements are confined to one particular variety, which from its nature is peculiarly adapted for human use. As Quatrefages says, no torrents or other natural causes could have exercised such a discrimination, which could only have been made by an intelligent being, selecting the stones best adapted for his tools and weapons."

Leland W. Patterson (1983, pp. 305-306), a modern expert on lithic technology, has written: "The selective occurrence of certain types of raw material can be useful in identifying human activity at a specific location. The lack of a local source for a raw material is an argument in favor of transport by humans to a site. Another consideration is the selective occurrence of only certain types of raw materials for specimens proposed to be man-made. Man would tend to be selective in use of lithic raw materials, while nature would tend to fracture a wide variety of stone types in a random manner."

But Marcellin Boule gave a geological explanation for the fact that the objects thought to be tools were formed from only one of the many kinds of flint present at Puy Courny. As noted by Rames, the various kinds of flint all came from different layers of the underlying Oligocene formation. In 1889, Boule suggested that during the Late Miocene, only the layer containing the particular type of flint in question had been eroded. According to Verworn (1905, p. 10), that meant only this particular type of flint, lying loose on the surface, was available for toolmaking by intelligent beings in the Late Miocene.

But Boule completely rejected the idea that the flint objects of Aurillac were manufactured by humans or human evolutionary ancestors. His analysis of the erosion of flint at Aurillac was intended to demonstrate that in the Late Miocene, only a certain type of flint had been subjected to purely natural forces tending to create toollike forms.

Boule's account of the successive erosion of the various flint-bearing Oligocene layers may not, however, have been correct. Perhaps several layers eroded simultaneously. If so, this would preserve the point Rames made about intelligent selection of one kind of flint from among many for the purpose of toolmaking. But even if we do accept the sequence of geological events outlined by Boule, this still would not allow one to conclude that the chipped flint objects from the Late Miocene of Puy Courny were produced by purely natural forces. It would seem that all the other kinds of flint that later eroded from layers below the one described above should also have been shaped by natural forces into forms resembling tools. Considered in this way, Boule's explanation tends to explicitly confirm human rather than natural action.

Furthermore, Boule's geological explanation, if correct, merely accounts for the selection of a particular kind of flint. It does not explain the special character of the chipping on the flints. As previously mentioned, the chipping on the flints, confined to one side of one edge, with the chips removed consecutively and in parallel, was not of the type one would expect from random natural battering or geological pressures. In fact, the flint objects were, according to many authorities, identical to accepted unifacially flaked flint tools from the Late Pleistocene.

#### 4.3.3 Verworn's Expedition to Aurillac

In the first part of the twentieth century, some professional scientists continued to recognize specimens from the sites near Aurillac as the work of human beings in the Late Miocene. Among them was Max Verworn of the University of Göttingen in Germany.

In his introduction to a lengthy report on the implements of Aurillac (Cantal), published in 1905, Verworn pointed out that the existence of human beings in the Pleistocene period had been established beyond doubt by skeletal remains, stone artifacts, and other objects of human manufacture. Verworn (1905, pp. 3-4) stated: "The fact that the skeletal remains so far discovered in our Pleistocene investigations can be recognized by their morphology as genuinely human should indicate, in the most lucid manner, to every modern researcher who stands upon the ground of the theory of descent, that the beginning of our race and its specific human characteristics must reach far beyond the Pleistocene, and, at very least, deep into the Tertiary.

Yet despite this theoretical advancement in the investigation of natural history, science is very reluctant to enter fully into the question of the existence of Tertiary man, and any discussion of the evidence in this regard has been treated with utmost distrust and skepticism in the scientific community. Of course this is Justifiable, because in all true science every provisional truth must pass the test of the critical fire of doubt before It can be granted full recognition."

In Verworn we have an excellent example of a scientist with Darwinian credentials accepting evidence (In this case, evidence for a human presence in the Miocene) that would completely contradict current Darwinian Ideas about the origin of the human species. The present scientific establishment propagates the belief that only fundamentalist creationists and early scientists opposed to evolution have ever presented evidence contradicting the current evolutionary understanding of human origins. But this is far from the truth. Scientists who believed in evolution have been the main source of the Information compiled in this book.

Scientific discussion of Tertiary humans peaked in the 1880s and decreased markedly In the final years of the nineteenth century. The question was reopened by Rutot's discoveries of flint implements In Belgium, which we shall consider later in this chapter (Section 4.4). Verworn, working In the very early years of the twentieth century, was himself at first quite doubtful about the human manufacture of eoliths, or "dawn stones," as the crudest of the early stone tools had come to be known.

Verworn (1905, pp. 4-5) wrote in his report on Aurillac: "I must confess that less than a year ago I was still skeptical about accepting the implemental nature of eoliths, and expressed my doubts at the meeting of the Gottingen Anthropological Society on July 22, 1904. Of course, I had seen with my own eyes only the finds of Dr. Hahne from the Pleistocene of the Magdeburg region, and I can say that regarding the greater part of Hahne's eoliths, in view of the strong Inorganic Influences upon them and the conditions of their occurrence, I still today maintain my skepticism, though I do recognize some isolated pieces that bear signs of human work. Meanwhile Herr Rutot was, in the course of the past year, kind enough to send to me as a gift a great collection of typical eoliths from the various levels of the Belgian Pleistocene, and after carefully analyzing them I could no longer maintain any doubts about their Implemental nature. I was overcome with strong excitement. With these discoveries the traces of primitive culture extended far beyond all previous boundaries." Verworn, In these passages, is using the term eolith in a very broad sense. But as we shall see, he will later employ distinctions similar to the ones adopted in this book.

Verworn (1905, pp. 5-6) continued: "The question then arose for me, whether such evidence might extend back Into the Tertiary. The evidence supporting this proposal gathered in earlier times, which in some cases had been introduced with great precision, had not been able to win general recognition. For me there was no doubt about the theoretical possibility of man existing in the Tertiary; the real question was whether or not the Tertiary ancestors of humankind had been capable of manufacturing stone tools, which would give evidence of their existence to those of us in a far removed time. I was still skeptical on this point. When Rutot and Klaatsch had become convinced of the existence of Tertiary eoliths and published some illustrations of such, I could not,

from their descriptions and illustrations alone, reach any positive conclusion about their Implemental nature. There is no alternative, for anyone who wants to come to his own decision, to having the objects in his own hands, to being able to turn them around and analyze all their features. Furthermore, It is necessary to understand the objects in terms of their circumstances of discovery by visiting the places from which they came, especially in order to come to firm conclusions about their geological age, which is required. So Just as for years I had conducted my own experimental flint-flaking studies in order to understand flint objects bearing the characteristic signs of human work, I decided to conduct my own onsite excavations, and thus be in a position to be able to reach a definite decision, for or against the Implemental nature of the Tertiary flints in question. I can honestly say that I entered upon my investigation without any preconceived opinions. I would have been Just as happy to answer the question negatively as positively."

Verworn then had to decide where to conduct his search for Implements. He was aware that France had furnished Investigators with many examples of reputed Tertiary flint tools. The site at Thenay was a possibility, but two scientists, L. Capitan and P. Mahoudeau, had recently published an extremely negative report about the flint objects found there, so Verworn decided to look elsewhere.

Aurillac, in Cantal, where several discoveries of Late Miocene implements had been made over the course of many decades, seemed a more profitable place to conduct his study. Verworn also considered the valley of the Tagus at Lisbon, where Ribeiro had uncovered his Miocene specimens, but because no further discoveries had been made in that region, Verworn ruled out going there. At other sites, such as the Kent Plateau in southeast England and St. Prest in France, the geological context was thought to be Pliocene, not as suitable for Verworn's purposes as the older Miocene age of the Implement-bearing formations at Aurillac. So Aurillac it would be (Verworn 1905, pp. 6-7).

On his way to France, Verworn visited Rutot in Brussels and examined specimens of stone implements in the Royal Museum of Natural History, including some from Aurillac. These had been forwarded to Brussels by the French geologists Pierre Marty and Charles Puech.

Verworn (1905, p. 7) noted: "Even these collections had pieces that I could not easily account for as being other than the product of human work, and the same was true of L. Capitan's large collection of flints from the same site that I soon thereafter had the opportunity to see. . . Capitan has like Klaatsch personally conducted excavations at Aurillac, but has not yet published his findings. Despite the fact that my firsthand observation and testing of these discoveries was leading me to belief in a Miocene flint culture in the Auvergne, I must nevertheless state that my scientific skepticism, and my own previous negative convictions in this matter, were strong enough to inspire new doubts that brought my positive decision again into question. I knew that I had to see the things on the spot, that I must personally get to know the circumstances of discovery, that I must with my own hand remove specimens from the ground— otherwise, I would not be certain. So I traveled to Aurillac"

Verworn remained at Aurillac for six days. Pierre Marty, a local geologist who had written a monograph on the Late Miocene fauna of Joursac (in Cantal), explained to him the geology of the region. Marty also showed Verworn a site he had himself discovered at Puy de Boudieu, and Verworn's excavations there yielded him the majority of his specimens. Charles Puech, a geologist and engineer of roads for the department of Cantal, also gave Verworn extensive geological information.

Verworn (1905, p. 8) reported: "It happened that in the course of my very first excavation at Puy de Boudieu I had the luck to come upon a place where I found a great number of flint objects, whose indisputable Implemental nature immediately staggered me. I had not expected this. Only slowly could I accustom myself to the thought that I had in my hand the tools of a human being that had lived in Tertiary times. I raised all the objections of which I could think. I questioned the geological age of the site, I questioned the implemental nature of the specimens, until I reluctantly admitted that all possible objections were not sufficient to explain away the facts. In what follows, I shall attempt to show all this in detail. At the same time, if anyone doubts the facts as presented, then let him, as I did, go and see."

Concerning de Mortillet's proposal that the maker of the Implements of Aurillac was a small apelike human precursor called *anthropopithecus* (later *homosimius*), Verworn (1905, p. 11) said: "It hardly seems necessary to mention that these speculations, insofar as they are based on the flint tools, are completely arbitrary."

Describing his own discoveries at Aurillac, Verworn (1905, p. 16) wrote: "I especially noted at Puy de Boudieu, where I had the good fortune to come upon a very productive site, that the worked stones, sometimes 5, 10, or 15, would be grouped quite close to each other, separated only by a little tuff or clay, while for 50 to 80 centimeters [roughly 2 to 3 feet] around there would be no such nests or only a few single specimens. As far as appearance goes, the unworked stones appeared to be quite rolled. The worked specimens showed little or no evidence of rolling." Verworn (1905, p. 16) added: "at Puy de Boudieu I was almost exclusively excavating specimens with edges as sharp as when they had been made. All the quartz stones found among the flints are rolled until almost round." The presence of sharp-edged flint objects amidst rolled and rounded pebbles of other kinds of rock at Puy de Boudieu signified that the flint objects had not been subjected to much movement since their deposition and that the flaking upon them was therefore of human rather than geological origin. The fact that the sharp-edged implemental flints were found in groups suggested the presence of workshop sites.

Summarizing the geological context of the discoveries, Verworn gave the following account. The basal layers are Oligocene freshwater and brackish sedimentary deposits containing beds of flint. Above these are Miocene layers of fluvial sands, stones, and eroded chalk containing fossils of *Dinotherium giganteum*, *Mastodon longirostris*, *Rhinoceros schleiermacheri*, *Hipparion gracile*, etc., along with flint Implements. Layers of basalt from volcanic eruptions cover these Late Miocene Implement-bearing layers and in some cases go under them. Above the basalt and the Miocene layers, there are some Pliocene layers, with *Elephas meridionalis* and other Pliocene

mammals. Volcanic layers from Pliocene eruptions cover these. There was no further volcanic action, and the cold periods of the Pleistocene followed. Paleolithic and Neolithic Implements of the standard types are found in the upper terraces (Verworn 1905, p. 17). The basic volcanic sequence outlined by Verworn is still accepted today (Autran and Peterlongo 1980, pp. 107-112).

Venvorn pointed out that those who disputed the Miocene age of the Cantal flints had not visited the sites. Verworn (1905, p. 19) stated: "In fact, in connection with the age of the flints there is, among the geologists who have actually visited the sites, not the slightest degree of reservation. They are all in agreement, and outside of Noetling and Keilhack, I am not aware of any other who have expressed doubt."

Keilhack suggested that perhaps the volcanic eruptions, said by Verworn to have ended in the Pliocene, had in fact continued into the Quaternary. If this were true, then perhaps the implements, some of which were found between layers of lava, were more recent than the Pliocene or Miocene. But what about the fact that the implements were found together with Miocene fossils? Keilhack proposed that the action of streams had mixed in bones from older Miocene layers with more recent Quaternary flint implements.

To these objections Verworn replied as follows. First of all, in no case were fossils of mammals that lived only in the Pleistocene found together with flint implements beneath the lava at Aurillac. This indicated that there had been no Quaternary eruptions. Therefore, any flint-bearing beds found under the several layers of lava were definitely Pliocene or older. Furthermore, the layers of basalt and other volcanic rock were separated by freshwater sedimentary beds with sharply characteristic fossil remains. For example, one might find under a particular layer of basalt a sedimentary bed containing Pliocene fossils and under this another layer of basalt. Under this second bed of basalt, one might then find another sedimentary layer, this with fossil remains of Miocene plants and animals along with flint implements. And under a third layer of basalt one might find another Miocene sedimentary layer containing flint implements, this layer lying upon the Oligocene basement formation. From such evidence, Verworn concluded that the flint-bearing sedimentary beds below or directly above the lowest layer of basalt at Aurillac were Miocene rather than Pliocene in age.

Venvorn (1905, p. 20) concluded: "So we find these implement-bearing layers always directly over the Oligocene or directly upon the basalt from the oldest eruptions, which directly cover the Oligocene layers. The fact that over these oldest eruptive masses one finds beds that contain a typical Late Miocene fauna, like that found at Jonisac, with Hipparion, Dinotherium, etc. means that the underlying implement-bearing beds cannot be any more recent than the Late Miocene. Thus the second doubt of Keilhack, namely that the Miocene fauna has been secondarily introduced into the implement-bearing layers from below, is cleared away."

Venvorn (1905, p. 21) then discussed at length various ways to identify human work on a flint object. He divided evidence of such work into two groups: (1) signs of percussion resulting from

the primary blow that detached the flake from a flint core; (2) signs of percussion resulting from secondary edge-chipping on the flake itself.

On a flint flake, the principal signs of percussion from the main blow that detached the flake from a flint core would be a striking platform, bulb of percussion, and *erraillure*. According to de Mortillet, the presence of a striking platform, bulb of percussion, and *erraillure* together on a flake is a very good indicator of intentional work (Verworn 1905. pp. 21-22).

In addition to the three features mentioned above, Verworn (1905. pp. 22-23) described several more signs of percussion that can be observed on flint flakes (Figure 4.15). Concentrated near the point of impact on the top of the flake one can see a small formation of concentric circular cracks. Radiating from the point of impact and extending over the entire surface of the flake there is also visible a series of curved percussion marks, or force ripples. The stronger the blow that separated the flake from the flint core, the stronger the ripples. Raylike cracks, emanating from the point of impact, intersect the curved force ripples. Verworn also pointed out that in a flake made by percussion the plane of fracture is not straight. If one looks at the flake, edge on from the side, one sees that the ventral surface of the flake is convex at the bulb of percussion, near the top of the flake, and concave at the lower portion, giving an S-shaped contour. Sometimes one can also see on the striking platform a crush mark from a previous blow that failed to detach the flake from the flint core. Negative impressions of some of the above-mentioned features are sometimes visible on the core from which the flake was taken.

It would seem that the presence of combinations of these percussion signs would make it easy for one to identify human work on a flint object. But according to Verworn, this is not necessarily so. All the above-mentioned characteristics are symptoms of just one thing—a blow of sufficient force directed at a given point. But if nature could deliver the blow, then the presence of all the symptoms of percussion is not enough to establish human workmanship (Verworn 1905. p. 23).

The question as to whether nature can actually deliver such a blow has been much debated. Verworn (1905. p. 24) wrote: "It is generally recognized that extreme fluctuations of temperature and moisture, and the action of frost, do not result in fracturing that produces the above-mentioned features. It is otherwise with the question whether or not strongly agitated water, as in flooded mountain streams, waterfalls, or ocean shores, can throw stones together in such a way as to bring about the typical characteristics of percussion. I do not rule this out, but I would tend to believe that such things, if they occur, do so only in very isolated instances." In this respect, Verworn is in agreement with modern authorities on lithic technology such as Leland W. Patterson (1983) and George F. Carter (1957, 1979).

Verworn (1905. p. 24), willing to consider all possibilities, further stated: "I could also imagine that falling stones, loosened by erosion, could produce such effects, but again, very rarely. Finally it would appear to me that stones pressed against each other by glacial action could produce the characteristic symptoms. In summary, the possibility that purely inorganic factors could act on flint to produce the above-mentioned signs of percussion is something I do not wish to dispute. Therefore the bulb of percussion, *erraillure*, striking platform, force ripples, etc., are not, contrary

to de Mortillet's view. definite criteria for intentional flaking." Here Venvorn was perhaps showing too much caution. Even in terms of his own analysis of evidence indicating percussion, it is not very likely that nature would, except in extremely rare circumstances, produce such combinations of effects.

Verworn believed that retouched edges on flint flakes were good, but again not absolutely certain, evidence of human manufacture. He recommended very careful study of the features of such retouching, including the depth and size of individual marks, the similarity of their planes of impact, and their arrangement in regular rows along the edges of the presumed flint implement (Verworn 1905, pp. 24-25).

Unidirectional flaking on one side of an edge is generally taken as a very sure sign of human work, but Venvorn (1905, pp. 27-28) stated he could "imagine circumstances in which you might have a sharp piece of flint sticking out from a wall of limestone, and then have pieces of rock falling from above, hitting the edge manytimes, producing unidirectional flaking."

Venvorn recommended that special attention be given to signs of use on the edges of possible flint implements. One would expect that an implement used for scraping wood, bones, or skin, or for digging the earth, would display certain characteristic markings. Venvorn conducted extensive experimental research in this area.

He concluded: "It is characteristic of use-patterns that there are only small marks on the edge. on the average no greater than 1-2 mm [.04-.08 inch]. Even when an edge is used with great pressure on the hardest materials, the use marks are no longer than 5 mm [.2 inch]" (Verworn 1905, pp. 25-26). Use marks should, of course, be confined to the edge employed in scraping and be arrayed in the appropriate direction, in a regular parallel fashion. Less pressure can be applied with a small flint, so use marks should be smaller on small pieces than on big pieces (Verworn 1905, p. 26).

Considering all the various characteristics of percussion and use, Verworn suggested that none of them are in themselves conclusive. Verworn (1905, p. 29) stated: "I propose that in each separate case a critical diagnosis must be made, founded on a deep and thorough analysis of the characteristics of each specimen in connection with the circumstances of its discovery. The diagnosis of each specimen should not be concerned with just one, but with a whole series of symptoms, just as a doctor analyzes internal diseases by a complex of symptoms. . . . What must concern us is therefore not the discovery of a single, all-embracing, universally applicable criterion for recognizing manufacture in stone implements; such a criterion does not exist in reality and every attempt to find one is fruitless. What we must concern ourselves with is the development of a critical diagnostic method, similar to that employed by physicians. The more carefully we develop this diagnostic method through observation and experiment, the more we shall be able to



reduce the number of questionable factors. The critical analysis of a given combination of symptoms is the only thing that will put us in a position to make decisions.”

This is the same methodology suggested by L. W. Patterson (1983). Patterson does, however, give more weight than Venvorn to bulbs of percussion and unidirectional flaking along single edges of flakes, especially when numerous specimens are found at a site. Patterson's studies showed that natural forces almost never produce these effects in significant quantities.

Venvorn (1905, p. 29) then provided an example to illustrate how his method of analysis might be applied: “Suppose I find in an interglacial stone bed a flint object that bears a clear bulb of percussion, but no other symptom of intentional work. In that case, I would be doubtful as to whether or not I had before me an object of human manufacture. But suppose I find there a flint which on one side shows all the typical signs of percussion, and which on the other side shows the negative impressions of two, three, four, or more flakes removed by blows in the same direction. Furthermore, let us suppose one edge of the piece shows numerous, successive parallel small flakes removed, all running in the same direction, and all, without exception, are located on the same side of the edge. Let us suppose that all the other edges are sharp, without a trace of impact or rolling. Then I can say with complete certainty—it is an implement of human manufacture.”

Venvorn, after conducting a number of excavations at sites near Aurillac, analyzed the many flint implements he found, employing the rigorously scientific methodology described above. He then came to the following conclusion: “With my own hands, I have personally extracted from the undisturbed strata at Puy de Boudieu many such unquestionable artifacts. That is unshakable proof for the existence of a flintworking being at the end of the Miocene” (Venvorn 1905, pp. 29-30).

At his main excavation site at Puy de Boudieu, Venvorn (1905, p. 30) found that the implements were sharp, showing no movement since they were deposited. Venvorn (1905, p. 32) stated: “I find that in terms of size, shape, and adaptation to the human hand, these specimens are not different from Paleolithic implements. That, as is evident, rules out de Mortillet's supposition that the small size of the tools meant that the bodily size of the hypothetical *Homosimius* was inferior to that of a human being. The tools do not give grounds for such a conclusion.”

Venvorn discovered in the Miocene formations at Aurillac 199 worked pieces of flint, 98 with bulbs of percussion. In reality, more should have been counted as having bulbs of percussion, for, in many cases, although the part of the flake with the bulb was broken off, the remainder of the flake showed all the usual signs of percussion. Most of the tools were 4-5 centimeters (about 2 inches) in size, although some went up to 10 centimeters (4 inches).

Venvorn (1905, p. 33) wrote: “The typical signs of percussion, such as striking platform, bulb of percussion, *écaillures*, fissures of percussion, and curvature of the plane of fracture, were clearly evident. Only the force ripples on the plane of fracture were not very strongly developed, and the circular percussion marks near the point of impact were not to be seen very clearly, perhaps because of the opaqueness of the material and its strong, dark patination. The backs of the flakes

sometimes bear upon them the cortex, but for the most part they display the scars of earlier flakes that always have been removed in the same direction. Sometimes four or five flake scars run over the back, and often the negative bulbs of percussion from these flakes are still well preserved. Next to them one often sees the strong crush marks of blows delivered in the same direction."

Venvorn (1905, p. 33) performed his own experimental flint flaking and reported: 'With hammer stones, I have struck from the flat pieces of flint from the Miocene beds a number of flint flakes, and these flakes closely resemble the old ones'" Venvorn stated that because of the cortex covering the flint, the blows had to be quite hard, resulting in well-marked bulbs of percussion like those on the Miocene flakes. The cushioning effect of the relatively soft cortex also accounted for the lightness of the rings of percussion on the flakes detached from the flint core.

In addition to flakes, Verworn also found many cores from which flakes had been struck. Venvorn (1905, p. 34) analyzed the situation as follows: "In fact one finds a great number of slabs of flint, on the edges of which one finds characteristic flake scars with negative bulbs of percussion. . . . One might have taken a good slab and removed one or more flakes from the edge. One finds a number of flake scars next to each other on the edge, mostly removed by blows in the same direction, though there are some cases where they have been removed at different angles."

Most of the implements found by Verworn in the Miocene beds of Aurillac were scrapers of various kinds: "Some scrapers show only use marks on the scraping edge, while the other edges on the same piece are quite sharp and unmarked [Figure 4.16J. On other specimens the scraping edge displays a number of chips intentionally removed in the same direction. This chipping displays quite clearly all the usual signs of percussion. Even today the edges of the Impact marks of previous blows on the upper part of some Implements are perfectly sharp [Figure 4.17]. The goal of the work on the edges is clearly and without doubt recognizable as the removal of cortex or the giving of a definite form. On many pieces there are clearly visible handgrip areas, fashioned by the removal of sharp edges and points from places where they would injure or interfere" (Verworn 1905, pp. 37-38).

Figure 4.16. Four views of a flint scraper found in Late Miocene strata at Aurillac, France (Verworn 1905, p. 37). Top left: Ventral surface with large bulb of percussion. Bottom left: Ventral surface tilted to show the lower edge, with numerous small use marks. Top right: Dorsal surface of the scraper, showing removal of five large

parallel flakes. Bottom right: Dorsal surface tilted to show the lower edge, with use marks on the left and remnants of cortex on the right.

Figure 4.17. Left: Ventral surface of Late Miocene flint scraper from Aurillac, France, showing (1) bulb of percussion and (2) striking platform. The cortex of the flint has been removed from the lower edge by percussion, leaving numerous flake scars oriented in approximately the same direction. Right: Dorsal surface, showing five large parallel scars of flakes removed before the scraper itself was struck from the parent flint core. The upper left corner of the implement shows impact damage from one of the previous blows (Verworn 1905, p. 38).

Figure 4.18. Late Miocene flint scraper from Aurillac, France, with large flakes removed in parallel (Verworn 1905, p. 39). This feature reminded Verworn of Late Pleistocene examples.

Figure 4.19. A pointed flint implement from the Late Miocene at Aurillac, France (Verworn 1905, p. 40).

About the object in Figure 4.18, Verworn (1905, p. 39) said: "the flake scars on the scraper blade lie so regularly next to each other in parallel fashion that one is reminded of Paleolithic or even Neolithic examples." In the accepted sequence, Paleolithic and Neolithic tools are assigned to the later Pleistocene.

Vernon also found many pointed scrapers (Figure 4.19): "Among all the flint objects, these show most clearly the intentional fashioning of definite tool shapes, at least in the area of the working edges. In fact, the points are generally made in such a way that one can speak of genuine care and attention in the technique. The edges have been worked by many unidirectional blows in such a way as to make the intention of fashioning a point unequivocal. I characterize as pointed scrapers those tools on which the chips on both sides of the point run in the same direction" (Vernon 1905, p. 40).

Also found at Aurillac were notched scrapers (Figure 4.20), with rounded concave openings on the working edge suitable for scraping cylindrical objects like bones or spear shafts. Vernon (1905, p. 41) observed: "In most cases the notched scrapers are made by chipping out one of the edges in a curved shape by unidirectional blows."

Figure 4.20. Left: Ventral surface of a notched scraper from the Late Miocene of Aurillac, France (Vernon 1905, p. 40). Right: Dorsal surface, showing removal of cortex on the working edge, upon which Vernon observed tiny use marks.

Vernon also uncovered several tools adapted for hammering, hacking, and digging. Describing the one in Figure 4.21, Vernon (1905, p. 41) wrote: "A large pointed tool for chopping or digging. It is formed from a natural slab of flint by the working of a point. One sees on the surfaces of the piece the cortex of the flint and at the top a point made from numerous flakes, mostly removed in the same direction." About another pointed tool, Vernon (1905, p. 41) stated: "This tool has on the side directly below the point a handgrip made by removing the sharp, cutting edges. It might have been a primitive handaxe used for hammering or chopping." Vernon also found tools he thought were adapted for stabbing, boring, and engraving.

Figure 4.21. A Late Miocene flint tool from Aurillac, France. The point is formed by removal of many flakes in the same general direction (Vernon 1905, p. 41).

Venvorn (1905, pp. 44-45) concluded: "At the end of the Miocene there was here a culture, which was, as we can see from its flint tools, not in the very beginning phases but had already proceeded through a long period of development. . . . this Miocene population of Cantal knew how to flake and work flint." The only visible signs of human work upon the Eolithic tools (Chapter 3) were use marks and perhaps slight chipping to improve the working edge. Venvorn saw signs of more extensive intentional work on the tools of Aurillac (Cantal)—removal of cortex (the rough outer surface of the flint) to expose a sharp edge and the subsequent shaping of the edge for a particular purpose. But the modification was confined to the specific edge that was meant for use. Modification did not extend to the shaping of an entire implement, as in the Late Paleolithic and Neolithic. A third sign of intentional work on the tools from Aurillac was the removal of sharp edges to form a comfortable handgrip (Verworn 1905, pp. 44- 47). For these reasons, we have placed the flint implements found by Verworn at Aurillac in the category of crude paleoliths.

Venvorn (1905, p. 50) designated the implements of Aurillac as archaeoliths, placing them between eoliths and paleoliths. Eolithic industries, according to Verworn, are those in which the natural edges of pieces of stone are used as tools without any further modification. Use marks would be the only sign of human action upon them. In Archeolithic industries, the working edges of the tools are modified for specific purposes, and in Paleolithic industries the entire piece of stone is worked with some degree of artistry into a specific tool shape.

Venvorn (1905, p. 50) believed that purely Eolithic cultures—with implements displaying no retouch, just use marks—had not yet been found. As can be seen, Verworn's definition of an eolith is somewhat different than the one we employ, which encompasses slight retouching as well as use marks on naturally produced stone flakes. Our category of crude paleoliths differs from the category of eoliths in that an industry of crude paleoliths would contain at least some tools deliberately struck from cores and subjected to more extensive retouching.

Venvorn felt that geological considerations are primary in determining the age of stone tools, because different levels of culture exist at different times. Even today, he said, there are people who make and use the crudest sort of stone tools (Venvorn 1905, p. 50). Verworn's methodology protects one from automatically assuming that a technologically advanced stone tool found in very old strata must in fact be recent or that a crude tool must necessarily be old.

Venvorn (1905, p. 47) further stated: "Concerning the Miocene culture of Cantal, the facts teach us that we must guard against a mistake, often encountered in the field of prehistoric research when an ancient culture level is discovered. That mistake is forming too low an estimate of the culture in question. The Tertiary age of the culture in this case should in no circumstances force us into underestimating it." We fully agree with Venvorn on this point.

Venvorn (1905, pp. 48-49) went on to say: "Concerning the physiological status of the Miocene inhabitants of Cantal, I would like to make a few observations. I have already indicated that de Mortillet's conclusion from his study of the implements that the manufacturers were of small bodily size is fallacious, because the supposition that the tools are especially small is not supported by observation. I would, on the contrary, with a great deal of certainty say that the size of the

Implements points toward a being with a hand of the same size and shape as our own, and therefore a similar body. The existence of large scrapers and choppers that fill our own hands. and above all the perfect adaptation to the hand found in almost all the tools, seems to verify this conclusion in the highest degree. Tools of the most different sizes. which show with perfect clarity useful edges. use marks, and handgrips. lie for the most part so naturally and comfortably in our hands, with the original sharp points and edges intentionally removed from the places where a hand would grasp, that one would think the tools were made directly for our hands."

Of the manufacturers of the implements found at Aurillac. in Cantal, south central France, Verworn (1905. p. 49) stated: "While it is possible that this Tertiary form might possibly have stood closer to the animal ancestors of modern humans than do modern humans themselves. who can say to us that they were not already of the same basic physical character as modern humans, that the development of specifically human features did not extend back into the Late Miocene? Perhaps the Miocene Inhabitants of Cantal were so highly developed that we could unquestionably give them the title of human being. Such a proposition is neither more nor less likely than de Mortillet's hypothesis of an intermediate form. On the other hand, what would prevent us from seeing in this Tertiary being a line of development parallel to the main line of human descent? All of these are simply possibilities that do not allow for proof or disproof, for the simple reason that we do not have any right to connect a specific culture level with a specific level of physiological development. So long as we have no bodily remains of the Tertiary inhabitants of Cantal. all we say will be speculation without meaning. On the same grounds. all attempts at linkage with *Pithecanthropus* of Trinil (Java man) are worthless. In one case we have cultural remains with no bodily remains. and in the other bodily remains with not a trace of cultural remains. We have simply a comparison of two unknowns. Nothing will come of it. We need patience and more material."

Verworn here makes an important point. From a viewpoint ranging from hundreds of thousands to several million years after the fact. it is very difficult to connect stone implements with particular sets of physiological remains from the same period. if such exist. As we explain in Chapter 6, fossil skeletal remains indistinguishable from those of fully modern humans have been found in Pliocene, Miocene, and even Eocene and earlier geological contexts. When we also consider that humans living today make implements not much different from those taken from Miocene beds in France and elsewhere. then the validity of the standard sequence of human evolution begins to seem tenuous. In fact, the standard sequence only makes sense when a lot of very good evidence is ignored. When all the available evidence. implemental and skeletal, is considered, it is quite difficult to construct any kind of evolutionary sequence. What we are left with is the supposition that there have been various types of human and humanlike beings, living at the same time and manufacturing stone tools of various levels of sophistication, for tens of millions of years into the past.

#### 4.3.4 A Footnote on Aurillac

Shortly after Verworn's excavations at Aurillac (Cantal), the French researcher L. Mayet delivered a report about his own investigations, which led him to the conclusion that the objects found there were products of nature rather than the result of intentional human work. In a footnote to his famous report on the "pseudoeoliths" of Clermont (Section 3.4), Breuil referred to "Mayet's study of Cantal, where in the dislocated strata he found broken blocks of flint resembling eoliths." Breuil (1910, p. 407) stated: "There you also have some broken flints with the pieces still held in place by the sandy matrix." This was obviously to be taken as conclusive and final proof that the stone tools of Puy Courny, like those of Clermont, were produced by geological pressures rather than human action.

But not everyone responded as favorably as Breuil to Mayet's report, originally delivered at a meeting of the French Association for the Advancement of Science, held in Lyon in 1906. Dr. Hermann Klaatsch (1907, p. 765) later wrote: "At a time when the problem of primitive stone artifacts is in a phase permeated with complete lack of clarity, we must happily receive every work that without prejudiced views attempts a factual solution to the eolith puzzle, and we should also give due recognition to the courage of the author who attempts to deal with such troublesome material. In every genuine discussion, opposition is just as welcome as agreement. In this spirit, the authorities who, like myself, are in favor of the human manufacture of the Tertiary flint objects of Cantal, find especially worthy of attention any work that attempts to demonstrate they were formed by purely natural causes." Klaatsch (1907, p. 765) added something Breuil neglected to mention: "It must be noted that L. Mayet in no way shares the radically negative standpoint of Boule, but instead fully recognizes the artifactual nature of the Belgian eoliths [Section 4.4]."

Mayet had twice visited the classic Cantal sites (Puy Boudieu and Puy Courny) and conducted excavations. Klaatsch (1907, p. 765) wrote: "After his introductory lecture, in which Mayet gave assurances that he could supply proofs of the natural process by which the flint objects had been formed, I was extremely disappointed by the way he sought to demonstrate his point. I had hoped that he would clearly inform me about the ways in which natural forces had acted so ingeniously as to transform the site at Puy Boudieu into 'a veritable eolith factory.' That significant shifting and partial resorting of the beds have occurred here is well known to anyone who has conducted excavations. But it remains for L. Mayet to make it plausible that these forces were responsible for the very sophisticated way in which pieces of flint have been broken and worked. Instead he puts off the knowledge-thirsty listener with the suggestion that one cannot precisely describe the action of these natural forces, among which he numbers 'atmospheric agents, variations in temperature, torrential waters, shifting of geological beds, and certainly other factors about which we remain ignorant.' It is as if he were trying to silence an unruly child by intimidating him with a multitude of hints of terrible future events, the consequences of which one could not even imagine." Breuil (1910, p. 407) had tried to do the same thing in his study: "It is clear that the observations made at Belle-Assise do not explain all the natural formations of the Eolithic type; the process that is observed can be juxtaposed with others, such as the action of torrents of water, periods of flooding, the trampling of animals and men, etc."

In discussing Mayet's conclusions about the Puy de Boudieu site, Klaatsch (1907, p. 765) made the following observation: "But about the fact that animal teeth in this frightful topsy-turvy have remained quite whole, as if that were possible, we hear nothing." In other words, if the geological pressures were sufficient to crush blocks of flint, why not the accompanying animal fossils? Klaatsch (1907, pp. 765-766) then stated: "I am therefore not satisfied by Mayet's concluding assertion that 'the action of the intense natural forces that have mixed together the sands and flints at this point are perfectly able to have produced the eoliths, eliminating the necessity of suggesting the intervention of human industry.' People who simply accept these closing words at face value will repeat them as wisdom, and it will afterwards appear that Mayet has proved the natural origin of the Tertiary flint implements. But no, we cannot proceed in this fashion. One should really demand that our adversaries in this debate should fight us on experimental grounds. This reasonable request to solve by experimentation the puzzle of how the flint objects could be produced by the

'intelligent' action of natural forces is not weakened by the fact that Mayet was unsuccessful in producing anything resembling a flint implement by the process of banging blocks of stone together."

Klaatsch (1907, p. 766) then turned his attention to Mayet's statements about the other site at Aurillac, Puy Courny: "Regarding Puy Courny, Mayet cannot call attention to any geological disturbances such as were present at the other site. Instead he seeks, by heaping up questions, to lead one around the complete lack of reasonable arguments and evidence in favor of his point of view. He simply states with utter complacency in his 'conclusions' that the eoliths of Puy Courny 'are in all likelihood the products of the same natural forces.' The fact that countless fossils found in the same beds remain completely unchanged by these forces is here also not mentioned."

Klaatsch (1907, p. 766) then answered one of Mayet's specific objections: 'The great number of specimens at Puy de Boudieu startled him. But in another publication I have pointed out the great masses of artifacts that are to be found at stone workshops in Tasmania. Were such sites to be covered by a stream of lava and then again exposed, this would present much the same sort of scene that confronts one at Puy de Boudieu.' In Africa also, there are sites with thousands of stone tools scattered about. 'On the whole,' stated Klaatsch (1907, p. 766), "I must sadly conclude that the work of Mayet has not brought us one step closer to solving the eolith problem."

#### 4.3.5 A Final Report

As late as 1924, George Grant MacCurdy, director of the American School of Prehistoric Research in Europe, reported in *Natural History* about the flint implements of Puy Courny (Cantal). Finds similar to those of Rames at Puy Courny and Verworn at Puy de Boudieu had been made in England by J. Reid Moir. Some critics argued that natural forces, such as movements of the earth, had fractured flints by pressure, thus creating stone objects resembling tools. But scientists showed that in the particular locations where the flint tools were found, the geological evidence did not suggest the operation of such natural causes.



MacCurdy (1924 b, p. 658) wrote: "Breuil is authority for the statement that conditions favoring the play of natural forces do not exist in certain Pliocene deposits of East Anglia, where J. Reid Moir has found worked flints. . . . Can the same be said of the chipped flints from Upper Miocene deposits near Aurillac (Cantal)? Sollas and Capitan have both recently answered in the affirmative. Capitan finds not only flint chips that suggest utilization but true types of instruments which would be considered as characteristic of certain Palaeolithic horizons. These not only occur but reoccur: punches, bulbed flakes, carefully retouched to form points and scrapers of the Mousterian type, disks with borders retouched in a regular manner, scratchers of various forms, and, finally, picks. He concludes that there is a complete similitude between many of the chipped flints from Cantal and the classic specimens from the best-known Palaeolithic sites. " William Sollas held the Chair of Geology at Oxford, and Louis Capitan, a highly respected French anthropologist, was professor at the College of France.

#### 4.4 Discoveries By A. Rutot In Belgium (Oligocene)

From France, let us now proceed to Belgium, where A. Rutot, conservator of the Royal Museum of Natural History in Brussels, made a series of discoveries that brought the question of anomalous stone tool industries into new prominence during the early twentieth century. Most of the industries identified by Rutot dated to the Early Pleistocene. The oldest of his Pleistocene industries, the Reutelian, was named after the small village of Reutel. east of Ypres. Then came the Mafflian and Mesvinian, named after the villages of Maffle and Mesvin. Last in the series was the more highly developed Strepyan industry, named after the town of Strepy. Rutot regarded the Strepyan as marking the transition to the true

Paleolithic industries of the later Pleistocene (Obennaier 1924, p. 8).

But in 1907, Rutot's ongoing research resulted in much more startling finds, this time in the Oligocene, from 25 to 38 million years ago. Georg Schweinfurth gave an initial report in the *Zeitschrift für Ethnologie*. using the term eolith in its broadest sense to describe the new finds. But on the basis of Rutot's later published descriptions, we have classified the tools as crude paleoliths.

Schweinfurth (1907, pp. 958-959) stated: "The continuing search for eoliths in the high plateau of the Ardennes led to this discovery. . . . As Rutot searched a sand pit near Bonnelles, 8 kilometers [5 miles] south from Lutich, he found an eolith- bearing stone bed under the sands at a depth of 15 meters [49 feet]. The sand is generally regarded as Oligocene, but there were no fossils in it, and therefore the age of the bed is not certain. But in the course of further research Dr. Rutot found in another sand pit a well-developed marine fauna of the Late Oligocene, and at the bottom of this sand there was also a stone bed containing eoliths. Among them were choppers, anvil stones, knives, scrapers, borers, and throwing stones, all displaying clear signs of intentional work that produced forms exquisitely adapted for use by the human hand. Rutot has now brought together a complete series of these artifacts and is preparing for publication a comprehensive report, with illustrations, for the bulletin of the Geological Society of Belgium. On September 30, the fortunate discoverer had the pleasure to show the sites to

34 Belgian geologists and students of prehistory. They all agreed that there could be no doubt about the position of the finds."

Schweinfurth (1907, p. 959) then reproduced this preliminary statement by Rutot about the geology of the Bonnelles region: "On the plateau (between the Maas and Ourthe rivers) the primary stone was covered with flint-bearing chalk, and during the Eocene period the chalk was eroded away, leaving behind heaps of flint that later formed the flint beds. At the beginning of the Late Oligocene a marine intrusion covered the flint beds, depositing 15 meters [49 feet] of fossilbearing sands over them. Finally, during the Middle Pliocene, streams deposited an additional 3 meters [10 feet] of white quartz gravel (a formation now called the Kieseloolithe) along with beds of sand and clay. Then began the excavation of the present valleys." Rutot believed that human beings manufactured the Bonnelles eoliths before the Oligocene marine intrusion, when the land surface was a flint-heaped lowland bordering the sea.

Rutot's complete report on the Bonnelles finds appeared in the bulletin of the Belgian Society for Geology, Paleontology, and Hydrology and provided extensive verification of the preliminary reports cited above. Rutot (1907, p. 479) also supplied information that stone tools like those of Bonnelles had been found in Oligocene contexts at Baraque Michel and the cavern at Bay Bonnet. At Rosart, on the left bank of the Meuse, stone tools had also been found in a Middle Pliocene context, thus making them as old as the eoliths of the Kent Plateau.

In his report on Bonnelles, Rutot (1907, p. 442) stated that the initial discovery of implements had been made by E. de Munck, in a sand pit situated alongside the main roadway from Tilff to Bonnelles, about 500 meters (1640 feet) from a crossroad at the place called "Les Gonhir." In the very bottom of the sand pit, workmen had excavated a hole about half a meter (a foot and a half) deep in order to extract flint to be used as gravel for roadbeds. This enabled de Munck to gather from the matrix of clayey yellow sand many flint flakes showing signs of fine retouching and utilization (Rutot 1907, p. 442). "It was these implements, including a scraper with a clear bulb of percussion and nicely retouched sharp edge, which convinced me that at the place pointed out by de Munck there existed a deposit of Tertiary eoliths that deserved to be explored and studied," said Rutot (1907, pp. 442- 443). A bulb of percussion indicates the scraper was intentionally flaked from a flint core for the purpose of tool manufacture, which, according to our conventions, places such an implement in the category of the crude paleoliths, rather than the eoliths.

Rutot and de Munck worked together at Bonnelles, enlarging and deepening the original excavation. The flint bed was about 1 meter (3 feet) thick and rested on a Devonian sandstone base, surmounted by 15 meters (49 feet) of Oligocene marine sands and clays (Rutot 1907, p. 443). Rutot and de Munck recovered over a hundred specimens, which Rutot (1907, p. 444) said represented "numerous examples of all the various Eolithic types, that is to say percuteurs (choppers), enclumes (anvils), couteaux (cutters), racloirs (side scrapers), grattoirs (end scrapers), and perçoirs (awls)." Rutot (1907, p. 444) stated: "These tools display, in all their detailed features, the same characteristics as other well-known and authenticated Tertiary and Quaternary Eolithic industries." Rutot called the industry the Fagnian, after the name of the region, Hautes-Fagnes.

Another pit 500 meters (1640 feet) to the northwest of the first also yielded tools. Furthermore, this site provided confirmation of the Oligocene dating of the flint bed bearing the tools. Whereas the first site did not furnish any fossils, the layers of sediment above the flint bed at the second site contained many shell imprints. About a dozen species were recognized (Rutot 1907, p. 444). It was obvious that the shells represented a typical Oligocene assemblage. The most common species was *Cytherea beyiichi*. Rutot (1907, p. 447) stated: 'This shell is characteristic of the Late Oligocene of Germany, notably the beds at Sternberg, Bunde, and Kassel. . . . The other recognizable species (*Cytherea incrassata*, *Petunculus obovatus*, *P. philippi*, *Cardium cingulatum*, *Isocardia subtransversa*, *Glycimeris augusta*, etc.) are all found in the Late Oligocene.'

Rutot (1907, p. 448) concluded: 'Therefore, the Eolithic industry found in the flint bed at the base of the Late Oligocene sands is at least Middle Oligocene in age.' The Oligocene ranges from 25 million years ago to 38 million years ago. Rutot's interpretation of the stratigraphy at Boncelles is upheld by other authorities. Maurice Leriche (1922, p. 10) and Charles Pomerol (1982, p. 114) both characterize the sands of Boncelles as Chattian, or Late Oligocene.

"We are thus confronted with a grave problem, or rather a fact the importance of which one cannot escape," wrote Rutot (1907, p. 448). Referring to the controversies regarding the discoveries of some of the tools we discussed earlier, Rutot (1907, p. 448) observed: 'In fact, it is not without a certain repugnance that some have been obliged to accept, in recent times, the idea of the existence of intelligent beings who made and used tools in the Late Miocene. And it is almost with a sense of relief that some have been able to decrease the importance once

accorded to the site at Thenay, reported as Aquitanian [Early Miocene].'

"But now it appears," said Rutot (1907, p. 448), "that the notion of the existence of humanity in the Oligocene, at a time more ancient than that represented by Thenay, has been affirmed with such force and precision that one cannot detect the slightest fault. This is something that offends our old ideas, which have barely become habituated to the simple conception of humans in the Quaternary. But little by little the reality of Pliocene man of the Kent Plateau has been affirmed and accepted, which has in turn permitted the introduction of the idea of humanity in the Late Miocene, contemporary with *Mastodon*, *Hipparion*, and *Dryopithecus*." The Late Miocene discoveries are probably those of Ribeiro in Portugal and of Tardy and others at Aurillac, in France.

'Of course,' added Rutot (1907, p. 448), "passing abruptly from the Late Miocene to the Middle Oligocene may seem somewhat improbable; nevertheless it is proper to submit to the inevitable and accept the facts as they are, seeing that they are not susceptible to any different explanation."

Figure 4.22. Plain chopper (percuteur simple) from below the Late Oligocene sands at Boncelles, Belgium (Rutot 1907, p. 452).

"Moreover," continued Rutot (1907, pp. 448- 449), "hesitation is no longer possible after the discovery of an Industry fashioned by recently living Tasmanians, which has been brought to our attention through the research conducted by Dr. F. Noetling. The bringing to light of this

Industry is, as it were, providential, because it demonstrates quite positively that eoliths are a reality. The discovery shows that scarcely sixty years ago human beings were making and using Implements that are, according to competent and Impartial observers, absolutely of Eolithic type. " Perhaps the Tasmanians would still have been making such implements during Rutot 's time had they not been exterminated by European settlers in the middle of the nineteenth century.

Rutot then described in detail the various types of tools from the Oligocene of Boncelles, beginning with percuteurs (or choppers). "Concerning choppers," said Rutot (1907, pp. 451-452), 'there exist almost always several distinct types, which are: plain choppers, sharpened choppers, pointed choppers, small choppers, and retouchers. Almost all of these are found at Boncelles. The plain chopper [Figure 4.22] is a pebble or block of stone that has been used to strike blows. Such choppers may or may not have retouching to facilitate gripping. These are rare at Boncelles, and the ones collected do not appear to have been used much. One notes on their surfaces relatively faint traces of the special and characteristic marks of percussion. "

Figure 4.23. Sharpened chopper (percuteur tranchant). Rutot (1907. p. 452) noted use marks on the working edge.

The sharpened chopper (Figure 4.23) was the most abundant type. The simple chopper described above could have been used as a hammer stone to strike flakes from blocks of flint, and these flakes could then have been fashioned into sharpened choppers. But at Boncelles, according to Rutot, many natural flakes, were scattered over the land surface, so it was not necessary to produce them artificially. After some retouching to enable them to be comfortably gripped in the hand, they could immediately be put to use. In contrast to the plain chopper, the sharpened chopper is fit for varieties of practical work ( Rutot 1907, pp. 452-453).

"The sharpened choppers collected at Boncelles," wrote Rutot (1907, pp. 452-453), "are as fine and characteristic as possible. Clearly evident is the fact that most of the flaking from usage is angled to the left, as always happens when an Implement is gripped in the tight hand. The opposite occurs when it is employed with the left hand."

Figure 4.24. Small sharpened chopper (tranche/J from below the Late Oligocene sands at Boncelles, Belgium (Rutot 1907, p. 453). The sides show retouching to accommodate gripping by the hand, while the lower edge, said Rutot, shows use marks.

Figure 4.25. Pointed chopper (percuteur pointu) also from Boncelles, Belgium. Rutot (1907, p. 454) said it shows signs of use on both ends.

The tranche (Figure 4.24), according to Rutot, was a smaller version of the sharpened chopper. "The tranche," said Rutot (1907, pp. 453-454), "was certainly used for percussion, and the scratch marks of utilization on the edges are the same as those produced on the large sharpened choppers, though of much smaller size. It appears the tranche rendered service analogous to that of a hatchet. This Instrument is not rare at Boncelles, and we give an Illustration of one. One notes on the vertical edges deliberate retouching, in the form of removal of sharp edges, for easy gripping, and on the lower horizontal edge one notes the irregular marks of utilization."

Rutot (1907, p. 454) noted: "The Oligocene of Boncelles also has pointed choppers [Figure 4.25], that is to say, elongated pieces of flint with one or two of the ends having been used to strike blows. They display on the utilized ends a characteristic star-shaped pattern of flaking, which one can see very well."

The final type of percuteur described by Rutot was the retoucher, which, as the name implies, is a small percussion implement used in the retouching of the edges of stone tools. He illustrated a retoucher (Figure 4.26) with very evident signs of use along the working edge (Rutot 1907, p. 454). Also found at the Boncelles sites were several anvil stones (Figure 4.27) characterized by a large flat surface showing definite signs of percussion (Rutot 1907, pp. 455- 456).

Figure 4.26. An Oligocene retoucher (retouchoij, with percussion marks on working edge (Rutot 1907, p. 454).

Rutot then described some implements he called couteaux, best translated as cutters. "One can see that couteaux are made from relatively long flakes of flint, blunt on one side and sharp on the other. The blunt side

generally retains the flint's cortex. Prolonged usage of the blade turns the rectilinear edge into a sawlike edge, with small irregular teeth. This is caused by chipping of the edge when the blade is pressed against the irregularities of the surface of the object being cut. The cutters were not retouched. They were used for a

long time, until blunted by usage and polishing. It was rare that they were employed until completely unusable. At Boncelles one finds cutters [Figure 4.28] of a very characteristic type." (Rutot 1907, p. 456).

Rutot then described the racloir, or side scraper. The racloir was ordinarily made from an oval flake, produced either naturally or by deliberate flaking, with one of the longitudinal edges blunt and the opposite edge sharp (Figure 4.29). After retouching for a suitable grip, the blunt edge was held in the palm of the hand, and the sharp edge of the implement was moved along the length of the object to be scraped. During this operation, series of small splinters were detached from the cutting edge of the implement, thus dulling it. Rutot (1907, p. 458) stated: 'The characteristic feature of the racloir, used as such, is the presence along the working edge of a series of small chip marks, all arranged in the same direction and located on the same side. When the implement became unusable, it was possible to restore its edge with the retoucher stone, allowing it to be further used."

Figure 4.29. Three views of a side scraper (racloir) found below the Late Oligocene sands at Boncelles, Belgium (Rutot 1907, p. 458).

"The special purpose of the retoucher," said Rutot (1907, p. 458), "was the striking upon a Implement's working edge of a series of small regular blows in the same direction, detaching flakes from 2 millimeters to 5 millimeters [about 0.1 to 0.2 inch] indiameter. The juxtaposition of the flake scars restored the Implement's sharp edge." According to Rutot, this type of retouching is, without a doubt, clearly distinguishable from the retouching performed for accommodation of the hand. Rutot (1907, p. 458) stated: 'Retouching for accommodation of the hand involved hammering and blunting various sharp edges that were either harmful or not usable. But retouching for sharpening was performed to resharpen, by repeated blows in a single direction, an edge dulled by use. One is therefore able to recognize the two types of retouching."

Figure 4.30. This tool was designated by Rutot as a notched side scraper (*racloir 11 encoche*) Scrapers of this type are commonly found in Late Pleistocene assemblages. This tool was recovered from below the Late Oligocene sands at Bonnelles, Belgium (Rutot 1907, p. 458).

Figure 4.31. A double scraper (*racloirdouble*) from below the Late Oligocene sands at Bonnelles, Belgium. Retouching of the two notches in the middle allowed it to be comfortably gripped. Marks of utilization are visible at the top and bottom (Rutot 1907, p. 459).

Rutot (1907, p. 458) pointed out that a good piece of flint can be resharpened several times. But he added "the accumulation of retouching rapidly broadens the original sharp angle of the edge, and when the angle surpasses 45 degrees, the edge offers such resistance that no retouching can be executed, and the implement, now irreparable, is discarded."

Rutot (1907, p. 459) then described another type of *racloir* discovered at the Bonnelles sites: "Frequently the working edge is not straight: It is finished by means of retouching into one or more concave notches, probably for the purpose of scraping long round objects. This is the notched *racloir* [Figure 4.30]. Some are made from natural flakes, others from flakes derived from deliberate percussion."

At Bonnelles, racloirs with two scraping edges, or double racloirs, were also found. About this type of implement Rutot (1907, pp. 459-460) said: "I have provided an illustration of an interesting example [Figure 4.31]. It could be held in the hand, between the thumb and forefinger, at the points nicely indicated by the two lateral notches; the other double racloirs. In the form of pointed flakes with two sharp edges, resemble the true 'Mousterian points.' Mostly they look, as is the case with the one shown in ... [Figure 4.32], as if they were detached by percussion and show a pronounced bulb."

Figure 4.32. This implement was taken from below the Late Oligocene sands at Bonnelles, Belgium (Rutot 1907, p. 460). Rutot said it resembled a Mousterian point from the Late Pleistocene of Europe. The implement's ventral surface (right) shows a bulb of percussion.

Mousterian implements are found in Late Pleistocene contexts of Europe. It is the resemblance of some of the flint implements discovered at the Bonnelles, Belgium, site to Late Pleistocene implements that causes us to classify this industry among the crude paleoliths. Another specimen looking very much like a Mousterian point is shown in Figure 4.33.

Figure 4.33. A racloir from below the Late Oligocene sands at Bonnelles, Belgium. Rutot (1907, p. 460) observed it looked very much like a Mousterian point from the Late Pleistocene of Europe.

Rutot also described a special category of tools, which he called mixed implements (Figure 4.34), because they looked as if they could have been employed in more than one fashion. Rutot (1907, p. 460) stated: 'They tend to have on the sharp edge a point formed by the intersection of two



straight edges, or more frequently, two notches, made by retouching. These implements might be said to resemble awls, but in general the point is too short or rounded. In fact, although the notches are the result of deliberate flaking and retouching, the point seems to be merely the incidental byproduct of the intersection of the two notches."

Rutot (1907, p. 460) went on to say: "This type of implement, of quite singular form, is quite abundant in the old Eolithic period, very rare in the Paleolithic, and again quite abundant in the Neolithic, particularly in the Flenslan assemblages. Good examples also appear among the tools of the modern Tasmanians."

The next type of implement discussed by Rutot was the *graffoir*, another broad category of scraper. According to Rutot (1907, p. 462), the *graffoir* differed from the *racloir* in that "its working edge is employed longitudinally in relation to the direction of the force of application, whereas the *racloir* is held between the thumb and forefinger in such a manner to set the working edge transverse to the direction of the force. When being used, the working edges of the *racloir* and the *grattoir* are thus situated perpendicular to each other." Rutot observed that in order to help the user direct and push the cutting edge of the *graffoir*, these implements in many cases had special notches to accommodate the thumb and forefinger (Figure 4.35b), this in addition to the usual removal of sharp edges to facilitate gripping. At the Boncelles site in Belgium, from strata dated to the Oligocene, there were unearthed a variety of *grattoirs* (Figure 4.35), including the especially large specimen shown in Figure 4.36.

Figure 4.34. This pointed flint implement was discovered in a stratigraphic position below the Late Oligocene sands at Boncelles, Belgium (Rutot 1907, p. 461). The ventral surface (right) of this tool shows a well- developed bulb of percussion with an *erraillure*. According to Rutot, this type of implement is common in Neolithic and modern assemblages.

Figure 4.35. End scrapers (grattoirs) from below the Late Oligocene sands at Boncelles, Belgium: (a) two views of a grattoir, the ventral surface of which (right) shows a bulb of percussion; (b) grattoir with curved indentations for gripping; (c) two views of a double grattoir, with the chipping on each of the two working edges confined to one side of the flake; (d) grattoir with finely retouched working edge (Rutot 1907, pp. 462- 464).

Figure 4.36. Large end scraper (grattoir) from below the Late Oligocene sands at Boncelles, Belgium (Rutot 1907, p. 463).

Rutot (1907, pp. 462, 464) noted: "In the case of grattoirs as well as racloirs, there are some that bear very well marked bulbs of percussion. I do not, however, consider these flakes to have been intentionally made for use as implements. I believe that the flakes with the bulb of percussion were detached involuntarily from the edges of anvils while they were being struck by hammer stones. These detached flakes were usable as tools just as were the sharp natural flakes found nearby. And they were in fact used like them, but they were not deliberately struck for this purpose."

It is difficult, however, to comprehend how Rutot could tell what was going through the minds of his ancient toolmakers as they struck flakes and worked them into implements. Specifically, we wonder how Rutot could say with such certainty that the flakes made into implements were not deliberately struck for that purpose, especially the ones with bulbs of percussion. Here it may be recalled that the bulb of percussion is considered by many authorities, such as Leland W. Patterson (1983), to be a clear sign of intentional controlled flaking.

Rutot was probably attempting to fit the evidence before him within his own framework of evolutionary ideas. He apparently wanted to characterize the makers of the Oligocene industry of Boncelles as more primitive than the makers of industries at Pleistocene sites. But leaving aside Rutot's evolutionary expectations, we can see no reason to rule out the possibility that some of the Boncelles specimens are tools intentionally made from flakes struck for specific purposes.

Rutot then described poinçons, which might be called awls or borers. "These instruments, also called poinçons" he stated, "are characterized by the presence of a sharp point, obtained by intentional modification of a natural flake that already has a somewhat pointed shape. This modified point is situated indifferently in regard to the axis of the instrument, sometimes in a position oblique to the axis" (Rutot 1907, p. 464). An instrument with an oblique point is shown in Figure 4.37, along with two awls with straight points.

Figure 4.37. Three awls (pergoirs) from below the Late Oligocene sands at Boncelles, Belgium (Rutot 1907, p. 465).

According to Rutot (1907, pp. 464 - 465), the Boncelles toolmakers had two ways of modifying a naturally pointed flake to make an awl: "Sometimes the chipping on the two edges making the point was done on just one side of the flake. But sometimes one edge was chipped on the flake's front side, and the other edge was chipped on the flake's back

side. This procedure is convenient because it allows all the blows to be struck in the same position and the same direction. In effect, when the first edge is chipped, one flips the implement and chips in the same place on the other edge to make a point."

Figure 4.38. An awl discovered below the Late Oligocene sands at Boncelles, Belgium (Rutot 1907, p. 465). The chipping on one edge of the point is on the dorsal surface of the implement (left), while the chipping for the other edge of the point is on the ventral surface (right). According to Rutot, this pattern illustrates the use of a specific chipping technique that allowed the maker of the implement to chip one edge of the point, turn the implement over, and chip the other edge of the point from the same position and direction.

Rutot showed a find with this kind of chipping, unlikely to have occurred by random natural battering (Figure 4.38). Rutot remarked that the point obtained by this method of chipping can easily be broken and that, in fact, most of the specimens of awls recovered at Boncelles do have broken points.

Rutot also noted the presence at Boncelles of objects that appeared to be pierres dejef—throwing stones or sling stones. "Throwing stones," observed Rutot (1907, p. 466), "are polyhedral pieces of stone that present an irregular combination of natural and artificial surfaces. They are somewhat rounded in shape and of small volume, appropriate for throwing violently with the hand or with a sling. Such a weapon would strike in such a manner as to produce not only shock from impact but

also cutting from the rotation of the sharp edges of the projectile. The flint industry of Boncelles contains many such polyhedral stones that give every appearance of being throwing stones. ”

Rutot concluded that flint objects with certain characteristics may very well have been used by the ancient inhabitants of Boncelles to make fire. “Not only In Eolithic series, but In Paleolithic and Neolithic assemblages,” stated Rutot (1907, p. 467), “one encounters pieces of flint which along one side bear traces of numerous and repeated violent blows, distributed In groups, each group presenting a series of blows arranged in the same direction. Furthermore, each distinct group has its traces of blows arranged in a direction different from that of the other groups.” These marks could be interpreted as the result of attempts to strike sparks from the pieces of flint. In French, flints used to ignite fire are called *brigueufs*.

According to Rutot, these peculiarly marked stones might superficially resemble other tool types such as anvils, *raclours*, or *grattoirs*. But he pointed out that ‘they are different from these in the violence and the irregularity of the blows inflicted upon them and also by the presence of the flint cortex on the surface marked by the blows, which eliminates any supposition that these are actual cutting Implements” (Rutot 1907, p. 467). The working edges of implements are almost always free of cortex.

Regarding his hypothesis that the pieces of flint in question might have been used for making fire, Rutot (1907, p. 467) mentioned in a footnote: ‘The same idea has been nicely expressed by E. Lartet and Christy in *Reliquiae aquitanae*, pages 85-86 and also pages 138-140. One sees that some Mousterian specimens are represented as briquets for making fire, and the very interesting explanation is given that the fire was obtained not only by friction of flint and pyrite but by flint against flint. A note calls attention to the fact that in England, in Norfolk and Suffolk, up until a century ago, people used the friction of two flints to obtain fire. Dried moss was used as the combustible substance while one rapidly moved two pieces of flint together. ”

All in all, Rutot believed the present-day implements that the objects in question most singularly resembled were briquets, flints used for making fire.

Rutot (1907, pp. 467- 478) wrote: "One could respond that it is a bit rash to think that the primitive humans of Boncelles made fire; nevertheless, I have some reasons to think that they did have knowledge of the usage of fire, but the moment has not come to introduce them. In any case, the humans of Mesvin and Reutel did know how to make fire, and we encounter in the debris of their industries stones that look like briquets. At Boncelles, stones of the exact same type are found, and these also appear to have been used as briquets. We therefore believe It is useful to point out, with some reserve, and by means of comparison, that the stones with special signs of usage and flaking at Boncelles could in fact be either briquets or *pierres a feu* (fire stones).”

Rutot (1907, p. 468) then stated: "So we have now conducted our review of the variegated industry of the intelligent beings of the Oligocene, and we are Justifiably astonished at their expertise, given the vast duration of time that has elapsed since they were present. On the other hand, when

we examine the industry of the recent Tasmanians, which has been brought to light by the research of Dr. Noetling, then we are no less Justifiably astonished to see its extraordinarily primitive and rudimentary character. So the truth, after direct comparison, is that the two industries are exactly the same [Figure 4.39] and that the Tasmanians, now annihilated, but still in existence Just sixty years ago, were at the same level of culture as the very primitive inhabitants of Boncelles and the Hautes Fagnes. " Only the materials from which the Tasmanian tools were made were different— quartzite, diabase, granite, and similar types of rock rather than flint.

Figure 4.39. Implements manufactured by native Tasmanians in recent historical times (Rutot 1907, pp. 470 - 477). Rutot said they resembled almost exactly the tools from the Oligocene period at Boncelles, Belgium. (a) Side scraper (racJoi/), compare Figure 4.33. (b) Pointed implement (pergoii), compare Figure 4.34. (c) Anvil (enc/lime), compare Figure 4.27. (d) Stone knife (couteau), compare Figure 4.28. (e) Double end scraper (grattoir double), compare Figure 4.35c. (f) Awl (pergoiJ), compare Figure 4.37. (g) End scraper (grattoir" compare Figure

4.36.

At some Tasmanian campsites, noted Rutot, Klaatsch found vast numbers of stone implements, attributing this accumulation to the long perLod of habitation. Rutat pointed out that some opponents of anomalously old early stone industries had traditionally used the very large numbers of specimens recovered at various sites as an argument against their being the product of human industry. Rutot (1907, p. 482) believed that Klaatsch's observations proved this objection invalid.

Rutot (1907, pp. 480-481) then clearly framed the essential question posed by his discoveries: "When we take into consideration the analogies, or rather the identities, between the Oligocene eoliths of Boncelles and the modern eoliths of the Tasmanians, we find ourselves confronted with a grave problem—the existence in the Oligocene of beings intelligent enough to manufacture and use definite and variegated types of implements. Who was the intelligent being? Was it merely a precursor of the human kind, or was it already human? This is a grave problem—an idea that

cannot but astonish us and attract the attention and the interest of all those who make the science of humanity the object of their study and

meditation” (Rutot 1907, pp. 480-481).

It might be a shock to many persons with scientific training that a statement like this could have appeared in a scientific Journal in the twentieth century. Today mainstream scientists do not give any consideration at all to the possibility of a human—or even protohuman—presence in the Oligocene. We believe there are two reasons for this—unfamiliarity with evidence such as Rutot's and unquestioning faith in currently held views on human origin and antiquity.

#### 4.5 Discoveries By Freudenberg Near Antwerp ( Early Pliocene to Late Miocene)

In addition to being the site of Rutot's finds in Oligocene strata, Belgium was also the site of another intriguing series of discoveries. In February and March of 1918, Wilhelm Freudenberg, a geologist attached to the German army, was conducting test borings for military purposes in Tertiary formations west of Antwerp. In clay pits at Hoi, near St. Gillis, and at other locations, Freudenberg discovered flint objects he believed to be implements, along with cut bones and shells.

Most of the objects came from sedimentary deposits of the Scaldisian marine stage, which Freudenberg (1919, p. 2) regarded as Middle Pliocene. But according to modern authorities, the Scaldisian spans the Early Pliocene and Late Miocene (Klein 1973, table 6; Savage and Russell 1983, p. 294). The Scaldisian is thus dated at 4-7 million years (Klein 1973, table 6). Freudenberg (1919, p. 9) suggested that the objects he discovered may have dated to the period Just before the Scaldisian marine transgression, which, if true, would give them an age of

7 million years or more.

##### 4.5.1 Flint Implements

Freudenberg believed some of the flint implements he found had been used to open shells. One such implement (Figure 4.40) came from a cavity in the top part of the Scaldisian formation at Koefering, where it was found along with broken shells (Freudenberg 1919, p. 18).

Figure 4.40. This object, characterized by W. Freudenberg (1919, p. 16) as an implement for opening shells, was discovered in a Scaldisian formation (4-7 million years b.p.) at Koefering, near Antwerp, Belgium. The left end of the specimen object appears to be the working edge.

In describing a second shell-opening tool (Figure 4.41), Freudenberg (1919, p. 20) stated: "It comes from the Scaldisian sands of Mosselbank and was found together with many Pliocene molluscs in excavations for fortifications on the outskirts of Antwerp. It is a typical hook-shaped shell opener, found among broken Pliocene shells, especially the broken shells of *Cyprina tumida*. The shell heap appears to have been a Tertiary kitchen midden. The length of the shell opener is 9 centimeters [3.5 inches], when one includes the missing end section." In addition, Freudenberg uncovered some burned flints, which he considered to be evidence that intelligent beings had used fire during the Tertiary in Belgium.

Figure 4.41. A implement for opening shells, from a Scaldisian formation at Mosselbank, near Antwerp, Belgium (Freudenberg 1919, p. 16). Along with the implement, which could be from Early Pliocene to Late Miocene in age, many broken shells were found.

#### 4.5.2 Cut Shells

Of special interest were the numerous shells collected by Freudenberg from the Scaldisian sands at Vracene and Mosselbank. where fortifications were being constructed. About his discoveries, Freudenberg (1919, p. 39) wrote: "The shell heaps of Koefering and Mosselbank near Vracene have yielded countless examples of *Cyprina islandica* and *Cyprina tumida* broken while living and also a shell opener of shiny, patinated flint, like those found at Hoi."

Freudenberg (1919, p. 39) further stated: "The examination of the shell materials from Vracene and Hoi that I undertook in the beginning of 1919 at Gottingen proved the correctness of my initial Judgement that the shell beds were a kitchen midden. In cleaning off the yellow quartz sand and clay, I found many intentional incisions. mostly on the rear part of the shells. quite near the hinge [Figure 4.42]. This was particularly clear on the two *Cyprina* species. On the extinct *Cyprina tumida* specimens. the forward closing muscle was cut through quite regularly by an incision. . . .The incision could only have been made with the help of a sharp flint knife or a shark tooth (we find here teeth of *Oxyrhina hastalis* Ag.). The intentional nature of this action is quite apparent. I have 7 left half-shells of *Cyprina tumida* and 9 right half-shells with the same kind of incision near the depression in the shell that marks the point of attachment of the forward closing muscle."



Describing the incisions themselves. Freudentberg (1919. pp. 39-40) wrote: "The Inner surfaces of the cuts on the shells of *Cyprina tumida* are smooth and bear the same yellow-white weathered surface as the other old surfaces and breaks on any part of the shell. The length of the cut marks is a few millimeters. seldom more than half a centimeter. The Incisions on the shells of *Carina tumida* with well-preserved cut marks are sharply V-shaped, such as could only have been made with a sharp instrument. Other shells that are almost always found broken. as would be expected if they were being used for food. include those of the extinct *Voluta Lamberti* Sow. and *Cardium decorativum*. which along with *Cardium edule* and *C. echinatum*, could have served as edible shellfish. " The sharp cut marks found near the hinges of the shells collected by Freudentberg would appear to be more consistent with human work than the action of shellfish-consuming creatures such as otters.

Figure 4.42. A shell from a Scaldisian formation ( Early Pliocene-Late Miocene) near Antwerp, Belgium, with a cut mark to the right of the hinge (Freudentberg 1919, p. 33),

Freudentberg also found many oysters with broken and cut shells. Of *Ostrea edulis* L var. *unguiata* Nyst. Freudentberg (1919. p. 45) wrote: "I dug up 20 flat right half-shells and about half as many arched left half-shells. Many shells show puncture marks made by sharp. pointed objects, perhaps shark teeth used as tools. From the position of these marks on the edges of the shells. it is obvious they were intended to force them open. The marks sometimes repeat themselves in the same place. giving the impression of premeditated work. The marks are always found on the flat half-shells rather than the curved half-shells. which would be harder to pierce. Splintering is found only on the inner surface of the puncture marks. from which one can conclude that the sharp body that made them entered from outside. All this rules out a posthumous injury. because a dead shellfish opens its shell. and in that case any kind of shell-opening operation would have been pointless." Summarizing his report on shellfish, Freudentberg (1919, p. 50) said: "The number of extinct species is half the total, 27 of 54. Thus the late Tertiary date of the site is not in doubt. The existence of a shellfish-eating population on the

Flemish coast In the late Tertiary is also not in doubt."

#### 4.5.3 Incised Bones

In addition to cut shells, Freudentberg also found cut bones of marine mammals. Among them was part of the upperjaw of a member of the porpoise family, probably related to *Lagenocetus latifrons* Gray. The surface of the jaw is flat and bears upon it a series of incisions. Freudentberg believed the incisions had been purposefully made. In a taphonomic analysis of the jaw, he stated:

“Were these grooves not seen as Intentional work, but rather as the selective corrosion of the bone through chemical or mechanical means (such as the dissolving action of mineral salts or the fiction of sand), then one would expect that the grooves would reach as far down as the nourishment channels (Haversian canals) that run through the bone and there find their end. In reality the grooves cut straight through the nourishment channels; they are also independent of the fine bone structure” (Freudenberg 1919, p. 22). Freudenberg said that this jaw may have been used as some kind of a press.

As discussed in Section 2.11, incisions such as those reported by Freudenberg on the porpoise bone might have been the result of shark bites. In his report, Freudenberg did not mention this possibility, which thus needs further investigation. Still, if the shells from the Scaldisian at Vracene and Mosselbank are taken as bearing intentional cut marks, then this strengthens the possibility that the incisions on the porpoise bones may also have been made by tools.

Marked and polished whale bones were also discovered, along with bones of other marine mammals. Freudenberg (1919, p. 28) wrote: “Artificially broken long bones of walruses and seals are found directly on top of the Septarian clay (Middle Oligocene). These bone fragments were found embedded in clayey greensand, some of which has hardened into limonite on the bones. The bones bear the deep impact marks of blows that could have been made by stone hammers. The depth of the marks varied with the strength of the blows.’

#### 4.5.4 Possible Human Footprints

Further confirmation of a human presence came in the form of partial footprints, apparently made when humanlike feet compressed pieces of clay. From a clay pit at Hol, located just south of the road leading westward from St. Gillis to Meuleken, Freudenberg (1919, p. 3) recovered one impression of the ball of a foot and four impressions of toes (Figure 4.43).

The stone bed in which the footprints were found was judged to be Scaldisian on the basis of the shell fauna. The Scaldisian sediments, as previously mentioned, were deposited in the time period from the Early Pliocene through the Late Miocene. The footprints would thus be at least 4-7 million years old. Freudenberg (1919, p. 9), however, believed they were probably made during the period immediately preceding the Scaldisian marine transgression, and were later incorporated into the Scaldisian formation in which they were found. This would make the footprints somewhat more than 7 million years old. Freudenberg conducted a dermatoglyphic analysis of the prints, as carefully as modern physical anthropologists.

Figure 4.43. Five partial foot impressions from a Scaldisian formation (Early Pliocene to Late Miocene) at Hol, near Antwerp, Belgium (Freudenberg 1919, p. 9).

About the impression of the ball of a tight foot, Freudenberg (1919, p. 11) stated: “There are on the left side signs of displaced grains of sand as well as imprints of the dermal ridges and lines of the skin of the foot, and these show a movement from left to right, or from inside to outside, as would result from the normal movement of the foot in walking.”

Continuing his dermatoglyphic analysis of the print, Freudenberg (1919, p. 11) stated: "One notices that the right, or outer, side of the impression of the ball of the foot is also covered with impressions of fine lines like those of the foot of a humanlike being." According to Freudenberg (1919, p. 11), the pattern of the lines matched that of modern humans and was distinct from that of apes.

Freudenberg (1919, p. 12) added: "The number of lines found in the space of a millimeter is the same on the fossil impression as on the ball of the foot of a modern human adult. The fossil has about 2 lines in 1 mm (10 in 5 mm), in some places fewer. In adult humans, I have measured 4 lines in 2 mm, 5 in 2 mm, and 6 in 2 mm, giving an average of 2-3 lines per mm." Freudenberg (1919, p. 12) then mentioned another significant feature of the impression: "The outlets of the sweat glands are perhaps to be recognized on the fossil impression as tiny bumps, arranged in rows."

Freudenberg, having described the impression of the ball of the foot, then turned to the impressions of toes. Concerning an imprint of the fourth and fifth toes of a left foot, Freudenberg (1919, pp. 13-14) noted: 'The length of an impression of the little toe, measured on the inner side, is, for a 4-year-old child, about 18 mm. The same measurement on the fossil impression is 15 mm. . . . There are also to be observed the impressions of dermal ridges on the imprints of the toes. They are arranged in the same pattern as on the foot of a human child, in that they radiate in all directions from the juncture of the fifth and fourth toes. As in the case of the human child, there are 6-7 dermal ridges per 2 mm at this place. Furthermore, there are properly oriented wrinkles of the skin.'

Freudenberg (1919, p. 14) then stated: "The most important discovery on the fossil toe impression is the shortness of the fifth toe, which is reminiscent of the little toe of the human being. The anthropoid apes, including the gorilla, have long little toes. The foot structure of the genus *Homo* was in the Middle Pliocene already the same as today. The big toe was also short and broad, relative to that of apes, as shown on a somewhat fragmentary impression from Hoi, which appears to be that of a left big toe."

#### 4.5.5 The Identity of Freudenberg's Palaeanthropus

In his conclusion, Freudenberg (1919, p. 52) stated: "It stands without doubt that the sites at Hol and Koefering are part of the Scaldian formation of the Middle Pliocene [Early Pliocene to Late Miocene according to modern authorities]. The geological age of *Palaeanthropus*, the Flemish Tertiary man, dates back this far, if not into older times. This conclusion is especially supported by the fact that bones of Pliocene marine mammals provided Tertiary man with raw materials for his implements and Pliocene shellfish served as his food. Furthermore, the fossil footprints of a humanlike being are found among the Middle Pliocene beach pebbles of Hol."

Freudenberg (1919, pp. 52-53) directed the attention of his readers to supporting evidence from England—the carved shell discovered by Henry Stopes in the Pliocene Red Crag (Section 2.15), as well as the flint implements found in the same formation by J. Reid Moir (Section 3.3) and the cut

bones reported by Fisher (Section 2.16). As we have demonstrated, there is abundant evidence, of all kinds, in favor of a human presence dating to the Pliocene and earlier. In this context, the discoveries of Freudenberg are not at all surprising.

We should point out, however, that Freudenberg (1919, p. 12) was an evolutionist and believed that his Tertiary man must have been a very small hominid, about 1 meter (3.28 feet) tall, displaying, in addition to its humanlike feet, a combination of apelike and human features. Altogether, Freudenberg's description of his Flemish Tertiary man seems reminiscent of Johanson's portrayal of *Australopithecus afarensis* (Lucy). Even if Freudenberg's hypothetical picture of a primitive hominid with humanlike feet is accepted, one would not, according to current paleoanthropological doctrine, expect to find any australopithecines in Belgium during the Late Miocene, at the onset of the Scaldisian, over 7 million years ago. The oldest australopithecines date back only about 4 million years in Africa.

But a late Scaldisian (Early Pliocene) date of 4 million years for a Flemish australopithecine would be within the range of possibility. It should be kept in mind that African mammals such as the hippopotamus ranged as far north as England during the Pliocene and the interglacials of the Pleistocene. Modern paleoanthropologists might, therefore, have good reason to give some serious consideration to Freudenberg's reports, but unfortunately, the knowledge filtration process has, over the course of this century, resulted in the reports disappearing from view.

Thus far we have been going along with Freudenberg's assumption that the humanlike partial footprints from the Scaldisian of Belgium were made by a small primitive hominid. But there is another possibility. There are today, in Africa and the Philippines, pygmy tribes, with adult males standing less than five feet tall and females even shorter. The proposal that a pygmy human being rather than an australopithecine made the footprints found by Freudenberg has the advantage of being consistent with the whole spectrum of evidence—stone tools, incised bones, isolated signs of fire, and artificially opened shells. Australopithecines are not known to have manufactured stone tools or used fire. And, as we shall see in Section 11.10, the toes of australopithecines are noticeably longer than those of modern *Homo sapiens*, while the little toe of the Belgian hominid is similar in length to that of modern humans.

Freudenberg's principal reason for concluding that the being that left the footprints was quite small had to do with certain measurements he made. He ascertained that the radius of curvature of the imprint of the ball of a foot excavated at Hol was similar to that of a human child 4 years of age (Freudenberg 1919, pp. 10 -11). The radius of curvature is the radius of a circle that would fit a section of the curve of the print.

Another feature of the same imprint led Freudenberg to conclude that the creature, despite its short stature, was an adult. In the fossil imprint of the ball of a foot, he found 2 dermal ridges per millimeter. Human adults have 2-3 ridges per millimeter in this part of the foot whereas human children have about 4 ridges per millimeter. Freudenberg therefore believed that the creature must have been an adult, although the radius of curvature of the ball of the foot indicated it was only about 1 meter tall—the height of a human 4-year-old.

But other measurements reported by Freudenberg suggest the adult forms were taller. One of the toe impressions from the Scaldisian was about the same size as that of a human 4-year-old, indicating the creature stood about 1 meter high. On this impression Freudenberg (1919, p. 14) counted 3.0-3.5 ridges per millimeter. The toe impressions of human children have the same number of ridges in that location (Freudenberg 1919, p. 14). This suggests that the creature that made this print was not an adult but a child. Thus when it grew to adult size, it would have been somewhat taller than 1 meter.

To a modern reader, Freudenberg's reports are bound to seem somewhat idiosyncratic. Nevertheless, Freudenberg does provide yet another example of a professional scientist reporting in a scientific Journal finds that today would not be given a moment's serious consideration.

#### 4.6 Central Italy (Late Pliocene)

In 1871, Professor G. Ponzi (1873, p. 53) presented to the meeting in Bologna of the International Congress of Prehistoric Anthropology and Archeology the following report about evidence for Tertiary humans in central Italy: "The very ancient rocks of subappennine Italy that contain human vestiges are breccias which, reposing on the Pliocene yellow sands, can be referred to the end of the Pliocene or beginning of the Quaternary. These vestiges consist of one flint evidently worked into a triangular pointed shape, extracted from the breccia of the 'Acquatraversa sur 1a Vole Cassienne,' by the geologists de Verneuil and Mantovani, and several other flints, of almost the same type, collected by Rossi and Nicolucci in the breccia of Janicule." A breccia is a deposit composed of rock fragments in a fine-grained matrix of hardened sand or clay.

The Acquatraversan erosional phase, during which the breccia was laid down on the yellow sand, can still be regarded as Pliocene. Nilsson (1983, p. 95) stated that the Acquatraversan erosion "is taken to predate a volcanite with a radiometric date of 2.3 million years." This indicates that the stone tools embedded in the Acquatraversan breccias could be at least that old. The yellow sands, upon which the breccias are found, are most likely those of the Astian (Piacenzian) stage of the Late Pliocene (Nilsson 1983, p. 83). This case, although maybe not as strong as others just discussed, is nevertheless worthy of attention and study.

#### 4.7 Stone Tools From Burma (Miocene)

At the end of 1894 and beginning of 1895, scientific Journals announced the discovery of worked flints in Tertiary formations in Burma, then part of the British Indian empire. The implements were reported by Fritz Noetling, a paleontologist and Fellow of the Geological Society who served as director of the Geological Survey of India in the region of Yenangyaung. Noetling (1894, p. 101) stated in the Record of the Geological Survey of India "While engaged in mapping out a part of the Yenangyoung [Yenangyaung] oil-field my attention was particularly directed to the collecting of vertebrate remains, which are rather common in certain strata around Yenangyoung. One of the most conspicuous beds is a ferruginous conglomerate, upwards of ten feet in thickness. This bed may be distinguished a long distance off as a dull-red band, running, in a continuous line, across ravines and hills. Besides numerous other vertebrate remains, such as *Rhinoceros perimense*, etc.,

one of the commonest species is *Hippotherium* [*Hipparion*] *antelopini* Caut. and Falc. of which numerous isolated teeth can be found." Modern authorities still date the Yenangyaung fauna to the Late or Middle Miocene (Savage and Russell 1983, pp. 247, 326).

While picking up a lower molar of *Hipparion antelopinum*, Noetling noticed a rectangular flint object (Figure 4.44). He later described the object: "The two long edges run nearly parallel and are sharp and cutting. This flake affords particular interest in as much as the two faces must have been produced by an action, which is difficult to explain by natural causes" (Noetling 1894, p. 101).

Each face, one concave and the other convex, has two planes meeting in the middle to form an edge, giving the piece four plane surfaces. Noetling wrote: "Let us consider the convex face [on the left in Figure 4.44] first; It will be seen that one side is smooth, apparently produced by the chipping off of a single flake, while the other side shows that at least four smaller flakes have been chipped off at a right angle to the first one." Many authorities see chipping at right angles as a good sign of human work. as natural random battering tends to produce chipping at a variety of angles. In addition, such random battering also removes sharp edges. Noetling (1894, pp. 101-102) further stated: "The concave face which is however much damaged at one side must have been produced by the chipping off of two longitudinal flakes. The shape of this specimen reminds me very much of the chipped flint described in Volume I of the Records. Geological Survey of India, and discovered in the Pleistocene of the Nerbudda river, the artificial origin of which nobody seems to have ever doubted."

Figure 4.44. Two sides of a flint implement from the Miocene Yenangyaung formation in Burma (Noetling 1894, plate 1).

Noetling (1894, p. 101) searched further and found about a dozen more chipped pieces of flint. Some of these he categorized as "irregularly shaped." According to Noetling (1894, p. 101), the edges were "sharp and cutting." The remainder were "triangular flakes," about which he stated: "The lateral edges are straight. sharp, and cutting." Noetling (1894, p. 101) thought one of the triangular flakes shown in his illustrations (Figure 4.45) was "particularly remarkable" because "it shows that the upper face must have been produced by the repeated chipping off of thin flakes."

Figure 4.45. A flint tool from the Miocene Yenangyaung formation of Burma (Noetling 1894, plate 1).

Analyzing the stratigraphic position of his discoveries, Noetling stated that the ferruginous conglomerate containing the chipped flints was surmounted by a 4,620-foot-thick formation (Group C) composed of yellow sandstone alternating with beds of light brown clay. Noetling wrote (1894, p. 102): "A superficial examination of the vertebrate remains shows that the fauna is nearly identical with that of the Siwaliks, or in other words, that Group C ... must be of upper miocene if not pliocene age. We must therefore claim either pliocene or at the latest upper miocene age for the ferruginous conglomerate in which the chipped flints have been found. But whatsoever their particular age be, it is certain that a considerable amount of time must have elapsed since the deposit of a series of strata of more than 4,620 foot thickness, containing numerous genera of animals which are now-a-days either entirely extinct, or at least no longer living in India, which rests upon it [the ferruginous conglomerate]."

W. T. Blanford believed that *Rhinoceros perimensis* and *Hipparion antelopinum*, fossils of which accompanied the flints, characterized in India the Pliocene rather than the Miocene (G. de Mortillet and A. de Mortillet 1900, pp. 90-91). A Pliocene date (2-5 million years) would, however, still be quite anomalous, considering the now-dominant view that toolmaking beings (*Homo erectus*) first migrated from Africa around 1 million years ago. Blanford, however appears to have been wrong. According to modern authorities *Rhinoceros (Aceratherium) penmensis* and *Hipparion antelopinum* occur in Late Miocene assemblages of Asia, including India (Savage and Russell 1983, pp. 283 -284). Furthermore, as previously mentioned, the Yenangyaung fauna in general is today regarded as Miocene (Savage and Russell 1983, pp. 247, 326). This would place toolmaking creatures in Bunna over 5 million years ago.

According to de Mortillet, R. D. Oldham observed flints similar to Noetling's on a plateau rising above the location of Noetling's discovery. Oldham wanted to use this fact to dispute the age of Noetling's flints, but it is not clear why the presence of flints on the plateau should invalidate Noetling's statements about the stratigraphic position of his discoveries.

How certain was the stratigraphic position of Noetling's flints? According to de Mortillet, the strata in which the flints were found appeared to Oldham to be loosely compacted conglomerates, which suggested that the flints might have been introduced in recent times. But Noetling (1894, pp. 102-103) offered this account: "Having now described the geological position of the strata in which the chipped flints were found, there still remains the question to be discussed whether they were really found in situ, or not. To this I can only answer that to the best of my knowledge they were really found in situ. . . The exact spot where the flints were found is marked on my geological map of the Yenangyoung oil-field with No. 49 and is situated on the steep eastern slope of a ravine, high above its bottom, but below the edge in such a position that it is inconceivable how the flints should have been brought there by any foreign agency. There is no room for any dwelling place in this narrow gorge, nor was there ever any; it is further impossible from the way in which the flints were found that they could have been brought to that place by a flood. If I weigh all the evidence,

quite apart from the fact that I actually dug them out of the bed, it is my strong belief that they were in situ when found." It should be remembered that these statements were made by a professional paleontologist who was a member of the Geological Society of London and the Geological Survey of India.

In conclusion, Noetling (1894, p. 103) said: "As to their nature whether artificial or not, I do not want to express an opinion; all I can say is, that if flints of this shape can be produced by natural causes, a good many chipped flints hitherto considered as undoubtedly artificial products are open to grave doubts as to their origin." We agree with this statement. In our review of controversial evidence, we are not so much insisting on the Tertiary date or human manufacture of particular stone objects as insisting on consistent application of standards for evaluating such evidence. We have found that such consistent application is lacking, that prejudice and preconception very often come into play. This raises serious questions about the empiric method as the primary cognitive tool for understanding human origins and antiquity.

#### 4.8 Tools From Black's Fork River, Wyoming (Middle Pleistocene)

In 1932, Edison Lohr and Harold Dunning, two amateur archeologists, found many stone tools on the high terraces of the Black's Fork River in Wyoming, U.S.A. We may recall from our discussion of the eoliths of the Kent Plateau, England, that high river terraces are older than lower terraces. The stone implements found by Lohr and Dunning appeared to be of Middle Pleistocene age, which would be anomalous for North America.

Lohr and Dunning showed the tools they collected to E. B. Renaud, a professor of anthropology at the University of Denver. Renaud, who was also director of the Archaeological Survey of the High Western Plains, then organized an expedition to the region where the tools were found. During the summer of 1933, Renaud's party collected specimens from the ancient river terraces between the towns of Granger and Lyman.

Renaud, who had been trained in Europe under Henri Breuil, characterized the implements as similar to those of the early European Paleolithic (Minshall 1989, p. 86). Among the specimens were crude handaxes and other flaked implements representative of those frequently attributed to *Homo erectus*. In 1933, Renaud said the tools would "suggest a cultural complex in America similar to that of Europe, and also a possible great antiquity for these artifacts" (Minshall 1989, p. 86).

The reaction from anthropologists in America was negative. Renaud wrote in 1938 that his report had been "harshly criticized by one of the irreconcilable opponents of the antiquity of man in America, who had seen neither the sites nor the specimens" (Minshall 1989, p. 87).

In response, Renaud mounted three more expeditions, collecting more tools, which he studied carefully, comparing them with artifacts of similar age from France and England. Although many experts from outside America agreed with him that the tools represented a genuine industry, American scientists have continued their opposition to the present day.



The most common reaction is to explain that the crude Paleolithic specimens are in fact blanks (unworked flakes) dropped fairly recently by Indian toolmakers. Opposing this hypothesis, Herbert L. Minshall (1989, p. 87) stated that the tools "show heavy stream abrasion" even though they are "fixed in desert pavements on ancient flood plain surfaces that could not have had streams for over 150,000 years." In 1938, E. H. Stephens, a geologist at the Colorado School of Mines, visited the sites where the tools had been found. According to Stephens, the high flood plain terraces dated to the Illinoian glacial period (Minshall 1989, p. 88). This would mean they were formed from 125,000 to 190,000 years ago, and perhaps even further back in time (Minshall 1989, p. 88).

If found at a site of similar age in Africa or Europe or China, stone tools like those found by Renaud would not be a source of controversy. But their presence in Wyoming is certainly very much unexpected at 125,000 to 190,000 years ago. The view now dominant is that humans entered North America not earlier than about 30,000 years ago at most. And before that there was no migration of any other hominid.

Renaud's discoveries were therefore either ignored or explained away. Stephens and others suggested that the abrasion on the implements was the result of windblown sand rather than water. In 1957, Marie Wonnington stated: "It is true that many of Renaud's artifacts were found on high terraces and showed definite signs of abrasion. If it could be proven that this was the result of water action it might provide some evidence of age, for a considerable length of time has elapsed since water last reached these terraces. However, if the smoothing was due to wind erosion it provides no evidence of real antiquity" (Minshall 1989, pp. 89-90).

In reply Minshall (1989, p. 90) observed: "The specimens were abraded on all sides, top and bottom, ventral and dorsal surfaces equally. That is extremely unlikely for windblown dust to achieve on heavy stone tools lying in heavy gravel but expectable on objects subjected to surf or heavy stream action. Having examined thousands of stone tools on desert surfaces, I can testify that all-over wind abrasion is rare under any circumstances, is only present on specimens lying in loose sand, and never appears on heavy gravel inclusions. "

Minshall (1989, p. 91) also noted that the tools were covered with a thick mineral coating of "desert varnish." This varnish, which takes a long time to accumulate, was thicker than that on tools found on lower, and hence more recent, terraces in the same region.

The cumulative evidence appears to rule out the suggestion that the implements discovered by Renaud were blanks dropped fairly recently on the high desert floodplain terraces. But Minshall (1989, p. 87) noted: 'The reaction of American scientists to Renaud's interpretation of the Black's Fork collections as evidences of great antiquity was, and has continued to be for over half a century, one of general skepticism and disbelief, even though probably not one in a thousand archaeologists has visited the site nor seen the artifacts. "

According to Minshall, the tools found by Renaud were the work of *Homo erectus*, who may have entered North America during a time of lowered sea levels in the Middle Pleistocene. Minshall

believed this was also true of stone tools found at other locations of similar age, such as Calico, and his own excavation at Buchanan Canyon, both in southern California.

Minshall was, however, skeptical of another Middle Pleistocene site. In January 1990, Minshall told one of us (Thompson) that he was not inclined to accept as genuine the technologically advanced stone tools found at Hueyatenco in Mexico (Section 5.4.4). Hueyatenco was determined to be about 250,000 years old—roughly contemporary with Black's Fork, Calico, and other sites with primitive stone tools that Minshall was prepared to accept. But the advanced stone tools found at Hueyatenco were characteristic of *Homo sapiens sapiens*, and were thus not easy to attribute to *Homo erectus*. Minshall's response to Hueyatenco was to suggest, without supporting evidence, that the stratigraphy had been misinterpreted and that the animal bones used to date the site, as well as the sophisticated stone artifacts, had been "washed onto the site from different sources" (Minshall 1989, p. 93). This shows that researchers who accept some anomalies may rule out others using the double standard method.

### Advanced Paleoliths and Neoliths

Having reviewed the crudest of the anomalously old stone tools (the eoliths) and then the crude paleoliths, we shall now proceed to examine advanced paleoliths and neoliths. Here once more we face difficulties in classification, as many of the discoveries we shall be considering involve implements of various levels of sophistication. The deciding factor for including a group of implements in the category of advanced paleoliths is that a number of specimens represent a clear technical advance over the crude paleoliths discussed in the last chapter. For example, the stone tool industries discovered by Florentino and Carlos Ameghino in Argentina include many implements that might be classed among the eoliths or crude paleoliths; nevertheless, they also include implements of a higher order, such as presumed projectile points and bolas. In this chapter, we shall first discuss the discoveries of Florentino Ameghino, as well as the attacks upon them by A. Hrdlicka and W. H. Holmes. Next we shall consider the finds of Carlos Ameghino, which provide some of the most solid and convincing evidence for a fully human presence in the Pliocene. We shall then proceed to anomalous finds made at sites in North America, including Hueyatenco, Mexico; Sandia Cave, New Mexico; Sheguiandah, Ontario; Lewisville, Texas; and Timlin, New York. We shall conclude with the Neolithic finds from the Tertiary gold-bearing gravels of the California gold rush country.

#### 5.1 Discoveries Of Florentino Ameghino In Argentina

In the late nineteenth and early twentieth centuries, Florentino Ameghino thoroughly investigated and described the stratigraphy and fossil fauna of the coastal provinces of Argentina. He thereby became an internationally known and respected paleontologist. Ameghino's controversial discoveries of stone implements, carved bones, and other signs of a human presence in Argentina during the Pliocene, Miocene, and earlier periods served to increase his worldwide fame.

##### 5.1.1 Monte Hermoso (Middle and Early Pliocene)

Among the most significant examples of human work reported by Florentino Ameghino are those he discovered in 1887 at Monte Hermoso, on the coast of Argentina about 60 kilometers (37 miles) northeast of Bahía Blanca. Here is how F. Ameghino (1908, p. 105) recounted the circumstances of his first discoveries at Monte Hermoso, which were made in a formation he regarded as Miocene; "During an exploratory visit, which lasted from the end of February to the beginning of March of 1887, we had the good fortune to find remains that demonstrated the existence of an intelligent being contemporary with . . . extinct fauna at this site. These vestiges consisted of fragments of *tierra cocida* (burned earth), *fogones* (hearths), *escona* (glassy, melted earth), bone that had been split and burned, and worked stone. These discoveries caused me such surprise and appeared so important that I immediately wrote up my impressions and sent them to the *Journal La Nación*, which published them on March 10, 1887."

In another description of the initial discoveries made at Monte Hermoso, written in 1889, F. Ameghino (1911, p. 74) commented; "I was occupied in extracting part of a skeleton of *Macrauchenia anaique* [a camel-like Pliocene mammal] when I was surprised to see a piece of yellow-red stone among the bones. I picked it up and immediately recognized it as an irregular fragment of quartzite, displaying positive and negative bulbs of percussion, a striking platform, and a flake. These features indicated in an irrefutable manner that I had found a stone object worked by an intelligent being during the Miocene period. I continued my work and soon found similar objects. Doubt was not possible, and on the same day, March 4, 1887, I communicated to *La Nación* the discovery of objects evidently worked by an intelligent being in the Miocene formations of Argentina." F. Ameghino (1911, p. 74) added; "Later, at my instigation, the Museum of La Plata sent to the same place, for the purpose of collecting fossils, the preparator Santiago Pozzi, who found objects similar to mine."

Summarizing the Monte Hermoso evidence, F. Ameghino (1911, pp. 52-53) said: "The presence of man, or rather his precursor, at this ancient site, is demonstrated by the presence of crudely worked flints, like those of the Miocene of Portugal, carved bones, burned bones, and burned earth proceeding from ancient fireplaces, in which earth containing a substantial quantity of sand came in contact with fire so intense that it was partially vitrified."

Regarding the fireplaces, F. Ameghino (1911, p. 52) stated: "In this part of the formation there are no traces of volcanic activity, nor deposits of lignite, nor any vestiges of vegetation that might have sustained accidental fires with the rare property of occurring at intervals consecutive with the successive depositing of the strata at the site. Furthermore, these fireplaces, by the rarest of coincidences, are accompanied by burned bones. The temperature of the fires was so high, that in the pieces of burned earth there have formed spherical cavities, resulting from the expansion of air or the special gases produced by combustion of the substances contained in the earth."

F. Ameghino, who was, like most scientists of his time, committed to the concept of evolution, wrote: "The vestiges belong to such a distant epoch that I do not dare to consider them as proof of the existence of man, but rather as remains of 'a being more or less resembling man, directly

ancestral to man of the modern type'" (1908, p. 105). After two years of research, Ameghino decided the intelligent being that made the artifacts at Monte Hermoso was of a different genus than modern humans and their immediate ancestors. Among the fossils recovered from Monte Hermoso was a hominid atlas (the first bone of the spinal column, at the base of the skull). Ameghino thought it displayed primitive features, but A. Hrdlicka judged it to be fully human (Section 6.2.4). This strongly suggests that beings of modern human type were responsible for the artifacts and signs of fire discovered in the Montehennosan formation.

Although Ameghino thought the Montehermosan formation to be Miocene, modern authorities place it in the Early Pliocene. According to E. Anderson (1984, p. 41) the stratigraphic sequence of the Argentine coastal region can be dated in the following way: the Ensenadan at .4 -1.5 million years, the Uquian at 1.5-2.5 million years, and the Chapadmalalan at 2.5-3.0 million years. The Montehermosan precedes the Chapadmalalan in the general Argentine sequence, and thus it would be over 3 million years old.

Other authorities (Marshall et al. 1982, p. 1352) give a slightly different chronology, placing the Ensenadan formation at .4 -1.0 million years, the Uquian at 1-2 million years, the Chapadmalalan at 2-3 million years, and the Montehermosan at 3-5 million years. A potassium-argon date of 3.59 million years has been obtained for materials from the Montehermosan formation (Savage and Russell 1983, p. 347).

The antiquity of the Montehennosan formation is further supported by the character of its fossil mammalian bones. Paleontologists believe that during the early part of the Tertiary, North America and South America were separated by water and developed distinct mammalian populations. For example, huge ground sloths not found in North America populated South America. When a land bridge eventually formed, North American mammals migrated south, and South American mammals moved north. Modern authorities (Marshall et al. 1982, p. 1351) say that the Panamanian land bridge, which allowed the exchange of mammals between North America and South America, appeared 3 million years ago, just after the period represented by the Montehermosan formation. According to Ameghino (1912, p. 64), the fauna of Monte Hermoso reveals "the complete absence of North American types." Ameghino's discoveries in the Montehermosan formation—including stone tools, modified animal bones, signs of fire, and human skeletal remains—thus suggest a human presence in Argentina more than 3 million years ago.

#### 5.1.2 Hrdlicka Attempts to Discredit Ameghino

Ameghino's discoveries at Monte Hermoso and elsewhere in the Tertiary formations of Argentina attracted the interest of several European scientists, especially those who were attempting to demonstrate the existence of Tertiary humans on the basis of the European discoveries discussed in preceding chapters.

Ales Hrdlicka, an anthropologist at the Smithsonian Institution, also took great, though unsympathetic, interest in Ameghino's discoveries. Hrdlicka found the degree of support they

enjoyed among professional scientists, particularly in Europe, dismaying. In addition to being opposed to the existence of Tertiary humans, Hrdlicka was also extremely hostile to any reports of a human presence in the Americas earlier than a few thousand years before the present. After building an immense reputation by discrediting, with questionable arguments, all such reports from North America, Hrdlicka then turned his attention to the much-discussed South American discoveries of Florentino Ameghino. Hrdlicka was most concerned about the human skeletal remains reported by Ameghino (Sections 6.1.5, 6.2.4), but he also scrutinized Ameghino's discoveries of stone tools and other cultural remains.

In 1910, Hrdlicka visited Argentina, and Florentino Ameghino himself accompanied him to Monte Hermoso. Hrdlicka took an interesting approach to the discoveries that were made at that site. In his book *Early Man in South America* (1912), Hrdlicka barely mentioned the stone implements and other evidence of human occupation previously uncovered by Ameghino in the Montehermosan formation.

"In 1887," wrote Hrdlicka (1912, p. 346), "F. Ameghino announced the discovery, in the barranca of Monte Hermoso, a low cliff facing the sea in the central part of the coast of the Province of Buenos Aires, of vestiges of 'a being, more or less closely related to actual man, who was a direct forerunner of the existing humanity,' These vestiges consisted of fragments of 'tierra cocida, fogones (fire places)—some of the latter vitrified and having the appearance of scoria—split and burnt bones (of animals) and worked stones,"

Hrdlicka said nothing more about these particular discoveries of Ameghino—not even to dispute them. Instead, he devoted dozens of pages to casting doubt on subsequent, and less convincing, discoveries Ameghino made in the Puelchean, a more recent formation overlying the Pliocene Montehermosan at Monte Hermoso,

The Puelchean formation would, according to modern nomenclature, be included within the Uquian. Savage and Russell (1983) stated that the Uquian comprises several formations including the "Pulchense." The Puelchean would thus fall within the Uquian time range, estimated at 1.5-2.5 million years (Anderson 1984) or 1-2 million years (Marshall et al. 1982),

Apparently, Hrdlicka believed his lengthy refutation of the finds from the Puelchean formation was sufficient to discredit the finds in the far older Montehermosan formation at the same site. This tactic is often used to cast doubt on anomalous discoveries—criticize the weakest evidence in detail and ignore the strongest evidence as much as possible. Nevertheless, there is much evidence to suggest that the Puelchean finds, as well as the Montehermosan finds, were genuine.

In and of themselves, the Puelchean discoveries at Monte Hermoso are not of paramount interest to us. If accepted, they merely add to our already abundant stock of evidence for a human presence in the Early Pleistocene. But as a well-documented example of how scientists treat anomalous evidence, the case is significant. We shall therefore take the trouble to examine in detail the shortcomings of Hrdlicka's attempts to discredit the Puelchean implements.

As mentioned above, Hrdlicka and F. Ameghino together visited the Monte Hermoso site. Hrdlicka reproduced an English translation of Ameghino's report of their excursion. Ameghino stated: "On the 11th of June, in the afternoon, we visited Monte Hermoso, where with difficulty we were able to stay a couple of hours. . . . The deposits of sands and sandy ground which rest above the Hermosean and constitute the Puelchean stratum, formerly visible over a small space of only about 40 meters [about 131 feet], now appear exposed along the barranca for several hundred meters and also to a greater extent vertically" (Hrdlicka 1912, p. 105). Ameghino collected a number of implements from "the superior part of this formation" (Hrdlicka 1912, p. 105).

The stone implements recovered by Ameghino from the upper section of the Puelchean formation at Monte Hermoso were very crude. Judging from his descriptions, they appear to resemble the pebble tools of the Oldowan industry of East Africa. Ameghino characterized the Puelchean specimens as fragments of 'water-worn pebbles of quartzite" (Hrdlicka 1912, p. 105). To Ameghino, it was clear that the implements had been deliberately struck from quartzite pebbles: "The larger number of these fragments preserve still on one or two of their faces the natural surface of the rolled pebble, and on this surface are always observed scratches, bruises, abrasions, dints, etc., produced by strong and repeated blows given with other stones" (Hrdlicka 1912, p. 105). Furthermore, the sharp cutting edges of the implements, according to Ameghino, showed 'irregularities, denticulation, and other effects produced by use" (Hrdlicka 1912, p. 105). Ameghino noted: "these broken quartzites, however rustic they may appear, are surely the work of man or his precursor, for there can not be opposed to them the objections which are being made to the eoliths. In this case there can be no question of pressure by the rocks, of shocks produced by stones driven by water or due to falling stones, because, I repeat, they are loose in the sand, and are all separated from one another" (Hrdlicka 1912, p. 106).

Hrdlicka interpreted the finds in another way. Significantly, he did not dispute the human manufacture of even the crudest specimens. Instead, Hrdlicka, an anthropologist with little experience of South American paleontology, offered a different analysis of the stratigraphy than did Ameghino, a professional paleontologist who had devoted decades to the study of the formations in question.

Hrdlicka (1912, p. 118) said in his book: "The writer found that the Monte Hermoso formation exposed in the now famous barranca was covered by more recent material. On the old formation rests a layer of volcanic ash, then some stratified sand, while the highest part is formed of a stratum of gravelly sand continuous with the base of the sand dune situated above and a little farther inland from the edge of the barranca." Ameghino had said the volcanic ash, stratified sand, and gravelly sand comprised the Puelchean formation, overlying the Pliocene Montehermosan. Hrdlicka disputed the inclusion of the uppermost layer of gravelly sand within the superior part of the Puelchean formation. He observed: 'The last-named surface material [the gravelly sand] is unstratified and somewhat packed, but in no way consolidated, and bears every evidence of being very recent. It crumbles over the clearly marked, ancient Monte Hermosean deposit, and in falling down becomes here and there lodged on the shelves or in the depressions of the old formation" (Hrdlicka 1912, pp. 118 -119).

Hrdlicka (1912, p. 119) then recalled: 'In common with Professor Ameghino the writer found in such crumbled down material some large irregular and entirely fresh-looking fragments or chips of quartzite which indicate plainly the work of man. One of the heavier fragments had been employed as a hammer, portions of the periphery being distinctly worn by use. In addition, he found on one of the upper ledges a well-finished scraper of Jasper [a variety of quartz]. Subsequently he extracted a number of quartzite chips or fragments from the more gravelly part of the uppermost deposit itself, within 18 inches of the surface."

It is significant that Hrdlicka reported he extracted stone artifacts not just from crumbled material on ledges, but from within the upper deposit of gravelly sand itself. The fact that they were recovered a full 18 inches from the surface, upon which the recent sand dunes rested, shows they were an integral part of a distinct stratum. Ameghino said the stratum from which he (and Hrdlicka, it seems) took implements was part of the Puelchean formation, which according to modern opinion could be from 1.0 to 2.5 million years old.

#### 5.1.3 Willis Stacks the Geological Deck

Hrdlicka, perhaps worried that his visit to Monte Henneso had led him into a deadly ambush, wanted to suggest that the implements found there, by himself as well as Ameghino, were recent. As we have seen, he attempted to do this by casting doubt on the age of the stratum from which the tools had been taken. Hrdlicka received support in this regard from his companion, the geologist Bailey Willis.

Willis wrote: "Monte Hermoso is a dune on the southern coast of Buenos Aires. It surmounts a short section of the Pampean terrane, which is exposed by wave erosion in a low bluff along the shore. First described by Darwin. It has since been visited by many geologists who have studied the Pampean. . . . The Pampean terrane, which forms the base of the section, contains a notable fauna and the geologic age of the formation has been much discussed. General opinion places it among the lowest or as the lowest of the divisions of the Pampean and Ameghino regards it as Miocene" (Hrdlicka 1912, pp. 361-362). This lower Pampean formation is the Montehermosan, now regarded as Middle to Early Pliocene, and above it lies the Puelchean, which could be Early Pleistocene or Late Pliocene. According to Florentino Ameghino, the layer from which both he and Hrdlicka extracted stone implements during their excursion to Monte Hermoso represented the "superior part" of the Puelchean formation (Hrdlicka 1912, p. 105).

Describing the Puelchean, Willis stated: "The Puelchean consists of the stratified, slightly indurated, gray sands or sandstone, both above and below the volcanic ash, marked by very striking cross stratification and uniformity of gray color and grain. The writer regards it as an eolian formation. Later in the season, when studying the section exposed along the Rio Colorado from the delta to Pichi-Mahuida, he observed a very similar sandstone, which might be correlated with the Puelchean on grounds of lithologic identity. It is a thick widespread formation which is regarded as a Tertiary sandstone. The Puelchean, if the same, represents only a thin edge of it" (Hrdlicka 1912, p. 363).

We may note that the Puelchean sands, which Willis, with some hesitation, accepted as Tertiary, are characterized by their gray color, quite different from the underlying Montehermosan loess, which is yellow-brown. Willis then described the so-called recent topmost layer, apparently included by Ameghino in the Puelchean formation, as "a layer of 15 to 40 cm. [6 to 16 inches] thick composed of gray sand, angular pieces of gray sandstone and pebbles, some fractured by man" (Hrdlicka 1912, p. 362).

Willis elsewhere remarked that the top layer of gray implement-bearing sand is separated from the lower layers of the Puelchean by an "unconformity by erosion" (Hrdlicka 1912, p. 363). An unconformity is a lack of continuity in deposition between strata in contact with each other, corresponding to a period of nondeposition, weathering, or, as in this case, erosion. The unconformity may represent a long break in time, or a very short one. So by itself, the presence of an unconformity by erosion should not allow Willis to so greatly separate the top layer of gray gravelly sand from the underlying formation. He appears to have used the mere presence of stone tools to perform the desired operation, which was necessary to save Hrdlicka the deadly embarrassment of having discovered stone implements in an unexpectedly old formation in South America. For judging how much time might be represented between the formations lying above and below the line of unconformity, the surest indicator is faunal remains. Willis, however, did not mention them. It is thus unclear how much time might be represented by the unconformity.

Willis then stated about the upper layer of gray gravelly sand and the underlying Puelchean: "The two are identical in constitution; they are both eolian and may exhibit similar structures: the Recent formation may be consolidated almost or quite to the firmness of the older one. The unconformity between them suffices to establish the difference in age and is unmistakable when clearly shown in section" (Hrdlicka 1912, p. 363).

Let us carefully consider just what Willis is asking us to accept. First of all he admitted that the two strata are identical in composition, which would seem to be very much in favor of Ameghino, who considered the top layer to be part of the Puelchean. And, given the evidence, why not?

But for argument's sake, let us accept Willis's version. The unconformity by erosion that he proposed would mark a gap of 1-2 million years, as the upper gray layer is supposedly recent, while the gray sand layer below it, identical in composition, is referred to the Early Pleistocene-Late Pliocene Uquian formation.

Another, and perhaps more likely scenario, would be that the two gray sand layers, identical in composition, are separated by an unconformity representing a relatively short episode of erosion in the Early Pleistocene or Late Pliocene.

As mentioned previously, Willis could have accurately determined how long a period was represented by the unconformity only by examining animal fossils above and below the line of unconformity. If the fossils in the layer above the unconformity were all recent, only then would he have been justified in concluding that this layer was recent. But Willis did not make the slightest attempt to establish this. In the absence of such an age determination (which today



might be made by radiometric methods), the implement-bearing layer could very well be about the same age as the Puelchean formation below the unconformity, which it greatly resembles in content and texture.

Here is how Willis attempted to eliminate this alternative: "hand-chipped stones associated with the sands would mark them as recent, such objects being common in the belt of sand dunes which the Indians were in the habit of using as a line of march and cover in attacking Argentine settlements" (Hrdlicka 1912, p. 363). Willis simply assumed that the stone tools were recent and that the layer in which they were found also had to be recent. It would appear, however, that the implement-bearing gray gravelly sand may actually belong to the Puelchean formation, as Ameghino believed, and that the stone implements found there could be as much as 2.0 or even 2.5 million years old.

In short, the question of the age of the implement-bearing stratum below the dune sand at Monte Hermoso remains open. Ameghino's assertion that it belonged to the Puelchean was not conclusive, but neither was the attempt by Willis and Hrdlicka to assign it to the most recent historical times. Since the stratigraphic units in question contain layers of volcanic ash, their ages could be investigated by applying the potassium-argon test, which is specifically used for dating volcanic material. It may also be possible to make a determination by conducting a more thorough search for faunal evidence. In short, the question is still open and should still be a matter of active research. But the report by Willis and Hrdlicka succeeded in closing the books on this intriguing case.

#### 5.1.4 A Demolition Job by W. H. Holmes

Samples of stone tools from Monte Hennoso and other sites on the Argentine coast were sent by Hrdlicka to Washington, where W. H. Holmes of the Smithsonian Institution examined them. Concerning the attribution of any great antiquity to the implements, Holmes was as hostile as Hrdlicka or Willis. In opening his report, included by Hrdlicka (1912, p. 125) in *Early Man in South America*, Holmes stated: "No attempt is made in these notes to consider or weigh the published data relating to the stone implements of Argentina. The collections at hand are classified and briefly described, and such conclusions are drawn as seem warranted by their character and manner of occurrence." In other words, Holmes plainly intended to completely ignore the reports of Ameghino and other professional scientists, who had given detailed evidence for the Early Pleistocene or Pliocene age of the stone artifacts.

We may recall that Hrdlicka, in the company of Ameghino, personally extracted stone tools at a depth of 1.5 feet in the upper layer of the Early Pleistocene-Late Pliocene Puelchean formation at Monte Hermoso (Hrdlicka 1912, p. 104). This fact was subsequently reported by Ameghino in a scientific publication. Hrdlicka and his associates were anxious to discredit this report. If accepted, Ameghino's report on the discoveries he and Hrdlicka made together at Monte Hermosa would have contradicted the entire substance of the book Hrdlicka was then writing. Hrdlicka's book was specifically designed to prove that the only early inhabitants of South America had been the Indians, who had arrived within the past few thousand years.

We detect a slight sense of panic in the following passages, hastily added by Holmes to the end of his report on the stone tools from Argentina. Holmes wrote: "Subsequent to the completion of the foregoing pages Doctor Hrdlicka drew attention to certain specimens collected by him along the barranca at Monte Hermoso, which had escaped particular notice on the writer's part. Attention was directed also to a brief pamphlet just received from Dr. Ameghino, describing a series of similar specimens collected by him while examining this same barranca in company with Doctor Hrdlicka. Considering the nature of the specimens and the manner of their occurrence, the observations and interpretations of Doctor Ameghino are so remarkable that the writer is constrained to refer to them in some detail" (Hrdlicka 1912, p. 149). Otherwise, Hrdlicka's whole project would be shot to pieces. A report showing that Hrdlicka had, in the company of Ameghino, himself extracted undisputed stone tools from an Early Pleistocene or Late Pliocene formation would in itself have destroyed everything Hrdlicka had tried to accomplish by publishing *Early Man in South America*, which was nothing less than a polite but thorough demolition of Ameghino's work.

Holmes wrote: "the objects in question are about 20 freshly-fractured chips and fragments of coarse, partially fire-reddened quartzite, a larger fragment of the same material used as a hammer, and a knife or scraper of jasper. All were found in a surface layer of gravelly sand capping the Monte Hennoso barranca, or on the broken face of the barranca itself. The latter were picked up on the ledges of the bluff face, where they had cascaded from above. The jasper knife or scraper is of a type familiar in the coast region as well as in Patagonia" (Hrdlicka 1912, pp. 149-150). It should, however, be kept in mind that it is principally the objects found in situ that concern us. The implements found lying on ledges might very well have been recent.

Holmes suggested in every possible way that all of the objects, even those found in situ, were of recent origin, pointing to their discovery in a "surface formation." He also characterized most of the pieces of stone not as implements but rather as the "shop refuse" of recent tribes (Hrdlicka 1912, p. 150). This latter conclusion was apparently an attempt to contradict Ameghino's view that the crude nature of the objects was supportive of their being of extremely great antiquity,

Holmes stated: "The inclusion of such objects in superficial deposits which are subject to rearrangement by the winds and by gravity is a perfectly normal and commonplace occurrence" (Hrdlicka 1912, p. 150). As we have seen, it is not certain that the top layer of Ameghino's Puelchean formation, in which implements were found by Hrdlicka at a depth of 1.5 feet, should be classified as such a superficial deposit, especially one that could be rearranged by the wind. Even the large dune surmounting the stratum in which the implements were found was covered with grass and fixed (Hrdlicka 1912, p. 363),

Getting to the real heart of the matter, Holmes stated: "Such differences as may arise between the writer's interpretation and those of Doctor Ameghino are probably due in large measure to the fact that the points of view assumed in approaching the problem of culture and antiquity are widely at variance. Doctor Ameghino takes for granted the presence in Argentina of peoples of great antiquity and extremely primitive forms of culture and so does not hesitate to assign finds of

objects displaying primitive characteristics to unidentified peoples and to great antiquity, or to assume their manufacture by methods supposed to characterize the dawn of the manual arts, To him all this is a simple and reasonable procedure" (Hrdlicka 1912, p. ISO), This is not a fair characterization of Ameghino's work, for it is quite clear that in addition to the form of tools he also took into consideration their geological position, which for him served as the chief indicator of their age, If one finds stone implements in geological strata of a certain age, one is certainly justified in attributing them to a people that lived at that time, It would appear that accusations of bias and preconception are more properly directed at those who, like Holmes, Hrdlicka, and Willis, assume from the start that the human occupation of North America and South America goes back no further than a few thousand years, and who therefore dismiss, in various unfair ways, the extensive evidence that indicates a much more ancient human presence.

Holmes directly revealed his prejudice: 'The writer finds it more logical to begin with the known populations of the region whose culture is familiar to us and which furnishes lithic artifacts ranging in form from the simplest fractured stone to the well-made and polished implement, and prefers to interpret the finds made, unless sufficient evidence is offered to the contrary, in the illuminating light of known conditions and of well-ascertained facts rather than to refer them to hypothetic races haled up from the distant past" (Hrdlicka 1912, p. 150). Scientists are certainly entitled to their predispositions, which play a covert but substantial role in their supposedly objective evaluation of evidence. In this case, however, Holmes's overt preferences appear to have played too exclusive and dominant a role. To be sure, Holmes offered the condition that Ameghino's stone implements must be attributed to modern Indians unless "sufficient evidence is offered to the contrary." But what is sufficient contrary evidence? For someone with a strong negative bias, no contrary evidence will prove sufficient.

Holmes stated in his report: "Nothing short of perfectly authenticated finds of objects of art in undisturbed formations of fully established geologic age will justify science in accepting the theory of Quaternary or Tertiary occupants for Argentina" (Hrdlicka 1912, p. 149). Ameghino, of course, fully believed he had satisfied these criteria. Paleontological truth, it would appear, is, like beauty, in the eye of the beholder. Furthermore, as we have documented previously, objects of human industry have elsewhere been discovered by professional scientists in undisturbed formations of great antiquity, and yet reasons were still found to reject them. For example, we have Ribeiro's testimony that he extracted flint implements from within the interior of Miocene limestone formations in Portugal (Section 4.1), and yet opponents nevertheless found ample reason to disagree with his interpretation of their age. It seems clear that Holmes was selectively requiring an impossibly stringent standard of proof for evidence that challenged his preferred views.

#### 5.1.5 Other Finds by F. Ameghino

What do modern authorities have to say about Florentino Ameghino? Not much, because most modern authorities will not even have heard of Ameghino or his discoveries—both buried many decades ago. But if we go back to the 1950s, we can find some references to Ameghino by one of the scientists who did the burying—Marcellin Boule, author of the classic text *Fossil Men*. After

pointing out that Ameghino, like his contemporaries in Europe, had discovered stone implements and other evidence for a human presence in the Pliocene and Miocene, Boule added: "Ameghino also recorded facts of the same kind from much more ancient deposits dating, according to him, from the Oligocene and even from the Eocene. He claimed that they were rudimentary implements manufactured and used by the small apes of these remote periods, the supposed ancestors of the human kind. These statements are not even worthy of discussion" (Boule and Vallois 1957. p. 491). Boule may be commended for his candor. which demonstrates the parochialism sometimes manifest in the scientific mentality. In all fairness. why should not evidence presented by a professional scientist at least be considered and discussed. even if it does completely contradict accepted views?

Of course. this does not mean that one should uncritically accept everything Ameghino said. For example. Ameghino wanted to attribute some of his older stone tools to primitive apelike precursors of modern humans. But as we have several times noted. even the simplest types of tools are made and used today by culturally primitive yet fully human peoples. Furthermore. there is from parts of the world other than Argentina abundant evidence that points to a fully human presence throughout the Tertiary. One would therefore be fully justified in leaving open the possibility that humans of the fully modern type were responsible for the manufacture of any of the tools found by Ameghino in Argentina. including the oldest.

Indeed. one of the main tactics employed by Hrdlicka against Ameghino was to show that the fossil bones of presumed Tertiary human precursors found by Ameghino were in fact identical to those of morphologically modern humans (Sections 6.1.5. 6.2.4). For Hrdlicka. who firmly believed in the recent origin of the human species. this meant that Ameghino's fossils were also recent. But it could also mean something else. namely. that the skeletal remains were from the Tertiary. as Ameghino so ably maintained. and were. as Hrdlicka so ably demonstrated. anatomically modern.

The complete original reports of the finds Ameghino regarded as Eocene and Oligocene have proved very difficult to track down. As of this writing. we have bibliographical references giving the titles of these publications. which were small pamphlets of 8 pages each. apparently presented as papers at a scientific conference (F. Ameghino 1910a; 1910b). Ameghino did. however. refer to the discoveries described in these two papers in an article that appeared in 1912. "Recently" he wrote. "I have published a report on new materials. very well substantiated. found in the Entrerriean formation" (F. Ameghino 1912. p. 74). According to Ameghino. the Entrerriean formation could be assigned to the Late Oligocene. or perhaps the Early Miocene. He then mentioned a second report about discoveries in a formation he regarded as Late Eocene. the Santacrucian.

Today the Santacrucian formation. which Ameghino considered Late Eocene. is referred to the Early and Middle Miocene (Marshall et al. 1977. p. 1326). It would thus be about 15-25 million years old. We have not encountered any mention of the Entrerriean in the current literature we have examined. but since this formation comes before the Monte Hermosoan. it would be at least Late Miocene. over 5 million years old.

In the two reports published in 1910, Ameghino had apparently discussed only stone tools. Afterwards, Ameghino found other signs of a human presence. F. Ameghino (1912, p. 72) therefore wrote: "I can announce that I possess from these two formations even newer materials still more demonstrative than those I have published. Regarding this new material, I am not bringing into consideration more eoliths, which we find in our formations at the close of the Eocene and which differ from those of Boncelles in Belgium in that they are of much smaller size. Instead I base my assertions on bones that have been incised, cut, scraped, and split and on the vestiges of fire, found in the same beds as the bones." The modified bones and signs of intentional use of fire found along with stone tools at these two sites support the idea that anatomically modern humans may have been present in Argentina prior to the time of the Montehermosan, which is considered to be 3-5 million years old.

#### 5.1.6 Evidence for the Intentional Use of Fire

Let us now consider in detail an important category of evidence accompanying Ameghino's discoveries of stone tools—signs of intentional use of fire. At various locations, along with stone tools, Florentino Ameghino discovered remnants of hearths, in the form of burned earth (tierra cocida), slag (escoria), charcoal, and burned animal bones. This combination of evidence tends to strongly confirm the view that the tools were manufactured by human beings in the distant past. In some cases, Ameghino interpreted the presence of scoria (slag) and burned earth as signs of grass fires intentionally set by primitive hunters.

Ameghino gave great importance to his discoveries of burned earth and slag. While in Argentina, Hrdlicka and Willis therefore collected many such specimens. At Miramar, for example, Willis found broken chunks of red tierra cocida and pieces of heavy black scoria 8-10 centimeters [3-4 inches] in diameter, all of which "occurred in the undisturbed Pampean" (Hrdlicka 1912, p. 47).

Some scientists thought the Miramar tierra cocida and slag were the product of volcanoes. But Whitman Cross of the U.S. Geological Survey had conducted studies of the slag and burned earth. Willis stated: "According to Mr. Cross . . . they are probably not volcanic" (Hrdlicka 1912, p. 47). Some authors suggested grass fires as the cause. Cross tested this idea by burning the most common Pampas grass (cortadera) on samples of earth, but this produced only a very thin layer of hardened earth, with no bricklike tierra cocida or melted scoria. But Willis, while visiting the Rio Colorado region of Argentina, observed another kind of grass, called esparto, that grows more deeply into the earth, and saw a place where it had burned. At this location, he observed one could pick up pieces of brick-colored earth up to 10 centimeters [4 inches] in diameter. Some of the pieces were penetrated with grass roots and carbonized grass, as in the case of some of the specimens described by F. Ameghino (Hrdlicka 1912, pp. 46-48).

In his reports about Monte Hermoso and other Argentine sites, Ameghino had noted the presence of similar specimens. He said that Dr. Gustave Steinmann came to Argentina during an expedition to South America, and in 1906 visited the barrancas of the Atlantic coast near Cabo Corrientes, accompanied by Santiago Roth and Robert Lehmann-Nitsche. F. Ameghino (1908, p. 106) stated:

"These gentlemen discovered in the barrancas pieces of burned and partially vitrified earth, reporting specimens resembling or identical to those from the beds at Monte Hermoso, which I had attributed to the action of man and presented as proof of his existence in that distant epoch. "

But Steinmann believed that humans had appeared in South America only in recent times. F. Ameghino (1908, p. 106) noted: "In a report presented during the course of the past year by Dr. Steinmann at the Geological Society of Berlin, he stated that these reputed vestiges of *Homo amencanus* were in fact natural productions that appeared to be caused artificially only in the imaginations of recent immigrants of the species *Homo europaeus*. According to Dr. Steinmann, the specimens were pieces of volcanic lava which had arrived there through the air or more probably by means of water currents." The nearest volcanoes, however, were a thousand kilometers (621 miles) from the Atlantic coast, in the Cordillera, the mountain range running the length of western Argentina. Still, Steinmann believed that small pieces of scoria were transported by rivers.

F. Ameghino (1911, pp. 68- 69) responded: "Although all the strange affirmations of M. Steinmann will be refuted in detail in a monograph I am preparing, the facts have been so misrepresented by him that I cannot restrain myself from remarking that all that he has said in connection with the relative antiquity of man in South America and Europe is a natural result of his preconceived ideas. For Steinmann, the presence in true geological formations of scoria is an illusion, and the supposed formations do not actually exist. The pieces of scoria he encountered may be no bigger than nuts or somewhat bigger. But I have found masses of burned earth weighing many kilograms, the transport of which from the Cordillera to the places in which they are found, by means of movement through the air or by rivers, is impossible. Contrary to his statements, the scoria are accompanied by, that is to say, they are embedded in the same strata with, other vestiges of the activity of man (burned and broken bones, etc.)."

F. Ameghino (1908, p. 106) further stated about Steinmann's hypothesis: "Fantastic though it may be, this opinion is not completely new: I mentioned it 18 years ago, but did not consider it worth much discussion. Dr. Steinmann, in characterizing these vestiges as volcanic lava, has proceeded with excessive haste. What he has characterized as volcanic lava is a product resulting from the burning of fires intentionally set in dry grass." Ameghino noted that modern Indians sometimes burn dry Pampas grass to drive out small game for hunting, producing fused earth, which, because of the holes left by the roots, resembles lava. He held that the ancient Tertiary inhabitants of Argentina had done the same (F. Ameghino 1908, p. 107). Of course, one could also propose that the grass fires could have been started by lightning strikes.

But these light and porous specimens of burned earth were not the only kind found by Ameghino. Other specimens, from a variety of sites along the coast, were harder and more solid. Noting this distinction in his own research, Hrdlicka (1912, p. 50) stated: "Small particles and occasionally larger masses of terra cocida, were found by Mr. Willis or the writer in a number of localities along the coast from northeast of Miramar to Monte Hermoso, and were relatively abundant in the deposits exposed in the barrancas at the former locality. They occur at different depths from the

surface, to below the sea level at ordinary low tide. The pieces collected are all compact, with the exception of two or three that show on one side a transition to scoria. While there is a general resemblance, they all differ in aspect and weight from the very porous, light products of the burning of the esparto grass, collected by Mr. Willis on the Colorado."

As we have seen, Ameghino thought some of the compact pieces of scoria and burned earth were the remnants of fogones, or fireplaces, rather than grass fires, which may, it seems, have been set naturally rather than by humans. But Willis rejected human action in all cases. About some specimens from Monte Hermoso, Willis stated: "Through the courtesy of Doctor Ameghino the writer saw at Buenos Aires 10 pieces of burnt clay which would appear to have formed a layer about 10 by 15 cm. [4 by 6 inches] in area and about 5 to 10 mm. [.2 to .4 inch] thick, collected by Ameghino from the Monte Hermosean formation below high-tide level. As stated in describing certain observations on burnt earth of the Pampaeen, the writer finds that clays of that formation may be burnt without the agency of man, and he does not attach any significance to the occurrence of burnt earth as an evidence of man's existence in the Miocene (?) 'Monte Hermosean'" (Hrdlicka 1912, p. 364).

Willis also stated: "In order to prove that man maintained a fire which burned a particular mass of tierra cocida It would be necessary to bring independent evidence of his handiwork" (Hrdlicka 1912, p. 364). In many cases Ameghino did, however, supply such independent evidence. Hrdlicka himself noted that "burnt bones, carbon, and other substances that might possibly be due to man have been found at or near fogones" (Hrdlicka 1912, p. 50).

Willis was quick, perhaps too quick, to dismiss this evidence. He wrote: "Two classes of facts have been cited to demonstrate his [man's] agency: The presence of supposed artifacts and the arrangement of a mass of burnt clay: chief among the former are split, broken, or scratched fragments of bone, and it appears to the writer that these may be referred, with greater probability, to weathering, biting, gnawing, and accidents incident to the wanderings of bones. as strata were eroded and redeposited" (Hrdlicka 1912, p. 48). Willis's remarks about the bones are extremely suspicious, especially when considered in the light of our discussion of the treatment of such evidence in Chapter 2. Also, it should be kept in mind that Willis was a geologist, with no particular training in the study of incised bones. Any fair-minded investigator would want to have a careful look at those bones before accepting Willis's characterization of them.

Willis then stated: "Certainly the proofs of man's agency should be uncontrovertible and the possibility of explanation by other than human action should be positively excluded, before the conclusion that he intentionally or incidentally burned the earth can be accepted" (Hrdlicka 1912, p. 48). Here Willis is demanding a level of certainty that empirical evidence relevant to paleoanthropology is incapable of providing. Scientists representing an establishment view often dismiss anomalous evidence by requiring it to meet a higher standard of proof than the conventionally accepted evidence.

It is, however, possible that the compact burned earth and slag were not the product of campfires. as proposed by Ameghino. Hrdlicka observed some contemporary fire sites. noting that reddening

and blackening of the earth was produced, but no cohesion. This suggested the improbability that the compact tierra cocida resulted from campfires (Hrdlicka 1912, pp. 49-50). Furthermore, specimens of tierra cocida were sent to Washington, D.C., where they were examined by Frederick Eugene Wright and Clarence N. Fenner of the Geophysical Laboratory of the Carnegie Institution. These researchers reported that the tierra cocida was composed of Pampean loess heated at 850-1050 degrees Centigrade, a temperature they said was too high to be attributed to either grass fires or small wood bonfires (Hrdlicka 1912, p. 88).

Evidence for a more intensive fire was suggested by the presence of the scoriae, or pieces of slag. According to the report of the Geophysical Laboratory, the scoriae examined there were not of volcanic origin. Wright and Fenner noted that the scoriae "do not agree with any known eruptive rock or lava in their microscopic features" (Hrdlicka 1912, p. 94).

Wright and Fenner went on to note some puzzling features of the scoriae. First they were a melted loess, but the melted loess was not composed of the same materials as the layers of loess from which the scoriae had been extracted. To Wright and Fenner, this indicated the scoriae had not been produced by fire in that locality. Second, although the glassy scoriae contained iron compounds, they were not reddish in color, as would be the case if the iron compounds had been exposed to oxygen. This indicated that the scoriae were not formed by the action of fire in the open air. The scientists of the Geophysical Laboratory, straining for an explanation, suggested that the scoriae were produced underground by an extrusion of molten lava from deep within the earth, which melted a loess different from that found in the surface layers (Hrdlicka 1912, pp. 93-97).

But there are many difficulties with such an explanation. First of all, as noted by Wright and Fenner, there was no sign of any extrusion of lava in the strata throughout which the scoriae were found scattered. The researchers of the Geophysical Laboratory nevertheless stuck to their opinion that contact of loess with molten lava was the most likely cause of the scoriae. But they had to go to great lengths to explain away the absence of any normal lava at the sites from which the scoriae had come: "it may be that the volcanic extrusion was of the explosive type, whereby the lava . . . was shattered and reduced to dust, which fell to the surface as volcanic ash and now constitutes an integral part of the loess formation. Under these conditions the cooler, viscous, melted loess fragments would remain intact and be ejected as scoriae and resist attrition and breaking down more effectively than the shattered volcanic lava" (Hrdlicka 1912, p. 96).

#### 5.1.7 Primitive Kilns and Foundries?

The lava hypothesis of Wright and Fenner involves a quite extraordinary chain of speculative reasoning. There is, however, a possible explanation for the burned earth and slag that places considerably less strain on the limits of credibility—namely that they might be the result of intentional fire of a type other than campfires. Even today, one can observe inhabitants of many areas of the world making use of primitive foundries and kilns. Let us therefore consider the hypothesis that the burned earth and slag present on the Argentine coast are the byproducts of crude iron smelting furnaces. This idea was suggested to us by Arlington H. Mallery's book *Lost*



America. which describes primitive iron furnaces discovered in Ohio and other locations in North America. Mallery thought the makers of the furnaces came from Europe. Since the type of process used in these foundries went out of use in Europe before the time of Columbus, Mallery therefore concluded that the furnaces he found in America must have been used by pre-Columbian European immigrants. And this, according to standard views of history, is unexpected. Admittedly, the kiln or foundry hypothesis is speculative, but no more so than the disappearing lava hypothesis offered by Wright and Fenner.

Mallery (1951, p. 100) stated: 'The earliest iron-smelting furnaces in both the Old World and the New were merely shallow pits with rounded bottoms located on the hilltops. In order to catch the usual up-draft of air from the valley below for combustion. they were built close to the edge of the hillside facing the prevailing winds." In Argentina. the prevailing winds are the southeast trades that blow in from the ocean, so it seems the coastal slopes would be suitable for natural draft furnaces. Mallery (1951. p. 199) further stated: 'The bottoms of these pit-furnaces were frequently covered with a layer of clay spread evenly to form a rounded basin from six to twelve inches deep."

Describing the smelting process, Mallery (1951, pp. 197-198) stated: "iron smelting was performed in three distinct stages utilizing, as a rule, bog ore from swamps. The ore was first piled up in heaps on layers of wood fagots and heated or calcined until it was red. It was then mixed with fuel and burned in a smelting furnace operated at a temperature below the melting point (about 2100 degrees) of cast iron. At or below this temperature, the fusible material in the ore became a fluid slag which seeped down and formed a pool in the pit of the furnace. The Iron and mineral oxides in the ore were carried down with the slag and collected in a porous lump or bloom at the bottom of the pool. When the melt was completed the fire was quenched with water and the Iron-workers lifted the bloom, still red hot. out of the furnace. It was then beaten with stones or heavy hammers to squeeze out some of the contained slag. In the finishing stage the bloom was usually taken to a smithy, reheated in a smaller furnace or forge, and hammered to squeeze out more of the slag, the process being repeated until forgeable wrought iron was obtained."

What exactly is bog ore? Mallery (1951. p. 199) explained: "Bog ore is a yellowish-brown. clay-like material composed mainly of clay, loam. and hydrated oxides of iron. Some pottery maker who attempted to use bog ore instead of clay for his pots may have discovered the iron-extracting process. . . . Even now, the small closed furnaces used by the Agaria in India and, until recently, by the Liberian natives, resemble pottery kilns."

As it turns out. there is an Iron-rich earth at Miramar and other localities on the coast. For example, Wright and Fenner analyzed specimens from Miramar, describing them as "brown ferruginous earth" with "pronounced accumulation of limonitic material" (Hrdlicka 1912, p. 70). Limonite is an iron ore. Wright and Fenner also observed: "Brown ferruginous earths have also been considered tierra cocida by some investigators. A careful microscopic examination of these specimens has shown that they are simply loess in which ferruginous material abounds" (Hrdlicka

1912, p. 89). It is possible that these ferruginous earths could have served as the raw material for iron smelting.

A key indicator is the iron content of the slag left over from smelting. Mallery (1951, p. 200) pointed out: "The iron content of the slag . . . in the mounds of England, Belgium, Scandinavia, Virginia, and the Ohio Valley is very high—from

10 per cent to 60 per cent. Slag produced in modern blast furnaces, which have been in general use since the fourteenth century, seldom contains more than one per cent iron." He then gave a specific example: "On top of Ohio's Spruce Hill is an extensive deposit of slag. In this deposit are several low mounds composed mainly of typical hearth-pit slag, which tests show has an iron content of about ten per cent. Cutting a short trench into this heap, I uncovered the edge of a twelve-inch slab of clay. In the heap were large pieces of slag, lumps of red-burned bog ore, charcoal and glazed stone" (Mallery 1951, p. 204).

How does this compare with the slag found on the Argentine coast? Chemical analysis of a scoria sample from north of Necochea revealed 9.79 percent iron compounds (Hrdlicka 1912, p. 81). Another piece of scoria from San Bias, north of Rio Negro gave 9.71 percent iron compounds (Hrdlicka 1912, p. 86). Several other samples yielded at least 5 percent iron compounds.

The following description of a crude furnace uncovered in Sweden is interesting when compared with the evidence discovered in Argentina. John Nihlen stated: "The owner of the farm found some pieces of slag on a hill about two hundred meters south of the farm. In a smaller pit here was found under the grass, one-half meter [20 inches] deep, a large amount of slag pieces, such as iron slag in chunks of glazed pieces mixed with or attached to pieces of hard-burned red clay. At the bottom of the pit was dark sand and a few cinders of charcoal but no real burned material. Around the pieces of slag were some round stones but no real construction of stone" (Mallery 1951, p. 204).

Of particular interest in the above statement are the pieces of scoria "mixed with or attached to pieces of hard-burned red clay." At Miramar, reported Wright and Fenner, Hrdlicka and Willis collected some specimens of "tierra cocida and scoriae combined" (Hrdlicka 1912, p. 73). Wright and Fenner described a particularly interesting example: "The hard specimen shows a regular and uniform transition from a dark-gray scoria filled with small vesicles to a brick-red material, which bears a close resemblance to some of the specimens of baked earth. It is different from the latter, however, in this respect that, while the baked earths have a close, compact texture, the portion of this specimen which resembles them most... is filled with minute holes and is distinctly glassy in character.... A careful determination of the mineral fragments in the black and the red portions of the specimen proved them to be of the same general size and kind. Superficially the red portion of this specimen resembles the baked earths, but closer examination has shown it to be distinctly different. Its glassy, vesicular texture throughout is indicative of melting; the red coloration may be the result of alteration or oxidation, whereby magnetite has been changed to the red oxide of iron" (Hrdlicka 1912, pp. 73-74).

At another location in Sweden. John Nihlen discovered another furnace, and described it as follows: "While the gravel was being dug, pieces of slag were found here and there, none of them collected in heaps nor visible on the surface.

... It [the furnace] was about one meter [about 3 feet] wide in the upper part and narrowed slightly downward, being cup-funnelled at the bottom. The sides were made of round or flat gray stone which were laid in clay which also covered large parts of the inside. Probably the lining was not over the stones. The bottom of the furnace . . . consisted of a ten-centimeter [4 inch] layer of hard and partially burned clay. It could almost have been taken out. In the cup-formed lower part there still remained a ten-centimeter layer of slag, bog ore, and charcoal. The depth of the furnace was about one meter. ... it had been a simple earth furnace without a blast intake, built of stone and clay and with a thick bottom of burned clay" (Mallery 1951, p. 201).

Here we take note of the furnace bottom, which consisted of a "ten-centimeter layer of hard and partially burned clay." Willis described a similar section of hardened red earth found in the Chapadmalalan beds of a seaside barranca, or cliff, at Miramar. The Chapadmalalan, said by Ameghino to be of Late Miocene age, is dated by modern authorities to the Late Pliocene (about 2-3 million years before the present). According to Willis, the section of burned earth was approximately 1 meter, or just over a yard, long and 30 centimeters, or about a foot, deep. The upper part was of red clay, passing into a dark brown and black mass that faded into the brown loess. Willis stated: "The principle mass of red clay is 60 cm. [about 2 feet] long and 10 cm. [about 4 inches] thick" (Hrdlicka

1912, p. 46). Willis attributed this particular specimen to a process of chemical dehydration, but admitted that its "coloring might have been occasioned by a fire burning on the surface that is now red" (Hrdlicka 1912, p. 46).

It would, however, have taken an extraordinarily hot fire to produce the observed effects. Wright and Fenner stated: "The assumption that the large specimens of terra cocida were formed simply by the action of open fires is hardly possible in view of the quantity of heat involved, which must have acted through a period of time on large masses of material to have produced the effects observed" (Hrdlicka 1912, p. 85). They further stated: "Many of the specimens of terra cocida are so large and compact that one is forced, in explaining their mode of formation, to assume long-continued and confined heating at a fairly high temperature, such as would be encountered near the contact of an intrusive igneous or volcanic mass, but not beneath an open fire made of grass or small timber" (Hrdlicka 1912, p. 89). But there was no evidence of intrusive volcanic masses at the sites under consideration, and "long-continued and confined heating at a fairly high temperature" is characteristic of a kiln or furnace.

The furnace hypothesis would explain the dark gray rather than red color of some of the scoria. Wright and Fenner, in conducting thermal experiments, noted that when small samples of loess were burned they turned red because all the loess particles were exposed to oxygen. But when larger masses were burned, oxygen did not reach the interior, which remained gray, like some of the Argentine scoria (Hrdlicka 1912, p. 88). As we have seen, the smelting process outlined by

Mallery involved burning large masses of ore, the interior of which may have remained gray. Furthermore, the primitive furnaces operated on the principle of reduction rather than oxidation, which would also account for grayish rather than reddish slag.

In summary, we propose the following. While some types of *tierra cocida* might have been produced by grass fires, campfires, or perhaps even chemical dehydration, the thick, hard, red pieces of *tierra cocida* at the Argentine sites might well have been the burned earth that lined the bottom of primitive smelting furnaces. Samples of this burned earth were, according to Wright and Fenner, generally of low iron content (Hrdlicka 1912, pp. 88-89). Also found at the Argentine sites were pieces of brown ferruginous earths, which might represent unburned ore. Wright and Fenner did not give a chemical analysis of these earths, but they would appear to be of high iron content. The gray scoria and the gray-and-red scoria, as indicated by their iron content of about 10 percent, could represent the slag from primitive iron smelting furnaces operated on the Argentine coast several million years ago. Other specimens of scoria might have been produced in connection with pottery kilns.

#### 5.1.8 Ameghino on the South American Origins of Hominids

Florentino Ameghino proposed that human beings evolved on the South American continent and then migrated first to North America, and thence by separate routes to Europe and Asia. At this point, many will doubtlessly conclude that Ameghino was simply an overly patriotic Argentine nationalist promoting the totally absurd view that humans originated in the country of his own birth.

But the same skeptics accept without similar reserve the claims of scientists such as Leakey, Broom, and Dart, who resided in former British possessions in Africa and proposed that the human race just happened to originate there.

In fact, paleoanthropologists the world over have a tendency to claim their homelands as the cradle (or one of the cradles) of humanity. Scientists from China, India, and the former Soviet Union (Section 3.6.4) maintain such conceptions. Underlying most such claims is the assumption of a monogenetic evolutionary origin of the human race—that human beings evolved only once from apelike ancestors within a certain region and then radiated from there to populate the rest of the world. Today, the dominant view in science is that the first apelike human ancestor (*Australopithecus*) arose in Africa in the Pliocene. In the Early Pleistocene, this creature attained protohuman status (as *Homo habilis* and then *Homo erectus*). Further evolutionary progress in Africa resulted in the emergence of *Homo sapiens sapiens* about 100,000 years ago.

As we have seen, an important confirmation of human or protohuman status is the presence of stone tools, such as those found in Bed I of Olduvai Gorge. These tools, among the oldest given unqualified recognition, are attributed to *Homo habilis* in the Early Pleistocene. According to the monogenetic evolutionary assumptions underlying modern paleoanthropology, one should expect to find tools dating from the very early Pleistocene only in eastern or southern Africa.

Florentino Ameghino, however, discovered stone implements in strata dating back to the Early Pliocene (3-5 million years b.p.) and even as far back as the Miocene—and in Argentina instead of Africa. Along with stone implements, Ameghino found abundant signs of human occupation, such as evidence of fire, burned bones, incised bones, and human skeletal remains.

So one might wonder, do we intend to give support to Ameghino's claims that humankind originated in Argentina? Hardly. But we do feel that the evidence uncovered by Ameghino lends strong support to the conclusion that the whole concept of a monogenetic evolutionary origin for humanity, be it in Kenya, Argentina, Siberia, China, or Kashmir, is incorrect. If at various points around the world one can find stone implements and other evidence for the presence of human beings dating back as far as 20 million years, there is good reason to suspect that the current picture of human origins and antiquity is completely wrong.

It just might be that the version of human ancestry promoted by the dominant Anglo-American school of anthropology, namely that humans originated in former British possessions in Africa, deserves no greater credibility than an Argentine scientist's claim that humans originated in South America. Indeed, humans may not have evolved at all. Human beings may have been present on this planet, in their current form and at essentially the same level of cultural advancement, for as far back in time as we can carry our investigations. That is what the totality of the evidence—not the carefully edited selection of evidence found in current textbooks—actually suggests. Or to put it another way: the hypothesis that human beings of the fully modern type have existed on this planet for several millions of years accounts for all the available evidence, in the form of stone implements, incised bones, and human skeletal remains, more fully than the modern evolutionary theory, which survives only by discarding, under various excuses, a vast number of discoveries made by scientists over the past 150 or so years. The discoveries of Florentino Ameghino are a case in point.

## 5.2 Tools Found by Carlos Ameghino at Miramar (Pliocene)

After Ales Hrdlicka's attack on the discoveries of Florentino Ameghino, Ameghino's brother Carlos launched a new series of investigations on the Argentine coast south of Buenos Aires. From 1912 to 1914, Carlos Ameghino and his associates, working on behalf of the natural history museums of Buenos Aires and La Plata, discovered stone tools in the Pliocene strata of a barranca, or cliff, extending along the seaside at Miramar.

### A?5.2.1 Age of Site Commission of Geologists Confirms

In order to confirm the age of the implements, Carlos Ameghino invited a commission of four geologists to give their opinion. The geologists were Santiago Roth, chief of the paleontology section of the Museum of La Plata and director of the Bureau of Geology and Mines for the province of Buenos Aires; Lutz Witte, a geologist of the Bureau of Geology and Mines for the province of Buenos Aires; Walther Schiller, chief of the mineralogy section of the Museum of La Plata and consultant to the National Bureau of Geology and Mines; and Moises Kantor, chief of the geology section of the Museum of La Plata.

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In their report, the commission of geologists (Roth et al. 1915, p. 419) first told what they were asked to investigate: "The two questions are: (1) Were the objects in question found in primary deposits, that is to say, were they covered over at the time the deposits were being laid down, or is there reason to doubt this and to suppose instead that the objects were buried by a different cause at that site, at a time later than the formation of the respective deposits? (2) Concerning the stratigraphic position of the beds that contain the objects, can it be determined if they correspond to levels of the Eopampean horizon (the Montehermosan of F. Ameghino): or were the respective sediments more recently deposited against an ancient barranca, or in an eroded valley or some other depression of the earth that corresponds to the later part of the Pampean series?"

I -1

Stratigraphy of Coastal Provinces of Arinin

Marshall et. al. 1982

F. Ameghino 1909

Roth e, 191

Plateau

Postpamj.

Lujamai

Lujanian

Neopamj:

Bonarian

Ensenadan

Ens

Mesopaii

Uquian

Puelchean

Chapadmalalan

Chapadnalalan

Eopampe

Montehemio san

Montehermosan

The report (Roth et al. 1915, p. 420) then went on to describe the stratigraphy of the barranca in some detail, making use of a nomenclature different from that used by either Ameghino or modern authorities (Table 5.1): 'The cliff displayed four Pampean horizons: Eopampean (the Montehermosan and Chapadmalalan of F. Ameghino): Mesopampean

(Ensenadan); Neopampean (Bonarian and Lujanian): and Postpampean (Platean)."

Of special interest were the Mesopampean and Eopampean. The Mesopampean was a bank of water-redeposited loess, 3-4 meters [10-13 feet] thick, extending 500 meters [1640 feet] between two transverse valleys interrupting the barranca. Of the Mesopampean layer, the commission said: "C. Ameghino, Schiller, and Roth agree that the bank in question corresponds to the Ensenadan level in the subdivisions of the Pampean made by F. Ameghino" (Roth et al. "In some parts there are layers of rounded stones," reported the commission of geologists. "Also present is freshwater limestone, very common in the Mesopampean. The loess is traversed in all directions by veins or seams of calcareous tufa, which frequently form in such beds. These stratigraphic and lithological conditions make it impossible to suppose that cavities were formed and refilled after the Mesopampean formation was initially deposited" (Roth et al. 1915, p. 420).

This is an important consideration. One might say that the presence of stone implements in the Eopampean strata below the Mesopampean could be accounted for in the following way. Imagine a fairly recent Indian settlement on the top of the barranca. The villagers leave stone tools on the surface. Later, an arroyo forms in the barranca, cutting through the Mesopampean layers into the Eopampean formation. Stone tools are washed into the bottom of the arroyo. Later, the arroyo is refilled, leaving stone tools in the Eopampean layer. As we shall see, this is exactly the sort of challenge that would be made (Section 5.2.3).

According to the commission, the geological evidence ruled out such cutting and refilling (Roth et al. 1915, p. 420). The Mesopampean formation contained distinct layers of stones, seams of calcareous tufa (a porous limestone-like material), and deposits of tosca (a hard limestone deposit). All these would have been noticeably disturbed if cut by a gully that was later refilled.

The commission report then turned to the layers that contained implements: "The base of the barranca is formed of Eopampean deposits. Carlos Ameghino, Schiller, and Roth declare that the geological characteristics of this deposit are exactly like those of the loess found at the base of the Loberfa barranca south of Mar del Plata, where F. Ameghino originally established the Chapadmalalan formation. In both locales one encounters, according to Carlos Ameghino and Roth, remains of mammals typical of this level, among them an abundance of Pachyrucos [a small rabbitlike creature]" (Roth et al. 1915, p. 421).

The geologists went on to say: "The banks of calcareous tufa are almost completely absent, and in general the tosca is much scarcer than in the upper beds; but despite this, the loess forms a very consistent mass and to break it required a pick and crowbar. The general aspect is of an eolian loess, formed of very homogeneously pulverized mineral substances" (Roth et al. 1915, p. 421). The identification of the Eopampean as eolian (wind-deposited) loess is important, for later an opponent (Romero 1918) would charge the layers were of marine origin (Section 5.2.3).

The geologists noted that the loess contained pieces of burned earth and scoria (slag). "Moreover," said the report, "at distinct locales in the barranca, the original investigators found objects, instruments, and weapons of stone, fabricated by different techniques" (Roth et al. 1915, p. 421).

"The first deposit of implement-bearing loess to be examined," said the geologists, "was approximately 50 meters [164 feet] from the small drainage channel that exists in the slope of this barranca and more or less 1 meter [about 3 feet] lower than the limit of the horizon between the Mesopampean and the Eopampean" (Roth et al. 1915, p. 421).

### 316 5. Advanced Paleoliths and Neoliths

The geologists then recounted how the initial discoveries took place: "The first objects were discovered, according to Torres and Ameghino, when Lorenzo Parodi [a collector employed by the natural history museums of Buenos Aires and La Plata] attempted to extract a piece of slag. Parodi's pick struck a hard object, which when uncovered, turned out to be a bola stone. It was extracted encased in a chunk of loess, and it is preserved in the same condition in the Museum of Natural History in Buenos Aires. Later, Torres, Ameghino, and Doello-Jurado, digging at the same site, discovered other stone objects and instruments, and finally Parodi very recently discovered a round stone and a flint knife in place, and left them there, following the instructions he had received, in order that they could be extracted in the presence of this commission of geologists" (Roth et al.,

1915, p. 421). It is apparent from this description that the excavation was carried out with some degree of professionalism—the commission of geologists was able to study implements in situ.

The report then conclusively answered the first of the questions the geologists were asked: "This commission . . . after examining the place where the artifacts in question were found, gave their unanimous opinion that if the sediments had shifted after the time of deposition, the members



would have been able to see some alterations in the texture of the bed, but they were not able to observe any such alterations. The lithological composition of the sediments and the texture of the deposit that contained the artifacts did not demonstrate any difference in character from the loess of this horizon. All of those present declared that the stone artifacts . . . were found in intact, undisturbed terrain, in primary position. Based on this fact, the first question posed may be answered: visual inspection of the site where the artifacts were found has not given us any reason to suppose that the artifacts have been buried by any means whatsoever at a time after the formation of the bed. They are found in primary position and, for that reason, should be considered objects of human industry, contemporary with the geological level in which they were deposited" (Roth et al. 1915, p. 422).

The report was equally conclusive about the second matter under consideration: "In respect to the second question. whether it is possible that there was In this place a juxtaposition of strata, or if it is possible that the layer containing the artifacts was deposited up against an old cliff and therefore corresponds to one of the most recent levels of the Pampean formation. we firmly declare: the stratigraphic conditions at this place are so clear as to present no difficulty in resolving any such problem. In the first place, the above-mentioned layer of freshwater limestone, which corresponds to the Mesopampean horizon and is found directly above the Eopampean deposits. has not suffered any alteration. Nor has it been possible to find in any part of the bed In question any refilling of gullies or caverns after the bed's formation. Furthermore, the face of the cliff is quite vertical, which allows one to see clearly that the sediments found in the lower part were not deposited against an old cliff of the Mesopampean horizon. Rather the beds of the Mesopampean horizon pass in al places above the deposits in question. The undulations and irregularities presented by the horizon between the Mesopampean and Eopampean beds are filled with the calcareous tufa above mentioned. allowing one to distinguish a discordance between the two horizons. The committee of geologists is in accord that the second point In question can be defined in the following way: that the objects of human industry encountered in this place are situated In deposits of loess characteristic of the Eopampean horizon, which constitute the base of the barranca; and that the stratigraphic relationships allow us to establish with scientific certainty that there exists here no juxtaposition of newer layers and older ones" (Roth et al. 1915, pp. 422 - 423).

The Eopampean layers in which the implements were found correspond to the Chapadmalalan formation, sometimes called the Chapadmalean or Chapalmalean. Modern authorities assign the Chapadmalalan formation a Late Pliocene age of 2.5-

3.0 million years (Anderson 1984, p. 41) or 2.0 -3.0 million years (Marshall et al. 1982, p. 1352). In their world survey of Pliocene mammalian fauna, Savage and Russell (1983) list Miramar as a Chapadmalalan site.

Concerning the objects they examined in situ at Miramar. the commission of geologists reported: "The round bola stone, which was discovered in the loess and which was extracted in the presence of the commission, did not display any sign of human work; but from its form and size it would

appear to have served as a weapon, like the other bola previously discovered in the same stratum. The flint knife had fallen out onto the ground. but the place where it had been situated was noticeable; it presents every Indication of having been fabricated by percussion and pressure" (Roth et al. 1915, p. 423).

As stated before, burned earth (tierra cocida) and slag {escoria) were both found at the Miramar site. Earlier Hrdlicka and other researchers (Sections 5.1.6,

5.1.7) had rejected the possibility that the burned earth and slag discovered at various Argentine sites, Including the barranca at Miramar, could have resulted from fires of human origin. But the members of the commission of geologists reported: "Digging with a pick at the same spot where the bola and knife were found, someone discovered in the presence of the commission other flat stones, of the type that the Indians use to make fire" (Roth et al. 1915, p. 423).

Those committed to the standard view of human evolution will reflexively attempt to explain away all this evidence, but to those with more open minds, the facts clearly suggest that humans, capable of manufacturing tools and using fire, lived in Argentina about 2-3 million years ago in the Late Pliocene. The report further stated: "Carlos Ameghino, who continued the excavation, encountered another stone of small size, completely round and smooth, presenting the characteristics of a stone subjected to intentional work" (Roth et al. 1915. p. 423).

The commission made another find confirming its views: "About 50 meters [164 feet] from this site, in a layer still lower, there were found fossil remains of a species of the suborder Gravigrada [ground sloths]. During the excavation, there were discovered in this spot, in the presence of the commission, other round stones associated with the fossil remains. . . .

Considering all the circumstances surrounding this discovery, as well as the condition of the objects and their stratigraphic relation to the bed, the commission is of the opinion that they are objects manufactured by humans who lived at the time of the geological period corresponding to the Chapadmalalan" (Roth et al. 1915, p. 423).

#### 5.2.2 A Stone Point Embedded in a Toxodon Femur (Pliocene)

After the commission left for Buenos Aires, Carlos Ameghino remained at Miramar conducting further excavations in the Chapadmalalan beds Just northeast of the spot where the bola stone and flint knife had been found. Ameghino uncovered many fossils of animals characteristic of the Chapadmalalan, such as Pachyrucos, a rabbitlike creature, and Dicoelophoros, a ratlike rodent. These animals were absent from the overlying Mesopampean beds (C.

Ameghino 1915, p. 438).

From the top of the Late Pliocene Chapadmalalan layers, Ameghino extracted the femur of a toxodon, an extinct South American hoofed mammal. resembling a furry, short-legged, hornless rhinoceros. Ameghino discovered embedded In the toxodon femur a stone arrowhead or lance point (Figure 5.1). giving evidence for culturally advanced humans 2-3 million years ago in

Argentina. Those who are committed to the view that *Homo sapiens sapiens* evolved about 100,000 years ago in Africa will likely attribute Ameghino's discovery to an intrusion from upper levels. But we would request such persons to, at least for a moment, set aside their preconceptions and withhold judgement while considering the facts of this remarkable case. Significantly, the toxodon femur was not discovered alone.

Figure 5.1. This toxodon thighbone (femur), with a stone projectile point embedded in it, was discovered in a Pliocene formation at Miramar, Argentina (C. Ameghino 1915, photograph 2).

C. Ameghino (1915, pp. 438-439) reported: "As we proceeded with the excavation. there also appeared in the barranca almost all of the bones of the rear leg of the toxodon, still articulated and conserved in their relative positions. This is very evident proof that the toxodon femur was buried in the terrain contemporaneously with the formation of the bed and that it has not since been subjected to movement. In addition to the femur. the proximal extremity of which scarcely cropped out of the barranca, the bones, which. as mentioned. appeared articulated. Included, the tibia and fibula, the calcaneum [heel bone]. the scaphoid and other pieces of the tarsus [ankle]. and finally some metatarsals. All the facts make it absolutely certain that these remains were found in their primary position. Their condition is identical to that of all the fossils that appear in this part of the barranca and in those parts that extend many leagues to the north. There have been discovered in this same barranca, on many occasions, perfectly articulated skeletons of animals from the same period as the toxodon. One of the most notable is a skeleton of *Paohyrucos*, which was discovered and extracted by the naturalist M. Doello-Jurado." From C. Ameghino's description, it is clear that the femur with the stone point embedded in it was the femur that was part of the articulated leg.

In December of 1914. Carlos Ameghino, with Carlos Biuch. Luis Maria Torres, and Santiago Roth. visited Miramar to mark and photograph the exact location where the toxodon femur had been found. C. Ameghino (1915, p. 439) stated: "Like the previous visits, this last visit was full of surprises. . . . When we arrived at the spot of the latest discoveries and continued the excavations, we uncovered more and more intentionally worked stones, convincing us we had come upon a veritable workshop of that distant epoch. "

The many implements, including anvils and hammer stones, resembled. in form and lithic material. those of Florentino Ameghino's *piedra hendida* (broken stone) industry, discovered in the same region. Carlos Ameghino and Roth continued their investigations to the south at Mar del Sur and found stone tools in the Ensenadan level. The identification of the formation as Ensenadan had

been accomplished previously, by the excavation from the same bed of a portion of the skeleton of *Tyotherium cristatum*, a rodentlike mammal attaining the size of a small bear.

Taken together, the discoveries from the formations at Miramar and Mar del Sur, and other locations on the Argentine coast, are significant in that they show continuous habitation of the region by humans, from the Pliocene to recent historical times, with scarcely any change in the inhabitants' mode of living.

Returning to the toxodon bones found at Miramar, we find that Carlos Ameghino anticipated accusations that the bones had worked their way into the Chapadmalalan formation from above. In his report he stated: "The bones are of a dirty whitish color, characteristic of this stratum, and not blackish, from the magnesium oxides in the Ensenadan" (C. Ameghino 1915. p. 442). This tended to rule out any suggestion that the toxodon bones had been mixed into the Chapadmalalan from upper beds of lesser age. Ameghino further pointed out that some of the hollow parts of the bones were filled with the Chapadmalalan loess. If the bones were derived from another level, one might expect them to be filled with a different kind of material. Of course, even if the bones had worked their way in from the Ensenadan, they would still be anomalously old. Dates for the Ensenadan range from 0.4 -1.0 million years (Marshall et al. 1982, p. 1352) to 0.4 -1.5 million years (Anderson 1984, p. 41).

In describing the nature of the loess in which the bones had been discovered, C. Ameghino (1915. p. 442) said: 'The terrain surrounding these remains is a loess exceedingly fine and pulverized, a true aeolian loess, fairly well decalcified and of reddish grey tint. a loess which, as we have said, corresponds to the Chapadmalalan' (C. Ameghino 1915. p. 442). Furthermore, as we have seen (Section 5.2.1), a commission of geologists had confirmed that the Chapadmalalan beds at Miramar were intact, showing no signs of disturbance.

Those who want to dispute the great age attributed to the toxodon femur will nevertheless point out that the toxodon survived until just a few thousand years ago in South America. They will say: "Of course. these early researchers were often surprised to find evidence of a human presence in connection with remains of the toxodon. an animal they thought typical of the Pliocene or Miocene, but since that time scientists have discovered the truth—that the toxodon roamed South America until quite recently." The clear implication is that if early researchers had been aware of this fact. they would certainly have hesitated to make claims for the great antiquity of humans based, for example, on the association of stone tools with toxodon bones.

But the fact that the toxodon lived until the Holocene does not rule out the discovery of toxodon bones in older strata, such as the Pliocene, for the toxodon definitely lived during that period. But the survival of the toxodon does allow critics to cast suspicion on finds such as Ameghino's, despite the fact that such finds were made in clear stratigraphic contexts.

Early researchers were often aware that mammals characteristic of ancient strata persisted until recent times. This is certainly true of Carlos Ameghino (1915. p. 442), who reported that the toxodon he found at Miramar, an adult specimen, was smaller than those in the upper, more

recent levels of the Pampean stratigraphic sequence. This indicated it was a distinct, older species. Carlos Ameghino (1915, p. 442) believed his Miramar toxodon was of the Chapadmalalan species *Toxodon chapalmalensis*, first identified by F. Ameghino, and characterized by its small size.

Furthermore, Carlos Ameghino (1915, p. 443) directly compared his Chapadmalalan toxodon femur with femurs of toxodon species from more recent Pampean formations and observed: "The femur of Miramar is on the whole smaller and more slender." Ameghino then reported more details showing how the femur he found in the Late Pliocene Chapadmalalan of Miramar differed from that of *Toxodon burmeisteri* of more recent Pampean levels.

None of this is mentioned by later critics. Boule, for example, in dismissing the toxodon femur, simply stated that the toxodon persisted in South America until fairly recent times (Boule and Vallols 1957, p. 492). But that does not invalidate Carlos Ameghino's conclusions. Toxodons did exist in the Pliocene, and according to Ameghino, the toxodon femur he recovered at Miramar was from a Pliocene species of toxodon. This information was available to Boule, yet he did not mention it. One could therefore say that Boule's presentation was dishonest. In order to have made a fair challenge to Ameghino, Boule should have demonstrated that Ameghino was incorrect in asserting that the femur he discovered was characteristic of a Pliocene species of toxodon.

In researching this book, we have learned that statements found in textbooks and scientific papers cannot always be trusted to give fair and accurate information about key discoveries. One quickly discovers that apparently objective statements reflect personal bias and prejudice and are often deliberately misleading. Nevertheless, Boule was not guilty of one of the most effective techniques for dealing with disconcerting evidence—complete omission.

Concerning the toxodon discovery at Miramar, we again emphasize that the bones of an entire leg were found articulated (in their natural relative positions) in the Late Pliocene Chapadmalalan formation. This indicates that the animal died in the Late Pliocene and that its bones were incorporated into the formation at that time. If the bones of a toxodon from a much later period had somehow been washed into the Chapadmalalan, one would not expect them to have been articulated.

Carlos Ameghino (1915, p. 445) then described the stone point found embedded in the femur: "This is a flake of quartzite obtained by percussion, a single blow, and retouched along its lateral edges, but only on one surface, and afterward pointed at its two extremities by the same process of retouch, giving it a form approximating a willow leaf, therefore resembling the double points of the Solutrean type, which have been designated *feuille de saule*. . . . by all these details we can recognize that we are confronted with a point of the Mousterian type of the European Paleolithic period." That such a point should be found in a formation dating back as much as 3 million years provokes serious questions about the version of human evolution presented by the modern scientific establishment, which holds that 3 million years ago we should find only the most primitive australopithecines at the vanguard of the hominid line.

Near the end of his discussion about the discovery at Miramar of the projectile point embedded in the toxodon femur, Carlos Ameghino (1915, p. 447) made some statements about Ales Hrdlicka, who, as we have seen, had attempted to demolish the work of Florentino Ameghino: "We cannot remain silent about the book recently published in this connection by Ales Hrdlicka and his collaborators (*Early Man in South America*, Washington, 1912). This work, apparently impartial and conscientious, serves, on the contrary, to reveal, especially in reference to the evidence for fossil man in this part of the Americas, the preconceived ideas of its authors. The authors did not spend in the terrain itself the time materially necessary to arrive at any judgement, as we had a chance to personally observe, since we accompanied them on many excursions. Without ignoring any part of the truth that this work may contain, we are convinced the conclusions of Hrdlicka are completely exaggerated. And the main proof of this is the report of the commission of geologists [Section 5.2.1]." The weight of evidence suggests Carlos Ameghino's statements about Hrdlicka's book are fully warranted.

Carlos Ameghino (1915, p. 449), in concluding his report on the projectile point found in the Miramar toxodon femur, stated that "at least since the Chapadmalalan, that is, the Late Miocene [Late Pliocene say modern authorities], there have existed in this territory humans of the type *Homo sapiens*, who, as surprising as it may seem, were possessed of a grade of culture and advancement comparable to the most recent prehistoric inhabitants of the region."

### 5.2.3 Romero's Critique of the Miramar Site

Carlos Ameghino's views about the antiquity of humans in Argentina were challenged by Antonio Romero. In a paper published in the *Anales de la Sociedad Científica Argentina*, Romero (1918) contradicted not only Carlos Ameghino, but his more famous late brother, Florentino Ameghino, who had for many years conducted research establishing a human presence in Argentina during the Tertiary. Quite apart from his work in paleoanthropology, Florentino Ameghino had gained an international reputation in the fields of paleontology and geology. A great deal of Argentine national pride was thus invested in Florentino Ameghino, who had almost singlehandedly focused the attention of the world's scientific community on his country. Romero was therefore very careful to frame his criticism of the Ameghinos with attention to Argentine patriotic sensibilities.

Early in the twentieth century, a dominant group within the scientific community was trying to "bury" evidence suggesting a human presence in the Tertiary. Romero was a supporter of this policy. In his paper, Romero (1918, p. 22), called special attention to the book *Fossil Man*, recently published by Hugo Obermaier, a noted European scientist who dismissed F. Ameghino's conclusions about a human presence in the Miocene and Pliocene of Argentina. Taking Obermaier's view as correct, and representative of responsible scientific opinion, Romero suggested that Carlos Ameghino and his supporters, by insisting on a human presence in the Tertiary of Argentina, were bringing ridicule and discredit upon the Argentine nation. Concerning Florentino Ameghino, Romero pleaded that Argentine science should continue to hold him in high regard for his valuable and quite extensive work in the areas of geology and paleontology, but that it was now time to set aside his unfortunate conclusions in the area of paleoanthropology, and

thus preserve his reputation as a great scientist. Romero (1918, p. 15) wrote: "We now have to consolidate the monument of his work. casting out the fantastic discoveries that have so much preoccupied simpleminded spirits and cost our country so much, greatly injuring the work of the great scientist and his contribution to our culture."

As part of his Investigation of Carlos Ameghino's discoveries, Romero visited the Miramar area. There he took time to view the fairly recent stone implements displayed in the small museum of Jose Maria Dupuy, a local collector. The implements had been gathered from the *paraderos* (settlements) of the coastal Indians. Noting the similarity of Dupuy's specimens to those Carlos Ameghino had forwarded to the Museum of National History in Buenos Aires from his excavations in the Chapadmalalan formation at Miramar, Romero (1918, p. 12) stated that he was "convinced they were made by the same artificers who made those that are considered to belong to a fanciful epoch." In other words, Romero believed that Carlos Ameghino's discoveries were manufactured by Indians in relatively recent times.

Romero (1918, p. 15) went on to state that notwithstanding the similarity of C. Ameghino's Miramar implements to objects recently manufactured by Indians "there are arguments of a more fundamental order that we intend to pose in support of our thesis." Romero (1918, p. 15) said he would "demonstrate with incontestable facts, in the plain light of truth, that it was false to suppose that artifacts discovered at Las Brusquitas [Miramar], resembling classical types of the Neolithic age, can be attributed to human beings that existed in the Miocene [Late Pliocene according to modern estimates]."

After reading Romero's combative introductory remarks, one might expect to find in his report some cogent geological reasoning backed up with convincing facts. Instead one finds assertions backed up with little more than some unique and fanciful views of the geological history of the Miramar coastal region.

About the fossil remains from the barranca, Romero (1918, p. 24) said: "All the evidence relevant to this investigation demonstrates quite well that the bones are not from animals that died in situ, but are instead from skeletons of animals transported great distances, fractured, and dispersed by water. " Then what about the almost complete rear leg and foot of a toxodon found by Carlos Ameghino? It hardly seems likely that flowing water brought together the several bones comprising the leg and foot and deposited them in their natural connection.

In this regard, Romero (1918, p. 24) said: "The discovery of bones more or less complete, and part of one skeleton, signifies that the bones were brought to their final resting place in that condition and not that the animal perished there." In this case, one would have to suppose that a detached rear leg of a toxodon, still covered with flesh was transported by water and somehow deposited in the lower levels at the Miramar site. But all this shows is that the animal died shortly before its leg wound up in the bed where it was found.

Romero implied that both the bones and the bed were recent. But according to Carlos Ameghino, the toxodon bones were from a Pliocene species of toxodon. Furthermore, modern authorities

(Savage and Russell 1983, p. 365) still list the Chapadmalalan at Miramar as a Pliocene formation containing a distinct Pliocene fauna.

Romero (1918, p. 24), however, insisted: "If you find the fossils of distinct epochs in different levels of the barranca, that does not signify a succession of epochs there, because water may have elsewhere eroded very ancient fossilbearing deposits of previous epochs, depositing the older fossils at the base of the barranca. I mention a case demonstrating this fact: the sea brings up fossil molluscs onto the beach, and my daughter found the foot bone of a great edentate, rolled up on the beach by the waves." The Edentata are an order of New World mammals that includes the sloths and armadillos. Romero was trying to build a case that the formations identified as Chapadmalalan and Ensenadan at Miramar were not really ancient, even though they contained fossils characteristic of the Late Pliocene and Middle Pleistocene, respectively. In making his case, Romero attributed remarkable capabilities to the action of sea waves and rivers. If Romero is to be taken at his word, he seems to have been implying that the random movements of water could selectively deposit fossils of certain periods in a definite sequence so as to mimic actual geological formations of those periods.

But Romero's speculative proposal appears incapable of accounting for the arrangement of fossils and sediments in such a way as to reproduce a series of actual geological formations, even in a relatively confined area. And here we are talking about a section of cliff extending for several hundred meters. Significantly, these same formations at Miramar had been extensively studied on several occasions by different professional geologists and paleontologists, none of whom viewed them in the manner suggested by Romero. Modern authorities also disagree with Romero.

In his attack on Carlos Ameghino, Romero sought to demonstrate that the implement-bearing beds of ancient Chapadmalalan loess at Miramar were fairly recent marine deposits. As evidence he cited the particular nature of the rounded stones that marked the boundary between the Chapadmalalan and the overlying Ensenadan. Romero (1918, p. 28) believed that their pattern of distribution, in an almost unbroken band along the entire formation, indicated they were pebbles formed by the action of waves and deposited on a beach. The large undulations now observed in the layer of stones were, according to Romero, caused by the action of later mountain-building forces in the region.

But Bailey Willis, no friend of Tertiary humans in Argentina, had earlier given a different interpretation of the undulating layer of stones in the barranca at Miramar. Willis, who had investigated several other barrancas on the same coast, wrote: "The sections were carefully studied in each locality, but since we require here only an illustration ... it will suffice to describe a characteristic relation observed in the Barrancas del Norte, north of Mar del Plata. . . . The upper surface of the basal formation in the Barrancas del Norte [the Chapadmalalan] is eroded and the hollows are filled by later deposits, sometimes of one character, sometimes of another. It will be seen that the formation was caived by an agent that undercut the sides and rounded the bottoms of the hollows, leaving masses with sharp points or edges in relief. Wind produces these effects in this material, whereas water cuts channels having nearly vertical walls. Thus it would appear that



wind erosion is favored. . . . The phenomenon recurs . . . in other exposures of the formation at Miramar" (Hrdlicka 1912, pp. 22-23). Romero thought the same hollows to be the result of marine action and mountain building rather than wind.

Then regarding the stone layer itself, found in the hollows, Willis wrote: 'The pebbles . . . could have been formed only by wind action, since the loess of which they consist would readily melt down in water and lose its form. The formation thus suggests arid conditions" (Hrdlicka 1912, p. 24). Willis, a member of the U.S. Geological Survey, was an expert in the study of loess formations, having conducted extensive investigations in the course of geological expeditions in North America and China.

Romero's qualifications are unknown to us, and his view that the pebbles are a sign of marine action seems in great disharmony with the geological evidence. The same is true of his assertion that the Chapadmalalan at Miramar is actually a fairly recent marine mud deposited against the base of the cliff. This opinion was based upon visual inspection of a piece of sediment from an excavation at Miramar. Romero (1918, p. 31). stated about this chunk: "It is constituted principally of a mixture of clayey elements and sand, very fine, and is deposited uniformly in layers about 1 mm thick, which indicates successive, slow, tranquil deposition in a bay. Throughout the piece are many holes .25-1.0 mm in diameter, forming small tunnels in the direction of the plane of stratification, and on close observation you can see traces of organic remains of annelids." On the strength of this one piece of sediment, which he did not demonstrate to be typical of the entire deposit, Romero (1918, p. 31) then concluded: "It is obvious the bed was deposited on the sea bottom, and any animal bones or human artifacts found in the beds were brought there by the action of waves or were washed down from the cliff and covered up." Romero neglect to mention that annelids include not only marine worms but ordinary earthworms.

Furthermore, Willis described the formation at the base of the cliffs as follows: "At Miramar . . . the formation . . . consists of loess-like alluvium, the surface of which has been eroded and filled in by wind" (Hrdlicka 1912, p. 27). Describing the Chapadmalalan layer in the Barrancas del Norte, which he regarded as continuous with that at Miramar, Willis wrote: "The writer is inclined to regard this formation and similar deposits as due to river work on confluent flood plains" (Hrdlicka 1912, p. 23). A river flood plain is generally covered with water during only a small part of the year, and not every year. Such conditions are very favorable for fossilization of animal remains in primary position, especially when conditions are becoming more arid. At such times, animal remains may be buried during floods and remain undisturbed for long periods of time because of lower water levels. In short, Willis gave no hint that the deposit at the base of the cliff at the Miramar site was a recent marine formation.

The incorrectness of Romero's interpretation of the stratigraphy at Miramar is confined by modern researchers, who identify the formation at the base of the cliff as Chapadmalalan and assign it to the Late Pliocene, making it 2-3 million years old (Savage and Russell 1983, p. 365).

Considering Romero's farfetched and strained geological reasoning, one would certainly have a right to be cautious in accepting his conclusion about the stratigraphic position of the artifacts in

the barranca at Miramar: "Visual Inspection demonstrates that the artifacts discovered were interred at a time after the formation of the bed, and that they are in a secondary position in relation to the formation, because of an intrusion resulting from erosion at this place" (Romero 1918. p. 27). Here Romero mockingly reproduced the language of the report by the commission of geologists, who concluded the stone implements of Miramar were found in primary position (Section 5.2.1).

Romero, however, did not provide a great deal of evidence in favor of his point of view. In addition to his sea wave hypothesis, Romero suggested that there had been massive resorting and shifting of the beds in the barranca, making it possible that implements and animal bones from surface layers had become mixed into the lower levels of the cliff. But the only facts that he could bring forward to support this conclusion were two extremely minor dislocations of strata.

Some distance to the left of the spot where the commission of geologists extracted a bola stone from the Chapadmalalan level of the barranca, there is a place where a section of a layer of stones in the formation departs slightly from the horizontal (Romero 1918. p. 28). This dislocation occurs near the place where the barranca is interrupted by a transverse valley. As might be expected, part of the barranca slopes down to the left at this point, but at the place where the bola stone was extracted, the horizontal stratigraphy remained intact. At another place in the barranca, a small portion of a layer of stones departed only 16 degrees from the horizontal (Romero 1918. p. 29).

On the basis of these two relatively inconsequential observations, Romero suggested that all the strata exposed in the barranca had been subjected to extreme dislocations. This would have allowed the intrusion into the lower levels of stone tools from relatively recent Indian settlements that might have existed above the cliffs. Romero (1918. p. 30) asserted: "I have demonstrated . . . that the artifacts had been intrusively buried in the strata called Chapadmalalan, and with this demonstration, which is irrefutable, I have also buried the opinion of the experts." But from photographs and descriptions of the stratigraphy by many other geologists, including Willis, it appears that the normal sequence of Pampean beds in the barranca at Miramar was intact in locations where discoveries were made.

Romero then continued with another barrage of unsatisfactory objections. He pointed out that some of the stone tools were found at the base of the Chapadmalalan bed and others in the middle, while the toxodon femur with the embedded projectile point was found at the top of the Chapadmalalan. Romero (1918. p. 33) thought the fact that the artifacts were distributed vertically within a limited horizontal space argued against their being in primary position. He would have preferred to see them all distributed in one horizontal plane. Why he thought this is not clear. If, as Carlos Ameghino believed, the place had been continuously inhabited from the Miocene until the recent past, by humans maintaining a constant level of cultural advancement, then one might expect to find just such a distribution of artifacts as was actually discovered.

Romero noted that stone tools resembling those extracted from the barranca are found on the surface, in a valley, slightly inland, that runs parallel to the barranca. He joked with his guide: "Are

these Miocene implements?" But even today African tribal people use stone tools as crude as the Olduvai Gorge pebble choppers, which are attributed to human precursors living almost 2 million years ago. In other words, humans and humanlike beings in Africa have been making the same kinds of stone tools for at least 2 million years. It is, therefore, not valid to argue that the tools from the Chapadmalalan at Miramar must be recent because they resemble tools made by modern Indians in the same region.

#### 5.2.4 Boule on the Toxodon Femur with

##### Arrowhead

Now that we have considered Romero's objections to Carlos Ameghino's discoveries, let us turn our attention to a rare mid-twentieth-century review of the toxodon femur with the projectile point embedded in it. In the 1957 posthumous edition of *Fossil Men*, revised by H. V. Vallois, Marcellin Boule said that after the original discovery of the toxodon femur, Carlos Ameghino found in the Chapadmalalan at Miramar an intact section of a toxodon's vertebral column, in which two stone projectile points were embedded. Boule stated: "These discoveries were disputed. Reliable geologists affirmed that the objects came from the upper beds, which formed the site of a paradero or ancient Indian settlement, and that they were found today in the Tertiary bed only as a consequence of disturbances and resortings which that bed had suffered" (Boule and Vallois

1957, p. 492). Here Boule footnoted as a reference only the 1918 report by Romero! Boule did not mention the commission of four highly qualified geologists who reached a conclusion exactly opposite that of Romero, perhaps because they were, in his opinion, not "reliable." However, having closely studied Romero's geological conclusions, particularly in light of those of Bailey Willis and modern researchers, we are mystified that Romero should be characterized as "reliable."

Boule added: "The archaeological data support this conclusion, for the same Tertiary bed yielded dressed and polished stones, bolas and boladeras, identical with those used as missiles by the Indians" (Boule and Vallois 1957, p. 492). Boule said that Eric Boman, an "excellent ethnographer," had documented these facts.

Could human beings have lived continuously in Argentina since the Tertiary and not changed their technology? Why not, especially if, as certified by a commission of geologists (Section 5.2.1), implements were found in situ in beds of Pliocene antiquity? The fact that these implements were identical to those used by more recent inhabitants of the same region poses no barrier to acceptance of their Tertiary age. Modern tribal people in various parts of the world fashion stone implements indistinguishable from those recognized as having been manufactured

2 million years ago. We should also point out that in 1921 a fully human fossil Jaw was found in the Chapadmalalan at Miramar (Section 6.2.5).

In his statements about the Miramar finds, Boule provides a classic case of prejudice and preconception masquerading as scientific objectivity. In Boule's book, all evidence for a human

presence in the Tertiary formations of Argentina was dismissed on theoretical grounds and by ignoring crucial observations reported by competent scientists who happened to hold forbidden views. For example, Boule said nothing at all about the above-mentioned discovery of a human Jaw in the Chapadmalalan at Miramar. We should thus be extremely careful in accepting the statements one finds in famous textbooks as the final word in paleoanthropology.

It is common to find scientists who disagree with certain controversial evidence taking the same approach as Boule. One mentions an exceptional discovery, one states that it was disputed for some time, and then one cites an authority (such as Romero) who supposedly conclusively settled the matter, once and for all. But we have found that when one takes the time to dig up the report that, like Romero's, supposedly delivered the coup de grace, it often fails to make a convincing case.

#### 5.2.5 Boman, the Excellent Ethnographer

What was true of Romero's report is also true of Boman's. Boule, we have seen, advertised Boman as an "excellent ethnographer." But in examining Boman's report, the reason for Boule's favorable judgement becomes apparent. Throughout his paper, which attacked Florentino Ameghino's theories and Carlos Ameghino's discoveries at Miramar, Boman, taking the role of a dutiful disciple, regularly cited Boule as an authority. As might be expected, Boman also quoted extensively from Hrdlicka's lengthy negative critique of Florentino Ameghino's work. Nevertheless, Boman, despite his negative attitude, inadvertently managed to give some of the best possible evidence for a human presence in Argentina during the Pliocene.

Boman (1921, p. 336) wrote: "Before November 1913, at which time commenced the discoveries of vestiges of human industry in the Chapadmalalan of Miramar, the theories of F. Ameghino could be considered to have been definitely rejected. But now there was reason to question whether these additional discoveries did not constitute a new proof for the existence of Tertiary man in South America. Carlos Ameghino, brother of Florentino, announced these discoveries in various summary and preliminary reports. These notices were received with ironic skepticism in the few scientific journals that continued publication during the war in Europe."

At that time, Boman (1921, p. 337) wrote a short article reviewing Carlos Ameghino's finds, citing negative assessments by Antonio A. Romero and the Italian anthropologist and geologist Guido Bonarelli, who believed the objects were not found in situ. Boman later stated: "I must observe that at the time I wrote my article, I had not yet visited Miramar and was thus guided by the facts furnished to me by Carlos Ameghino and others who had personally visited the site. I also personally inspected the objects that had been gathered there. "

Boman (1921, p. 337) then carefully yet deliberately raised the possibility of fraud by Lorenzo Parodi, the collector who worked for Carlos Ameghino: "Regarding the intervention of Lorenzo Parodi in the discoveries . . . I had no right to express any suspicions about him, because Carlos Ameghino had spoken highly of him, assuring me that he was as honest and trustworthy a man as could be found." Boman (1921, p. 341) added: "I do not have any personal reason to doubt the

honesty of Parodi, but generally speaking, a person in his condition participates in discoveries of this kind without any scientific interest. Instead, such persons tend to be solely interested in obtaining money and keeping their employment. Therefore, it is not possible to do anything but raise suspicions about fraud. Concerning the question of where it is possible to obtain objects for fraudulent introduction into the Chapadmalalan strata, that is a problem easily resolved. A couple of miles from the discoveries exists a paradero, an abandoned Indian settlement, exposed on the surface and relatively modern—about four or five hundred years old—where there exist many objects identical to those found in the Chapadmalalan strata. ”

Boman (1921, p. 342) went on to describe his own visit to the Miramar site on November 22, 1920: "Parodi had given a report of a stone ball, uncovered by the surf and still encrusted in the barranca. Carlos Ameghino invited various persons to witness its extraction, and I went there along with Dr. Estanislao S. Zeballos, ex-minister of foreign affairs; Dr. H. von Ihering, ex-director of the Museum of Sao Paulo in Brazil; and Dr. R. Lehmann-Nitsche, the well known anthropologist." At the Miramar barranca, Boman (1921, p. 343) convinced himself that the geological information earlier reported by Carlos Ameghino was essentially correct. Boman's admission confirms our assessment that the contrary views of Romero are not to be given much credibility (Section 5.2.3).

"Arriving at the final point of our Journey," wrote Boman (1921, p. 343), "Parodi showed us a stone object encrusted in a perpendicular section of the barranca, where there was a slight concavity, apparently produced by the action of waves. This object presented a visible surface only 2 centimeters just under an inch] in diameter. Parodi proceeded to remove some of the surrounding earth so it could be photographed, and at that time it could be seen that the object was a stone ball with an equatorial groove of the kind found on bola stones. Photographs were taken of the ball in situ, the barranca, and the persons present, and then the bola stone was extracted. It was so firmly situated in the hard earth that it was necessary to use sufficient force with cutting tools in order to break it out little by little."

Boman then confirmed the position of the bola stone (Figure 5.2a), which was found in the barranca about a meter (about 3 feet) above the beach sand. Boman (1921, pp. 343-344) stated: "The barranca consists of Ensenadan above and Chapadmalalan below. The boundary between the two levels is undoubtedly a little confused. ... Be that as it may, it appears to me that there is no doubt that the bola stone was found in the Chapadmalalan layers, which were compact and homogeneous." It bears repeating that this description invalidates the views of Romero (1918), who had sought to demonstrate that the Chapadmalalan formations at Miramar were recent marine deposits. Boman's account therefore also discredits Boule, who relied solely upon Romero in his own attempt to dismiss the discovery at Miramar of the toxodon femur and vertebral column, both with stone arrowheads embedded in them (Section 5.2.4).

Boman (1921, p. 344) then told of another discovery: "Later, at my direction, Parodi continued to attack the barranca with a pick at the same point where the bola stone was discovered, when suddenly and unexpectedly, there appeared a second ball ten centimeters lower than the first. . . It is more like a grinding stone than a bola." This tool (Figure 5.2b) was found at a depth of 10

centimeters (4 inches) in the face of the cliff. Boman (1921, p. 345) said it was "artificially worn." Still later Boman and Parodi discovered another stone ball (Figure 5.2c),

200 meters from the first ones, and about half a meter lower in the barranca (Boman 1921, p. 344). Of this last discovery at Miramar, Boman (1921, p. 346) said "there is no doubt that the ball has been rounded by the hand of man."

Boman then discussed the materials from which the implements had been made. The first bola was of quartzite, which can be found at Mar del Plata, about 15 miles northeast of Miramar. It was more difficult to account for the presence at Miramar of the diabase, from which the other two bolas were made. The nearest place where diabase could be found was near Rio Negro, about 300 miles to the southwest, from where it could have been carried up along the coast. The other possibility was the mountains of the Cordillera, deemed by Boman to be too far away—over 600 miles.

Altogether, the circumstances of discovery greatly favored a Pliocene date for the Miramar bolas. Boman (1921, p. 347) reported: "Dr. Lehmann-Nitsche has said that according to his opinion the stone balls we extracted were found in situ, are contemporary with the Chapadmalalan terrain, and were not introduced at any later time. Dr. von Ihering is less categorical in this regard. Concerning myself, I can declare that I did not observe any sign that indicated a later introduction. The bolas were firmly in place in the very hard terrain that enclosed them, and there was no sign of there having been any disturbance of the earth that covered them."

Boman (1921, p. 347) then artfully raised, as previously, the suspicion of cheating: "I have exchanged opinions with various colleagues about the possibility there could have been any

kind of fraud involved in the circumstances under consideration, and we came to the conclusion that this possibility cannot be completely excluded. One could drill in the barranca a hole of the required size, introduce the object, and then carefully cover it with some dampened earth, the same removed in making the hole.

Figure 5.2. These stone bolas were extracted from the Late Pliocene Chapadmalalan formation at Miramar, Argentina. in the presence of ethnographer Eric Boman (1924, p. 345).

It could then be left to the waves, which periodically strike the barranca, to smooth and harden the earth, in such a manner that after a few months or a year it would appear as if nothing had touched the barranca, It would be interesting to verify this experimentally. ”

Boman (1921, pp, 347-348) then went on to cast doubt on another discovery made at Miramar: “In the Museo de La Plata, I have made an experiment of a similar nature, relative to a specimen discovered in the Chapadmalalan of Miramar—the femur of toxodon which has embedded in its trochanter the point of an arrow made of quartzite. I searched the museum collection for a toxodon femur of the same size and state of fossilization, and drove a similar quartzite point into the corresponding region of the bone, C. Heredia, then secretary of the museum, who studied this piece for a long time on his desk, said he could not distinguish it from the original” However, Boman (1921, p, 348) himself admitted: “But this experiment does not demonstrate anything other than the possibility of an exact imitation, and is not a conclusive proof that the point of the arrowhead was introduced into the femur of Miramar when it was already in a fossil state. ”

Boman (1921, p, 348) added: "Concerning the question of the authenticity of the finds from the Chapadmalalan strata at Miramar, in the final analysis there undoubtedly exists no conclusive proof of fraud, On the contrary many of the circumstances speak strongly in favor of their authenticity,”

Despite this remarkable admission, Boman (1921, p, 348) could not resist once more raising the possibility of fraud or incompetence: "Nevertheless, the manner in which the discoveries were made, and above all, the continuous involvement of a person such as Parodi, necessarily give rise to suspicions, I do not believe that there is anyone in the world of science who could accept without the most careful consideration the above-mentioned discoveries as authentic proofs of nothing less than the existence of humans in South America during the Tertiary epoch,"

Boman (1921, pp, 348-349) then wrote: "In North America many analogous discoveries have been unanimously and definitively rejected because they were made by illiterate workers, miners, or prospectors of various kinds, Modern science requires stringent scientific control of the facts that serve as the foundations of its conclusions, It does not admit the affirmations and stories of ordinary persons, and stories in newspapers convince no one.” Here Boman footnoted a negative report by Holmes on the auriferous gravel finds in California, We shall consider the California discoveries in some detail later in this chapter (Section 5.5), but for now we shall simply forewarn the reader that Holmes's dismissals are themselves open to question,

It is difficult to see why Boman should have been so skeptical of Parodi.

One could argue that Parodi would not have wanted to jeopardize his secure and longstanding employment as a museum collector by manufacturing fake discoveries, In any case, the museum professionals insisted that Parodi leave any objects of human industry in place so they could be photographed, examined, and removed by experts. This procedure is superior to that employed by scientists involved in many famous discoveries that are used to uphold the currently accepted scenario of human evolution, For example, most of the *Homo erectus* discoveries reported by von

Koenigswald in Java were made by native diggers, who, unlike Parodi, did not leave the fossils in situ but sent them in crates to von Koenigswald, who often stayed in places far from the sites, Also, many fossil hominid discoveries in Africa were made in a manner similar to that employed at Miramar—native diggers uncovered fossils or stone tools and left them in place to be examined by professional scientists, It may further be noted that the famous Venus of Willendorf, a Neolithic statuette from Europe, was discovered by a road workman, It is obvious that if one were to apply Boman's extreme skepticism across the board one could raise suspicions of fraud about almost every paleoanthropological discovery ever made. Boman himself recognized this,

Boman admitted that his principal reasons for not accepting the Miramar discoveries were theoretical, Boman (1921, pp. 349-350) wrote: "If one were able to prove in an evident manner the authenticity of the discoveries in the Chapadmalalan of Miramar and the Tertiary age of these strata, this would provide proof not only of the existence of Tertiary humans in South America but also of a thing very strange—the identity of their artifacts with those of the modern Indians, Can anyone imagine that Miocene humans [Pliocene according to modern estimation] made polished bola stones with grooves around the middle? In response to this question, I can do nothing but repeat the point I made at the end of my last publication on Miramar, which has also been reproduced by Boule in his book on fossil man: 'The principal difficulty in accepting a Tertiary age for the objects we have finished enumerating consists in that without exception all the objects unearthed from the Chapadmalalan at Miramar are absolutely similar to like objects found in all parts of the surface and uppermost strata of the Pampas and Patagonia, Is It possible that man could have lived in the Pampas from the Miocene to the time of the Spanish conquest, without changing his customs and without perfecting his primitive industry in some fashion?'" But why not? As previously mentioned, scientists in Africa have found that modern tribal people make crude stone tools almost identical to those recovered from geological contexts 2 million or more years old, in the same localities,

Ironically, Boman's testimony provides, even for skeptics, very strong evidence for the presence of toolmaking human beings in Argentina as much as 3 million years ago, Even if, for the sake of argument, one admits that the first bola stone recovered during Boman's visit to Miramar was planted by the collector Parodi, how can one explain the second and third finds? These were instigated not by the collector Parodi but by Boman himself, on the spot and without any warning, Significantly, they were completely hidden from view, and Parodi did not even hint at their existence.

Altogether, it appears that Boule (Section 5.2.4), Romero (Section 5.2.3), and Boman did very little to discredit the discoveries of Carlos Ameghino and others at the Miramar site. In fact, Boman gave first-class evidence for the existence of bola makers there in the Pliocene period.

### 5.3 Other Bolas and Bolalike Implements

The bolas of Miramar are significant in that they point to the existence of human beings of a high level of culture during the Pliocene, and perhaps even earlier, in South America. Similar implements have been found in Africa and Europe in formations of similar Pliocene age. This



refutes the suggestion that the bolas discovered in the Pliocene Chapadmalalan of Miramar must be recent because of their resemblance to modern Argentine Indian bolas.

Bolas have also been found in Middle Pleistocene formations. In North America, bolas have been recovered from the Calico site, dated at about 200,000 years (Minshall 1989, p. 110). Bolas have also been found in China at the Gehe site, dated at about 600,000 years (Minshall 1989, p. 38), and at the similarly ancient Lantien site (Minshall 1989, p. 40).

Taken together, these round projectile stones, found in widely distant parts of the world in Pleistocene, Pliocene, and perhaps earlier geological contexts powerfully challenge the currently accepted notions of human origins and antiquity. In particular, the Pliocene discoveries of bolas strongly contradict the idea that 2-3 million years ago only very primitive protohuman hominids were living, and these only in Africa. The use of bola stones requires complex behavior generally associated with *Homo sapiens sapiens*. Let us now give some detailed attention to two significant cases of bolas and bolalike implements—the Bramford sling stone and the bolas of Olduvai Gorge.

### 5.3.1 The Sling Stone from Bramford, England (Pliocene to Eocene)

In 1926, one of J. Reid Moir's assistants uncovered a particularly interesting object from below the Pliocene Red Crag. Moir had been conducting excavations in the Red Crag and detritus bed below the Red Crag, which were exposed in a brick-earth pit on the north bank of the River Gipping at Bramford, near Ipswich. Moir (1929, p. 63) wrote: "The beds surmounting the loamy sand at Pit No. 2, Bramford, do not exhibit signs of glacial disturbance such as might have ploughed into the detritus-bed, and rearranged it with later material. The conclusion, therefore, must be that the object now to be described which was removed from the detritus-bed by my trained excavator, John Baxter, formed an integral part of that deposit."

Moir recalled that Baxter once gave him a small oval object that did not seem to warrant close inspection. Three years later, however, the round stone object (Figure 5.3) was noticed by Henri Breuil: "While I was staying in Ipswich with my friend J. Reid Moir, we were examining together a drawer of objects from the base of the Red Crag at Bramford, when J. Reid Moir showed me a singular eggshaped object, which had been picked up on account of its unusual shape. Even at first sight it appeared to me to present artificial striations and facets, and I therefore examined it more closely with a mineralogist's lens [Figure 5.4]. This examination showed me that my first impression was fully justified, and that the object had been shaped by the hand of man. . . . The whole surface . . . has been scraped with a flint, in such a way that it is covered with a series of facets running fairly regularly from end to end. . . . The scraping described above covers the whole surface of the object, and penetrates into its irregularities. As it stands, the object is entirely artificial, and, although somewhat smaller, it recalls the steatite sling stones of New Caledonia" (Moir 1929, p. 63). According to Moir (1929, p. 64), several other archeologists had confirmed Breuil's hypotheses. Moir, who believed the object had been shaped when soft, performed experiments with clay and flint, and he obtained results that were very much the same.

Figure 5.3. A sling stone from the detritus bed beneath the Red Crag at Bramford. England (Moir 1929. p. 64). At least Pliocene in age, the sling stone could be as old as the Eocene.

Figure 5.4. A drawing showing marks of intentional shaping on the sling stone from the detritus bed beneath the Red Crag at Bramford. England (Moir 1929. p. 65).

Moir (1929, p. 65) wrote: "it becomes clear that the presence of this object at such an horizon . . . points to the fact that man of the Pliocene period had already progressed some distance upon the evolutionary path, as it seems impossible to imagine any ape-like creature producing artifacts such as have now been found in the detritus bed," Sling stones or bola stones represent a level of technological sophistication universally associated with modern *Homo sapiens*, It may be recalled that the detritus bed below the Red Crag contains fossils and sediments from habitable land surfaces ranging from Pliocene to Eocene in age, Therefore the Bramford sling stone could be anywhere from 2 to 55 million years old,

It is altogether remarkable that almost without exception scientists have ignored the Bramford sling stone. It was found by a trained excavator, reported by a reputable archeologist, and examined by many experts including the famous Professor Breuil of the Institute of Human Paleontology in France, Some might object that it was found by a hired digger and not immediately noticed, But many of the Java *Homo erectus* fossils reported by von Koenigswald, which now figure prominently in every textbook of general paleoanthropology, were uncovered by native collectors, This is also true of the Petralona skull, found by Greek peasants in a cave, These cases, and many others like them, will be discussed in coming chapters, If these finds are accepted, despite the questionable circumstances of their discovery, then the Bramford sling stone deserves equal treatment. Otherwise we have another good example of scientists applying a double standard in the

treatment of anomalous evidence.

### 5.3.2 Bolas from Olduvai Gorge (Early Pleistocene)

In 1956, G. H. R. von Koenigswald described some human artifacts that were discovered in the lower levels of the Olduvai Gorge site in Tanzania, Africa. "Apart from these archaic handaxes," wrote von Koenigswald (1956, p. 170), "the same levels have yielded numbers of stones that have been chipped until they were roughly spherical. . . . These stones are enormously widespread in Africa, occurring both in the north and south: indeed, at Ain Hanech, east of Algiers, they are the only signs of human culture that have been found there in association with fossil remains of elephant and giraffe. They are believed to be an extremely primitive form of throwing ball. Stone balls of this type, known to them as bolas, are still used by native hunters in South America. They are tied in little leather bags and two or three of them are attached to a long cord. Holding one ball in his hand, the hunter whirls the other one or two around his head and then lets fly." An early hominid might have had enough intelligence to use bolas, but only *Homo sapiens sapiens* is actually known to have used them. Bolas are not unequivocally associated with the fossil remains of any other hominid. The objects reported by von Koenigswald, if used in the same manner as South American bolas, imply that their makers were adept not only at stoneworking but leatherworking as well.

All this becomes problematic, however, when one considers that Bed I at Olduvai, where stone balls were found, is 1.7-2.0 million years old. According to standard views on human evolution, only *Australopithecus* and *Homo habilis* should have been around at that time. At present, there is not any definite evidence that *Australopithecus* used tools, and *Homo habilis* is not generally thought to have been capable of employing a technology as sophisticated as that represented by bola stones, if that is what the objects really are. Some scientists doubt that *Homo habilis* was a toolmaker at all, and want to attribute tools found in the same level as *Homo habilis* to early representatives of *Homo erectus*.

Once more we find ourselves confronted with a situation that calls for an obvious, but forbidden, suggestion—perhaps there were creatures of modern human capability at Olduvai during the earliest Pleistocene. After all, the present inhabitants of the same region, as well as people in other parts of the world, make and use tools like the pebble choppers found in Bed I of Olduvai Gorge. Any crude stone tool now attributed to *Homo habilis* or *Homo erectus* could, therefore, also be attributed to *Homo sapiens*.

Those who find this suggestion incredible will doubtlessly respond that there is no fossil evidence to support such a conclusion. In terms of evidence currently accepted, that is certainly true. But if we widen our horizons somewhat, we encounter Reck's skeleton, fully human, recovered from upper Bed II, right at Olduvai Gorge (Section 11.1). And not far away, at Kanam, Louis Leakey, according to a commission of scientists, discovered a fully human jaw in Early Pleistocene sediments, equivalent in age to Bed I (Section 11.2.3). In more recent times, humanlike femurs

have been discovered in East Africa. in Early Pleistocene contexts (Section 11.6.3). These isolated femurs were originally attributed to *Homo habilis*, but the subsequent discovery of a relatively complete skeleton of a *Homo habilis* individual has shown the *Homo habilis* anatomy. including the femur. to be somewhat apelike. This opens the possibility that the humanlike femurs once attributed to *Homo habilis* might have belonged to anatomically modern human beings living in East Africa during the Early Pleistocene (Section 11.7.1). If we expand the range of our search to other parts of the world, we can multiply the number of examples of fully human fossil remains from the Early Pleistocene and earlier. In this context, the bola stones of Olduvai do not seem out of place.

But perhaps the objects are not bolas. To this possibility Mary Leakey (1971, p. 262) replied: 'Although there is no direct evidence that spheroids were used as bolas. no alternative explanation has yet been put forward to account for the numbers of these tools and for the fact that many have been carefully and accurately shaped. If they were intended to be used merely as missiles, with little chance of recovery. it seems unlikely that so much time and care would have been spent on their manufacture.' Mary Leakey (1971, p. 266) added: "Their use as bola stones has been strongly supported by L. S. B. Leakey and may well be correct."

It should also be noted that Louis Leakey (1960a, p. 1051) claimed to have found "agenuine bone tool" in the same level as the bola stones. Leakey (1960a, p 1051) said, 'This would appear to be some sort of a 'lissoir' for working leather. It postulates a more evolved way of life for the makers of the Oldowan culture than most of us would have expected."

The complex behavior required for making and using bolas seems clearly out of character for either *Australopithecus* or *Homo habilis*, both of which were quite apelike. As far as *Homo erectus* is concerned. this creature is not generally portrayed using bolas. If use of bolas were to be attributed to *Homo erectus*. this would require a substantial redefinition of his technological capabilities. It thus appears that the bola stones may point to the existence in the African Early Pleistocene of a being with the intellectual and physical abilities of *Homo sapiens*. This, of course, would do severe damage to the whole picture of human evolution. The sling stone discovered below the Red Crag, with its possible age of 2.5 million years. could also be damaging to the evolutionary hypothesis.

#### 5.4 Relatively Advanced North American Paleolithic Finds

We shall now examine some relatively advanced anomalous Paleolithic implements from North America. beginning with those found at Sheguiandah, Canada. on Manitoulin Island in northern Lake Huron. Many of these North American discoveries are not particularly old, but they are nonetheless significant because they give insight into the inner workings of archeology and paleoanthropology. We have already seen how the scientific community suppresses data with uncomfortable implications for the currently dominant picture of human evolution. And now we shall encounter revelations of another aspect of this—the personal distress and bitterness experienced by scientists unfortunate enough to make anomalous discoveries.

#### 5.4.1 Sheguiandah: Archeology as a Vendetta

The excavations at Sheguiandah were carried out between 1951 and 1955 by Thomas E. Lee, an anthropologist at the National Museum of Canada. The upper layers of the site contained, at a depth of approximately 6 inches (Level II), a variety of projectile points (Figure 5.5).

Figure 5.5. Projectile point from Level III of the Sheguiandah site, Manitoulin Island, Ontario, Canada (T. E. Lee 1983, p. 61).

According to Lee, excavation exposed an implement-bearing layer of unsorted sediments, apparently a glacial till. Ordinary sediments deposited by water tend to be sorted into distinct layers of sand and gravel.

Deposits laid down by receding glaciers are generally not sorted in this fashion. Since at Sheguiandah stone tools were found in an unsorted till, the implication was that human beings had lived in the area during or before the time of the last glaciation. Further study showed that there was a second layer of till, which also contained artifacts.

Among the stone implements found in the upper section of glacial till, Level IV, were several large, thin, bifacial implements (Figure 5.6). T. E. Lee (1983, pp. 64-65) said about the bifaces: "Many retain some portion of a large bulb of percussion at one end. . . . Secondary chipping is prominent. . . . An interesting feature of several bifaces is the curious shoulder produced at one end. . . . Some of the double-shouldered tools show unmistakable evidence of use as scrapers, presumably hafted." In addition, Lee (1983, p. 65) stated: "A few cutting and scraping tools have been found in Level IV. Two examples show fine cutting edges resulting from removal of small flakes from both sides of one edge."

Figure 5.6. Bifacially chipped implement from upper glacial till (Level IV) at the Sheguiandah site (T. E. Lee 1983, p. 64).

Figure 5.7. Quartzite bifaces from the lower glacial till (Level V) at Sheguiandah (T. E. Lee 1983, p. 66).

Geologist John Sanford (1971) argued these tools and the one in Figure 5.6, were at least 65,000 years old.

Stone implements were also discovered in the layers beneath the tills. The layer immediately below the lower till, a meltwater deposit, covered a pavement of boulders. In and just beneath the boulder pavement were discovered one notched biface and several scrapers. Below the boulder pavement were silty stratified clays, with some cobblestones and boulders. From the upper part of the stratified clays, apparently deposited in a lake, came a broken bifacial implement and several stone flakes apparently struck by human beings (T. E. Lee 1983, p. 49).

How old were the tools? In his first reports, Lee was indefinite. Yet it seemed to him that some Sheguiandah artifacts were older than standard views about the peopling of the New World would allow. Lee (1972, p. 30) stated: "It is impossible to set a maximum age with certainty. . . . Of the four geologists most closely concerned—Dr. John Sanford of Wayne [State] University, Dr. Bruce Liberty and Dr. Jean Terasmae, both formerly of the G.S.C. [Geological Survey of Canada], and Dr. Ernst Antevs of Arizona— all but Dr. Antevs suggested that the site might extend back to interglacial times. Opinions differed as to whether that was 30,000 or 100,000 years ago. Dr. Antevs favored an interstadial for the appearance of man . . . estimated by him at 30,000 years ago. On his advice the group, in close communication, made public their conclusion: 'a minimum of 30,000 years.'" In another paper, Lee (1981) said some of the geologists had suggested that the implements were perhaps 150,000 years old.

From this point on the story becomes murky. Lee's discovery was obviously controversial, pointing to a human presence in North America far earlier than most scientists thought possible. John Sanford nevertheless continued to support Lee's position. He provided geological evidence and arguments suggesting the Sheguiandah site was quite old. But the view advocated by Lee and Sanford did not receive serious consideration from other scientists. Instead, political maneuvers and

ridicule were employed to discredit Lee.

Sanford (1971) gave strong arguments for an early Wisconsin or Sangamon interglacial date for the tools in and below the tills at Sheguiandah. The reasoning he used was somewhat complex, reflecting the intricate series of Wisconsin glacial events at the site.

The Wisconsin, the final North American glacial age, is divided into three periods—early, middle, and late (or main). The entire Wisconsin glaciation was preceded by the Sangamon interglacial.

The geology of the Pleistocene glacial episodes is undergoing constant revision. In fact, some experts would favor scrapping the traditional system of four principal glaciations (the Gunz, Mindel, Riss, and Wurm of Europe and their North American equivalents) for a system of alternating warm and cold periods of shorter duration. This system is said to more accurately reflect the evidence obtained from oxygen isotope studies of ocean core samples (Evans 1971). Even so, most authorities continue to make use of the traditional nomenclature, and we have chosen to do the same.

The early Wisconsin was dominated by glacial advances from centers north of the Great Lakes down into Ohio, Indiana, and other states. In the eastern Great Lakes region, the ice front was divided into three principal lobes (the Huron, Erie, and Ontario Lobes), which tended to advance and retreat together. The Huron Lobe is the one that covered Sheguiandah, located on Manitoulin Island in northern Lake Huron.

The middle Wisconsin was a period of significant glacial retreats, which took place during interstadials, or warm periods. The interstadial retreats were interrupted by some partial readvances.

During the late Wisconsin, the glaciers again advanced, this time to their maximum extent, after which they finally retreated, leaving the present Great Lakes.

According to Sanford, the presence of tools in the tills indicated that Manitoulin Island must have been habitable (not covered by ice or water) at certain periods. During these times, people quarried stone and made tools. After these periods of habitation, glacial advances mixed the tools lying on the ground with stone and earth. When the glaciers retreated this material was deposited as till. So the most important problem facing Sanford was to identify the times when it was possible for toolmakers to have lived in the vicinity of Sheguiandah and the times that glaciers subsequently advanced over the habitation sites.

Supporters of the dominant view about the peopling of the New World would want the habitation dates to be as recent as possible. This is because they believe that a human presence in the New World does not go much further back than 12,000 years. It should be kept in mind, however, that such a recent period of habitation at Sheguiandah must have been followed by a glacial advance and retreat; otherwise, one would not find tools in glacial till.

Was there in fact such a situation within the past 12,000 or so years at Sheguiandah—a period when Manitoulin Island was habitable followed by a period of glacial advance and retreat? In the 1950s, when the site was discovered, it was thought there were two relatively recent glacial advances and retreats that might have reached Sheguiandah—the Cochrane advance, at maybe 8,000 years ago (Nilsson 1983, p. 390), and the Valdres advance, at around 11,000 years ago (Dreimanis and Goldthwait 1973, p. 81). These advances were thought to have taken place after

the main Wisconsin ice sheet retreated north of Manitoulin Island during the final part of the late Wisconsin. One might therefore propose that the tools found in the glacial till were manufactured in a warm period before the Cochrane advance or before the Valders advance. The Two Creeks interstadial has been mentioned.

But current geological opinion argues against this. First of all, during the Two Creeks interstadial, Sheguiandah appears to have been under ice (Hough 1958, p. 288). And when the ice finally retreated, it apparently did not come back (and deposit till). Also, recent authorities do not find evidence for either the Cochrane or Valders advances in the Lake Huron region (Dreimanis and Goldthwait 1973, pp. 71-72, 95-96; Nilsson 1983, p. 390). According to this view, around 11,000 or

12,000 years ago, the retreating Wisconsin ice sheet passed north of the region now occupied by Lake Huron, apparently without advancing again (Dreimanis and Goldthwait 1973, pp. 95-96). Furthermore, as the ice passed north of the present Lake Huron basin, it appears that Manitoulin Island remained under a body of water called Lake Algonquin. Lake Algonquin was a proglacial lake, one that forms at the front of an advancing or retreating glacier. "The position of the ice front is speculative," said Sanford (1971, p. 12). "However, if the map presented by Hough (1958, fig. 62, p. 288) can be considered as summarizing the opinion of geologists, and I believe that it can, Sheguiandah would have been covered by ice during the Two Creeks interval. On the other hand, let us suppose the ice had melted sufficiently so that the front was farther north: and supposing that the area would have been habitable so far as climate is concerned, even though the ice front was not very far away. What are the chances people could have lived on the island? They are extremely slight, because of the probability that the island would have been well covered by water."

Thus far in our review we have found no situation within the past 12,000 years that would account for stone tools in glacial till at Sheguiandah on Manitoulin Island in northern Lake Huron. What about in earlier late Wisconsin times? Sanford (1971, p. 3) stated: "We do not know whether there was an earlier late-Wisconsin interstadial during which the site was uncovered and suitable for occupancy. The literature is commonly indefinite on this point, but the series of charts by Hough (1958, figs. 53-75) appear to summarize general opinion quite well. The literature indicates that the site was covered by either ice or water throughout late Wisconsin time until the lowering of Lake Algonquin."

Sanford's judgement is confirmed in a later study by two experts on the Wisconsin glaciation—A. Dreimanis of the University of Western Ontario and R. P. Goldthwait of Ohio State University. In a report titled "Wisconsin Glaciation in the Huron, Erie, and Ontario Lobes/" published by the Geological Society of America, Dreimanis and Goldthwait (1973) provided a chart showing the changing position of the ice front during the entire Wisconsin glaciation (our Table 5.2, Figure 5.8). According to Dreimanis and Goldthwait (1973, p. 81), the ice front depicted on the chart "represents the advances and retreats of the Manitoulin Island, in northern Lake Huron, is at the same latitude as the middle part of the St. Lawrence Lowlands. As can be seen from this table (after Dreimanis and Goldthwait 1973, p. 81), the ice front was well south of this region during the



entire Wisconsin glaciation, except for the St. Pierre interstadial at 65,000 -70,000 years b.p. Tools found in glacial till at Sheguiandah, on Manitoulin Island, were probably made during this period or during the preceding Sangamon interglacial (Sanford 1971).

TABLE 5.2

Position of the Ice Front During the Wisconsin Glaciation

Lake Huron

Lake

Erie

Indiana

Figure 5.8. The Great Lakes region, showing Manitoulin Island, where the Sheguiandah site is located.

Ontario-Erie Lobe, including also participation of the Georgian Bay and Huron Lobes.” The latter two lobes are the ones that covered Manitoulin Island and the Sheguiandah site. From the chart and discussion supplied by Dreimanis and Goldthwait, it can be concluded that Manitoulin Island was under ice during the entire period from about 10,000 years ago back to the time of the St. Pierre interstadial, which ended about 65,000 years ago in the early Wisconsin.

Prior to the St. Pierre interstadial came the first Wisconsin ice advance, but Dreimanis and Goldthwait (1973, p. 81) said "there is no evidence that this first advance of the ice sheet reached the Great Lakes." Thus the Sheguiandah site was habitable during and before the St. Pierre Interstadial.

This brings us to the Sangamon interglacial period, which preceded the Wisconsin glaciation. According to some, the Sangamon Interglacial extended from 75,000 to 100,000 years ago, while others say it extended from 75,000 to 125,000 years ago. The latter conclusion is based on the fact that the Gulf of Mexico provides fossil and other evidence for a warm climate during that period (Nilsson 1983, p. 455). So the most likely period for the manufacture of the stone tools found at Sheguiandah extends from the St. Pierre interstadial back through the Sangamon interglacial, perhaps as far back as 125,000 years ago.

Therefore, according to the view outlined by Sanford, we can envision the following series of events to account for the artifact-bearing geological formations observed at Sheguiandah. During the Sangamon Interglacial or the earliest part of Wisconsin time, humans manufactured tools on and north of Manitoulin Island. As the ice sheet advanced after the St. Pierre interstadial, a proglacial lake formed in front of the glaciers. This proglacial lake, which covered the Sheguiandah site, deposited sediments. Perhaps a few tools discarded by humans traveling over the lake were incorporated into the sediments.

As the glacier advanced further toward Sheguiandah, it picked up tools, rocks, and earth. By ice rafting, some of these tools and rocks were floated a short distance into the proglacial lake and dropped, settling on top of the lacustrine sediments, which Lee found at the lowest levels of his excavations. Sanford (1971, p. 6) stated: "In consideration of the character of the overlying strata it would be difficult to explain these beds chronologically other than as an early Wisconsin, proglacial lake deposit."

As the ice approached, the early Wisconsin proglacial lake at Sheguiandah disappeared. Meltwater from the ice front created the glacio-fluvial layers of stone and clay over the lacustrine deposits. Sanford (1971, p. 6) stated: "The boulder pavement at the base of this unit probably represents a time of erosion during which the finer materials were removed."

Implements were found by Lee among and below the boulders in the pavement. Sanford (1971, p. 6) stated: "The presence of a few artifacts under boulders in the pavement is difficult to explain except by the same mechanism that provided for them in the still higher tills. They were picked up

by advancing ice from a cultural site that existed prior to this glacial advance, and therefore were available for incorporation in both the outwash materials and the till deposited by the melting ice. The stratigraphic relations indicate an early Wisconsin age for this horizon. Erosion responsible for the boulder bed may have taken place during an early Wisconsin interstadial or even during a very minor recession of the ice front during a time of increasing glaciation."

Meltwater from the glacier deposited over the boulder pavement more glacio-fluvial materials, which Sanford (1971, p. 6) characterized as "outwash materials from the front of the advancing ice." Sanford stated (1971, p. 12): "The glacio-fluvial materials underlying tills at the habitation area certainly must be as old as early Wisconsin in age."

Finally, the glaciers themselves advanced over the site and then retreated, leaving the lower layer of till, which contains stone tools. Then the region was again briefly inhabited by a group of humans who made a different kind of stone tool. The early Wisconsin glaciers advanced once more, and when they finally receded tens of thousands of years later, in late Wisconsin times, they left the upper till—along with the artifacts they picked up in the early Wisconsin.

Sanford (1971, p. 14) stated: "the artifacts in the lower till layer, and probably those in the uppermost, certainly date from early rather than late Wisconsin time. ... A Sangamon age for the earliest artifacts at Sheguiandah would appear more logical than an early Wisconsin date, at this latitude."

After the final glacial retreat, the site was covered by Lake Algonquin, which receded about 9,000 years ago, leaving Manitoulin Island as we now know it. Indians inhabited the island and left stone tools, including projectile points, now found in the surface layers above the glacial tills.

It is interesting to note that Sanford, unlike Lee, believed that the projectile point horizon (Level II) was also glacial in origin. According to Sanford (1983, p. 83): "It seems reasonably certain that the approximately six inches of surface material overlying the projectile point horizon was originally deposited as till. ... [its] general character shows a similarity to the underlying tills and indicates a common genesis. The artifacts show it to be a definite stratigraphic unit." If Sanford's view is accepted, then the very advanced projectile points (Figures 5.5, 5.9) should, like the tools in the layers below them, also be early Wisconsin or Sangamon in age.

Figure 5.9. Quartzite projectile point recovered from Level III of the Sheguiandah site, Manitoulin Island, Ontario, Canada (T. E. Lee 1983, p. 62).

So it seems there is very good evidence for the presence of toolmaking humans at Sheguiandah at least as far back as the St. Pierre interstadial, which ended 65,000 years ago. The implements could possibly have been manufactured during the Sangamon interglacial, which means they could be as much as 125,000 years old. The presence of relatively advanced stone tools in the St. Pierre interstadial or Sangamon interglacial of North America would, according to currently dominant views, be quite unexpected.

#### 5.4.1.2 How Lee Was Treated

We shall now present Thomas E. Lee's account of how his discoveries were received. Although this history will not be found in standard archeological publications, it is worth careful study. Lee's experiences shed light on how the scientific process works in practice. We shall leave it to the reader to decide whether or not his complaints are justified.

Lee (1966a, pp. 18 -19) recalled: "Several prominent geologists who examined the numerous excavations in progress during four years at Sheguiandah privately expressed the belief that the lower levels of the Sheguiandah site are interglacial. Such was the climate in professional circles—one of Jealousy, hostility, skepticism, antagonism, obstructionism, and persecution—that, on the advice of the famed authority, Dr. Ernst Antevs of Arizona, a lesser date of '30,000 years minimum' was advanced in print by some of the geologists to avoid ridicule and to gain partial acceptance from the more serious scholars. But even that minimum was too much for the protagonists of the 'fluted-point-first-Americans' myth. The site's discoverer [Lee] was hounded from his Civil Service position into prolonged unemployment; publication outlets were cut off: the evidence was misrepresented by several prominent authors among the Brahmins: the tons of artifacts vanished into storage bins of the National Museum of Canada: for refusing to fire the discoverer, the Director of the National Museum [Dr. Jacques Rousseau], who had proposed having a monograph on the site published, was himself fired and driven into exile: official positions of prestige and power were exercised in an effort to gain control over Just six Sheguiandah specimens that had not gone under cover: and the site has been turned into a tourist resort. All of this, without the profession, in four long years, bothering to take a look, when there was still time to look. Sheguiandah would have forced embarrassing admissions that the Brahmins did not know everything. It would have forced the re-writing of almost every book in the business. It had to be killed. It was killed."

Lee's account was supported by Dr. Carl B. Compton, who wrote in *The Interamencan* Oanuary 1966, p. 8): "When Thomas E. Lee found artifactual material in glacial till at Sheguiandah some years ago and when the age was estimated by several well-known and respected geologists at more than 30,000, the Brahmins presented their well-known 'Berlin Wall' to 'contain' this heresy" (T. E. Lee 1966b). Compton obviously thought that Lee was the victim of a power play in a scholarly community divided into hostile factions.

Here are additional comments by T. E. Lee (1964, p. 24) on his fate: "I, even as a professional archaeologist and officer of the National Museum of Canada over most of a nine-year period, found my work subjected to discrimination, whisper campaigns, and behind-the-scenes throat-

cutting of the most contemptible and despicable order.” Eventually, Lee had no choice except to resign from his position at the museum. He recalled: “My own resignation was in protest against the activities of R. S. MacNeish and was forced upon me by an impossible ultimatum delivered to me by that same Director of Natural History” (T. E. Lee 1964, p. 28).

Lee experienced great difficulty in getting his reports on his discoveries at Sheguiandah published through the National Museum of Canada. He wrote: “By depriving me of all essential services, burdening me with routine cataloguing, and closing publication outlets to me, every effort was made in the National Museum of Canada and in its string of satellites to block such publication. ... I was hounded from my Canadian government position by certain American citizens on both sides of the border and driven into eight long years of blacklisting and enforced unemployment” (T. E. Lee 1974, p. 23). He also said that papers written by him were “filed away and lost” in the museum (T. E. Lee 1964, p. 24).

Having failed to get his reports into print in government publications, Lee, as a private citizen, experienced similar difficulties with standard scientific Journals. Expressing his frustration, he wrote: “A nervous or timid editor, his senses acutely attuned to the smell of danger to position, security, reputation, or censure, submits copies of a suspect paper to one or two advisors whom he considers well placed to pass safe Judgement. They read it, or perhaps only skim through it looking for a few choice phrases that can be challenged or used against the author (their opinions were formed long in advance, on the basis of what came over the grapevine or was picked up in the smoke-filled back rooms at conferences— little bits of gossip that would tell them that the writer was far-out, a maverick, or an untouchable). Then, with a few cutting, unchallenged, and entirely unsupported statements, they ‘ kill’ the paper. The beauty—and the viciousness—of the system lies in the fact that they remain forever anonymous. The author may damn and fume and even correctly guess at their identity—but he is helpless. When in the course of time he is dead and safely buried—and proven to have been right—he will either be ignored or said to have been right, but for the wrong reasons” (T. E. Lee 1977, p. 2).

Most of the key reports about Sheguiandah were published in the *^diropological Journal ofCanada*, which Lee himself founded and edited. Lee died in 1982, and the Journal was then edited for a short time by his son, Robert E. Lee.

After Lee left the National Museum of Canada, he eventually obtained a teaching position at Laval University. In 1980, he received for review a book (*Initiation a l'archeologie*) by Rene Levesque, a former student. Levesque had written on the title page: “I hope not to lose your friendship with my book. But the eternal enemies are still on the war path. I am honored to be with you in that fight” (T. E. Lee 1981, p. 18). Somewhat puzzled, Lee paged through the book. It contained a list of important archeological sites in North America, but the list did not include Sheguiandah. Nor was there any discussion of Sheguiandah in the text. Lee (1981, p. 19) found this strange, because Levesque, his student, knew well “the inescapable proofs that put Sheguiandah back in the 150,000-year range, as determined by a number of geologists, both Canadian and American.” Lee noticed, however, a very complete list of his works on Sheguiandah In the bibliography In the back

of the book. "It should be clear by now," wrote Lee (1981, p. 19), "to everyone who reads this in the far corners of the earth, that a hatchet job was done . . . to eliminate Sheguiandah from the text, with the Bibliographie being overlooked, either in haste or arrogance."

Of course, it has not been possible for establishment scientists to completely avoid mentioning Sheguiandah, but when they do, they tend to downplay, ignore, or misrepresent any evidence for an unusually great age for the site.

Lee's son Robert wrote: "Sheguiandah is erroneously explained to students as an example of postglacial mudflow rather than Wisconsin glacial till; the reports, they are told, are too badly written to be worth reading, if indeed their existence is acknowledged" (R. E. Lee 1983, p. 11).

The original reports are, however, not so badly written, and give cogent arguments against the mudflow hypothesis. The elder Lee (1983, p. 58) wrote that many geologists "have stated that the deposits would definitely be called glacial till were it not for the presence of artifacts within them. This has been the reaction of almost all visiting geologists."

To Thomas E. Lee (1983, p. 58), the signs of the glacial origin of the deposits in question were unmistakable: "Among the indications which point to till are the lenses of fine gravels and sands observed in the lower half of the deposits. Such lenses are typical of till."

Any kind of mudflow or soil creep (solifluxion) would have required a slope of an appropriate inclination in the immediate area of the site, but no such slope was evident. The paths of flows from more distant high areas were blocked by transverse ridges of quartzite bedrock. Furthermore, according to T. E. Lee (1983, p. 58), the deposits in question were not of the same type as those resulting from solifluxion.

Lee (1983, p. 58) added: "An adequate explanation must also include the evidence in the Middle Quarry area near the high point of the hill, where unsorted artifact-bearing deposits are perched on the top of a ridge. There is no place from which soil could have crept, unless we can conceive of it crossing a swamp and climbing a ridge." Lee (1983, p. 59) concluded the deposits had been left on the ridge, and elsewhere, by glaciers.

Lee also considered other possible explanations for the presence of stone implements deep in unsorted deposits. One was that the deposits had been churned by the action of frost. But Lee (1983, pp. 59-60) pointed out: "If frost action has been churning the unsorted deposits, it is difficult to see why the artifact assemblages are not thoroughly mixed. . . . Frost churning cannot account for superimposed assemblages, typologically and quantitatively different, within the unsorted deposits. The occurrence of undisturbed horizontal lenses of fine gravels and sands in the lower half of the till deposits, sometimes with artifacts directly under them, is conclusive evidence that frost action did not severely affect the lower beds." The introduction of tools into the deposits by tree roots was considered and rejected for the same reasons.

Summarizing his findings, T. E. Lee (1983, p. 71) wrote: "Various explanations for the unsorted artifact-bearing deposits have been considered, including tree plowing, root slumping, beach

action, ice rafting, frost action, viscous flow, and soil creep. Although these factors may have been operative in a minor way, they do not explain the main body of the observed evidence. The suggestion of glacial till, on the other hand, is favored and supported by the nature of the deposits and their peculiar position on the site; the faceted stones, many of which are striated; the distribution and condition of encased artifacts; the occurrence of sand 'lumps'; and the presence of certain horizontal lenses of sorted sands, which are typical of till."

Sanford visited the Sheguiandah site several times during the period 1952-1957. In agreement with Lee, Sanford (1983, p. 82), a professional geologist, found the unsorted artifact-bearing layers to be glacial till: "There is no doubt in the writer's mind that this is till, although its origin has been questioned. It is made of a heterogeneous mixture of material ranging from clay to boulders." Sanford, for the same reasons as Lee, believed that neither frost, nor root action, nor mudflows could explain the formation of the layers in question and the presence in them of stone tools. The layers of till were thin, but Sanford said this was to be expected because previous glaciations had stripped the region of materials that could have been incorporated into the tills.

Sanford (1971, p. 7) wrote: "Perhaps the best corroboration of these unsorted deposits as ice-laid till was the visit of some 40 or 50 geologists to the site in 1954 during the annual field trip of the Michigan Basin Geological Society. At that time the excavation was open and the till could be seen. The sediments were presented to this group in the field as till deposits, and there was no expressed dissension from the explanation. Certainly had there been any room for doubt as to the nature of these deposits it would have been expressed at this time."

The belief that the Sheguiandah deposits are something other than glacial till is not confined to scientists holding the view that humans entered North America no earlier than 12,000 years ago. Even maverick researchers who accept a far more ancient date of entry hesitate about Sheguiandah because of the advanced nature of the stone tools found there. According to these researchers, early Americans, living in early Wisconsin and pre-Wisconsin times at sites such as Buchanan Canyon in San Diego and Calico in southern California, had a very primitive level of culture and used only the crudest kind of stone tools. From one advocate of this point of view, we have heard that the Sheguiandah tills are actually storm-driven beach deposits, perhaps 10,000 years old at most. This despite the fact that T. E. Lee (1983, p. 67) said "beach action has been considered and rejected." The reports of Lee and Sanford, full of references to old beaches on Manitoulin Island, show that they were fully acquainted with beach deposits and could make evaluations concerning their presence at particular locations. Arguing against the storm-driven beach deposit hypothesis is the fact that the stone tools found in the till are "not severely ground, battered or smashed" and that their "edges are often sharp" (T. E. Lee 1983, p. 58).

In cases of controversial claims, such as those made by Lee in connection with Sheguiandah, it is, of course, to be expected that counterarguments will be presented. But when such counterarguments and critiques are repeated blindly as conclusive verdicts, this tends to prevent any genuine discussion of the real issues. We might imagine the following classroom scene. "Tools found in till at Sheguiandah?" says the professor. "That's nonsense. Everyone knows the so-called



till is just a beach deposit.” Even a student with some genuine interest in the matter might hesitate to raise further questions because of fear of ridicule.

If one approach is to deny that the unsorted tool-bearing deposits are till, another is to demand excessively high levels of proof for a human presence at the site at the designated time. James B. Griffin, an anthropologist at the University of Michigan, believed that the most certain date for the entry of humans into the New World was 12,000 years ago, although he admitted growing evidence favored a 20,000-year date. Griffin (1979, pp. 43-44) added: “There are, however, some evaluations of the antiquity of man in the New World measured in the high tens of thousands and to hundreds of thousands of years or more on what I regard as either provocative or very slim evidence. These are simply not regarded as demonstrated by a large number of competent authorities. ”

This is how the social process of science works. Someone might present a good case, but if a consensus of established authorities does not support it, it goes by the wayside.

Griffin (1979, p. 46) further stated: "There are a large number of locations in North America for which considerable antiquity has been claimed as places inhabited by early Indians. Even whole books have been published on nonsites.

The reasons it is now difficult or impossible to include such 'sites' varies from location to location; a detailed dissent is not within the scope of this chapter." Griffin included Sheguiandah in the category of a nonsite. Of course, it is understandable that Griffin may not have had space in a chapter authored by him in a collective work to give a detailed discussion of why Sheguiandah should not be considered quite old. But he should have at least given some reference to where such a detailed dissent might be found. This he failed to do. As yet, we have found no detailed refutation in print, by Griffin or anyone else, of Sanford's analysis of the site's geology.

If, according to Griffin, Sheguiandah is a nonsite, then what is a real site? Griffin (1979, p. 44) stated that a proper site must possess “a clearly identifiable geologic context. . . . with no possibility of intrusion or secondary deposition.” He also insisted that a proper site must be studied by several geologists expert in the particular formations present there, and that there must be substantial agreement among these experts. Furthermore, there must be “a range of tool forms and debris . . . well preserved animal remains . . . pollen studies . . . macrobotanical materials . . . human skeletal remains. Griffin also required dating by radiocarbon and other methods. Although Griffin (1979, p. 44) himself admitted “this is an ideal model for an ‘Early Man’ site,” he nevertheless insisted that “Insofar as finds that have been proposed fail to satisfy such criteria they are inevitably open to question, rejection, or suspended judgement. ” The problem here is that practically none of the locations where major paleoanthropological discoveries have been made would qualify as genuine sites. This includes many sites crucial to the picture of human evolution so carefully built up over the past century. For example, most of the African discoveries of *Australopithecus*, *Homo habilis*, and *Homo erectus* have occurred not in “clearly identifiable” geological contexts, but on the surface or in cave deposits, which are notoriously difficult to interpret geologically. Most of the Java *Homo erectus* finds also occurred on the surface, in poorly

specified locations. At none of the places of these discoveries can one find the combination of factors Griffin deemed necessary for a proper site.

In this regard, it is interesting to note that the Sheguiandah site appears to satisfy most of Griffin's stringent requirements. Implements were found in a geological context clearer than that of many accepted sites. Several geologists expert in North American glacial deposits did apparently agree on an age in excess of 30,000 years. Evidence suggested there was no secondary deposition or intrusion. A variety of tool types were found. pollen studies and radiocarbon tests were performed, and macrobotanical materials (peat) were present. The only things absent were human and animal bones.

A few years after dismissing Sheguiandah as a nonsite, Griffin grudgingly accepted it as a recent site. In reading the report containing this admission, one gets the impression that the only tools found were those lying on or near the surface, and that the Sheguiandah site can best be dated with reference to peat bogs that formed after Manitoulin Island emerged from Lake Algonquin about 9,000 years ago. There is not the slightest hint that tools were found in glacial till and meltwater deposits. Griffin (1983, p. 247) stated, referring to Sheguiandah and two neighboring locales: "A reasonable estimate of the age of these sites would be from about 7000 to 6000 b.c., a time when there is a high pollen count from a nearby bog at Sheguiandah. These sites are almost certainly not earlier than the period of the lowest level of Lake Algonquin. "

In 1974, a similar approach was taken by P. L. Storck of the Royal Ontario Museum in Toronto. He listed Sheguiandah as a Shield Archaic site. The Shield Archaic is a recent, and broadly defined, Indian stone tool culture that spread across much of central Canada. Lee protested, pointing out the absurdity of treating the tools from Sheguiandah as if they all belonged to a single unit of recent historical time. To do so would mean ignoring the obviously stratified nature of the Sheguiandah site, with tools of distinct types found on the surface and at different levels below the surface—within glacial tills, meltwater deposits, and lacustrine sediments (T. E. Lee 1974).

The Shield Archaic culture was preceded by Paleo-Indian cultures in Canada, and it may thus be called a post-Paleo-Indian culture. According to Lee, the Paleo-Indian culture is represented at Sheguiandah by the upper projectile point level, lying above the glacial tills. The Shield Archaic came later, and might be represented by the tools found lying on the surface at Sheguiandah. In any case, both the Shield Archaic and Paleo-Indian cultures came after the glacial period. Disputing the Shield Archaic labeling of Sheguiandah, T. E. Lee (1974, p. 24) asked the following questions: "Where do we go to find post-Paleo-Indian in and beneath primary glacial tills, in meltwater deposits, and beneath boulder pavings? Shall we consult the geologists who extensively and intensively studied the site during four years while the trenches were open? Will their opinions carry weight in the face of a young archaeologist's statement? Four of them—the most closely involved out of a hundred geologists who saw the trenches—Dr. Sanford, Dr. Antevs, Dr. Terasmae, and Dr. Liberty put the age of the site at 'a minimum of 30,000 years.' They did so on the advice of old Dr. Antevs, who warned them that the profession was not prepared to accept an older date, and for the most part would balk at 30,000 (as they did). "

In recent years, a minority of archeologists have begun to accept sites showing a human presence in North America over

30,000 years ago. It is noteworthy that few of these archeologists mention the Sheguiandah site, testifying to the effectiveness of the suppression of reports regarding it. An exception is W. N. Irving of the University of Toronto. As early as 1971, he was drawing attention to sites at Old Crow River and Edmonton that yielded deliberately fractured bone from the middle and early Wisconsin respectively (Irving 1971, pp. 69, 71). The

Edmonton site may have been late Sangamon interglacial.

Irving (1971, p. 71) then wrote: "I think our recent findings require that Sheguiandah be reexamined, for the investigations there were not completed. No one has yet suggested an age of 30,000 years or more for Sheguiandah, and I do not do so now, but I would like very much to know how old it really is, and what is there." Irving was apparently unaware of Sanford's work, or deliberately avoided saying anything about it.

The most favorable review of Sheguiandah we have yet been able to locate comes from Jose Luis Lorenzo, of the National Institute for Anthropology and History, in Mexico City. He wrote (Lorenzo 1978, p. 4): "The site is a complex one with several levels of likely occupancy due to the fact that there is a type of quartzite on the island that is an excellent material for artifacts. Various series of artifacts were also found mixed with glacial debris at the bottom of the stratigraphy. All the studies on the glacial ecology of the area indicate that the remains not mixed with till are later than 12,500 years ago, whereas those that were mixed go back over 30,000 years according to available data (Prest 1969; Flint 1971; Dreimanis and Goldthwait 1973)."

It would seem that the Sheguiandah site deserves more attention than it has thus far received. The discoverer, Thomas E. Lee, certainly felt frustrated because of this. Looking back to the time when it first became apparent to him that stone implements were being found in glacial till, T. E. Lee (1968, p. 22) wrote: 'At this point, a wiser man would have filled the trenches and crept away in the night, saying nothing. Books had been written, lectures had been given, pronouncements made, and armchairs comfortably filled. . . . Indeed, while visiting the site, one prominent anthropologist, after exclaiming in disbelief, 'You aren't finding anything down there?' and being told by the foreman, 'The hell we aren't! Get down in here and look for yourself!', urged me to forget all about what was in the glacial deposits and to concentrate upon the more recent materials overlying them. Today, 13 years after vigorous professional efforts succeeded in halting the investigation of that great site, the same arguments and distortions are spreading through the literature. . . . The sacred cow must be defended, and to hell with the facts."

#### 5.4.2 Lewisville: The Vendetta Goes On (Late Pleistocene)

In 1958, at a site near Lewisville, Texas, stone tools and burned animal bones were found in association with hearths. Later, as the excavation progressed, radiocarbon dates of at least 38,000 years were announced for charcoal from the hearths. Still later, a Clovis point was found. Herbert

Alexander, who was a graduate student in archeology at the time, recalled how this sequence of finds was received. "On a number of occasions" stated Alexander (1978, p. 20), "I had the opportunity to listen to faculty and visitors discuss their visits to this site. The opinions voiced at that time were that the hearths were man-made, and the faunal associations valid. Once the dates were announced, however, some opinions were changed and after the Clovis point was found, the process of picking and ignoring began in earnest. Those who had previously accepted the hearths and/or faunal associations began to question their memories."

Finding a Clovis point in a layer 38,000 years old was disturbing, because orthodox anthropologists date the first Clovis points at 12,000 years, marking the entry of humans into North America. Some critics responded to the Lewisville find by alleging that the Clovis point had been planted as a hoax.

After mentioning a number of similar cases of ignored or derided discoveries, Alexander (1978, p. 22) recalled a suggestion that "in order to decide issues of early man, we may soon require attorneys for advocacy." This may not be a bad idea in a field of science like archeology, where opinions determine the status of facts, and facts resolve into networks of interpretation. Attorneys and courts may aid archeologists in arriving more smoothly at the consensus among scholars that passes for the scientific truth in this field. But Alexander noted that a court system requires a jury, and the first question asked of a prospective juror is, "Have you made up your mind on the case?" Very few archeologists have not made up their minds on the date humans first entered North America.

#### 5.4.3 Timlin, New York (Late Pleistocene)

The idea that Clovis-type projectile points represent the earliest tools in the New World is challenged by an excavation at the Timlin site in the Catskill mountains of New York State. In the mid-1970s, tools closely resembling the Upper Acheulean tools of Europe were found there. In the Old World, Acheulean tools are routinely attributed to *Homo erectus*. But such attribution is uncertain because skeletal remains are usually absent at tool sites. The Catskill tools have been dated to some 70,000 years b.p. on the basis of glacial geology. An interesting feature of the Timlin site is that investigators have been able to trace sequences of stone tool cultures from the "Upper Acheulean" level up to the recent Archaic period (Raemsch and Vernon 1977).

#### 5.4.4 Hueyatlatco, Mexico (Middle Pleistocene)

In the 1960s, highly sophisticated stone tools (Figure 5.10) rivaling the best work of Cro-Magnon man in Europe were unearthed by Juan Armenta Camacho and Cynthia Irwin-Williams at Hueyatlatco, near Valsequillo, 75 miles southeast of Mexico City. Stone tools of a somewhat cruder nature were found at the nearby site of El Homo. At both the Hueyatlatco and El Homo sites, the stratigraphic location of the implements does not seem to be in doubt. However, these artifacts do have a very controversial feature: a team of geologists, some working for the U.S. Geological Survey, gave them dates of about 250,000 years b.p. This team, working under a grant from the National Science Foundation,

Figure 5.10. Stone tools found at Hueyatlaco. Mexico, a site dated at about 250,000 years by a team from the United States Geological Survey.

consisted of Harold Malde and Virginia Steen-McIntyre, both of the U.S. Geological Survey, and the late Roald Fryxell of Washington State University.

These geologists said four different dating methods independently yielded an anomalously great age for the artifacts found near Valsequillo (Steen-McIntyre et al. 1981). The dating methods used were (1) uranium series dating, (2) fission track dating, (3) tephra hydration dating, and (4) study of mineral weathering. The carbon 14 and potassium-argon methods were not applicable at the Hueyatlaco and El Homo sites, and paleomagnetic measurements did not provide any useful information.

As might be imagined, the date of about 250,000 years obtained for Hueyatlaco by the U.S. Geological Survey team provoked a great deal of controversy. If accepted, it would have revolutionized not only New World anthropology but the whole picture of human origins. Human beings capable of making the sophisticated tools found at Hueyatlaco are not thought to have come into existence until about 100,000 years ago in Africa,

Of course, it is possible to dispute the dates reported by the U.S. Geological Survey team. But something more than a legitimate scientific disagreement over dating techniques appears to have been involved in the treatment of Hueyatlaco, as we shall see from the testimony of Virginia Steen-McIntyre. First, however, we shall examine how the anomalously old dates for the site were obtained.

#### 5.4.4.1 The Uranium Series Dating of the Hueyatlaco Site

The principal technique used for dating materials from Hueyatlaco and El Homo was the uranium series method. The tests were performed by Barney J. Szabo of the U.S. Geological Survey (Szabo et al. 1969). In this section, we will discuss Szabo's results in some detail to show they support his dates. In particular, Szabo's data suggest that leaching of uranium from the sample could not have produced erroneously old dates, as some have hypothesized. Readers uninterested in the technicalities may proceed to the next section,

The uranium series technique is based on the fact that each of several isotopes of uranium spontaneously breaks down into a distinct series of byproducts. At the Hueyatlaco and El Homo sites, Szabo was concerned with uranium 238 and uranium 235,

Uranium 238 decays to uranium 234, with a half life of 4,51 billion years, and uranium 234 decays to thorium 230, with a half life of 248,000 years, Thorium 230 in tum decays to radium 226, with a half life of 75,000 years (Considine 1976, p. 1866).

Uranium 235 decays to protactinium 231, with a half life of 707 million years, and protactinium 231 decays to actinium 227, with a half life of 32,500 years (Considine 1976, p, 1868),

The concept of a half life can be explained as follows, Sayyou start out with one pound of uranium 234, with a half life of

248.0 years, After 248,000 years, you would have a half pound of uranium 234, along withsome thorium and radium. After another 248,000 years, you would have a quarter pound ofuranium 234, with more thorium and radium, after another

248.0 years, an eighth of a pound of uranium and still more thorium and radium, and so on,

Small amounts of the uranium isotopes that form the starting points of our two series (uranium 238 and uranium 235) occur naturally in water, yet their decay products, thorium and protactinium respectively, are not found in water (Gowlett 1984, p, 86), Certain types of rocks (such as travertines, tufas, and concretions) form when inorganic carbonates precipitate out of water. During this precipitation, small quantities of uranium are included within the rock, but no thorium or protactinium. Hence, under ideal conditions, all of the thorium and protactinium found within such rocks comes from the decay of uranium isotopes, Also, bones that are soaked in uranium-bearing water tend to absorb uranium, which decays and produces byproducts,

Since the half lives of uranium, thorium, and protactinium are known, scientists say that by measuring the amounts of these elements present within a sample they can calculate the age of the sample. The more decay products present in the sample, the older it is, Determining the exact age of the sample is complicated by the fact that uranium and its byproducts may migrate in or out of the sample, An open system is one in which such migration occurs; a closed system is one in which migration does not occur.

Uranium series tests were applied to samples from Hueyatlaco and the nearby site of El Homo (Szabo et al. 1969). In obtaining these dates, both the uranium 234/thorium and the uranium 235/protactinium series were used, and they yielded results that were in substantial agreement with each other.

Calculations yielded dates of about 245,000 years b.p. for sample MB3 (a camel pelvis) from Unit C of the Hueyatlaco site. Unit C is the uppermost layer at Hueyatlaco, and was found to contain highly sophisticated stone tools. This layer is underlain by Unit E, which contained similar tools, and Unit I, which contained tools of a simpler mode of manufacture. Unit E and Unit I are separated by a stratigraphic discontinuity, which suggests that Unit I is considerably older than Unit E. In other words, 245,000 years is a minimum age for the site, the lower levels of which could be substantially older.

The uranium series method gave open and closed system estimates of over

280,000 years for sample MBS, a mastodon tooth, from El Homo. The El Homo site is at a lower stratigraphic level than any of the Hueyatenco layers, and contained tools similar to those of Unit I, the lowest tool-bearing layer at Hueyatenco. One wonders what remains of human culture might be found at levels lower, and hence older, than El Homo.

Szabo reported that he used calculations based on both open and closed systems in obtaining the above uranium series test results. Nevertheless, some scientists have suggested that these dates are in error because uranium and its decay products may have migrated into or out of the samples over the course of their interment to a greater extent than Szabo supposed. Cynthia Irwin-Williams, who originally discovered the tools, suggested that the real age of the samples should be around

25,000 years. But careful study of the data supplied by Szabo, who performed the uranium series tests, appears to rule out the hypothesis that migration caused falsely old dates.

There are two ways that a falsely old age can be obtained by uranium series dating—outward migration of uranium or inward migration of byproducts. If uranium has migrated out, this will result in a higher ratio of byproducts (thorium or protactinium) to uranium in a sample, and hence a greater than normal age for the sample. If byproducts (thorium or protactinium) have migrated in, that <sup>230</sup>Th, of course, also result in a higher than normal ratio of byproducts, and hence a greater age for the sample. This latter alternative is, however, highly unlikely since both thorium and protactinium are virtually insoluble in water.

Furthermore, thorium 230, the isotope produced by the decay of uranium 234, is always accompanied in nature by the far more common isotope thorium 232. So let us suppose that the Hueyatenco bone samples are in fact very young. Let us also suppose, although it is quite unlikely, that thorium 230 and thorium 232 have migrated into the bone, giving a falsely old age. In this case, one would expect to find a low ratio of thorium 230 to thorium 232, because thorium 232 is more common than thorium 230. But it was reported (Szabo et al. 1969, p. 243) that the ratio of thorium 230 to thorium 232 in the samples under consideration was "unusually high," which indicates that virtually all the thorium 230 measured in the samples was produced by the decay of uranium 234.

We have thus established that the uranium byproducts thorium and protactinium most probably did not migrate into the samples. That means that the hypothesis of a falsely old age depends on uranium migrating, or leaching, out of the samples.

In order to investigate the possibility of uranium leaching out of a sample, one of us (Thompson) analyzed two of several possible models—one in which leaching takes place at the end of the period of burial and one in which leaching is continuous throughout the period of burial. We shall now briefly discuss the results of these calculations.

Let us first consider the model in which leaching of uranium took place at the end of the period of burial. Taking bone sample MB3, we assumed, as claimed by Cynthia Irwin-Williams, that its real age is only 25,000 years instead of roughly

245,000 years. Then we computed the amount of leaching that must have taken place in order to give a date of 245,000 years for sample MB3, using the ratio of protactinium to uranium 235.

Sample MB3 was also originally dated at roughly 245,000 years using the ratio of thorium to uranium 234. So when we plugged the leaching factor for uranium 235 into the uranium 234 series equations we expected that the ratio of thorium to uranium 234 would yield a date of 25,000 years. Here we assumed that uranium 234 and uranium 235 are chemically identical (as atomic theory says they are) and that any leaching process would affect them equally. But the ratio of thorium to uranium 234, when calculated using the standard equations for radioactive decay, yielded an age of 52,451 years instead of 25,000 years. This result calls into question the leaching hypothesis.

A similar result was obtained when we reversed the order of the calculations. First we computed a uranium leaching factor using the ratio of thorium to uranium 234 and a sample age of 25,000 years. Using this uranium leaching factor, we then computed an age for sample MB3 based on the ratio of protactinium to uranium 235. This procedure yielded a date of 11,675 years rather than the expected 25,000 years.

Either way, these results are not consistent with the idea that the sample was deposited only 25,000 years ago, and that the leaching of uranium occurred fairly recently, at the end of the period of burial. According to our model, we should expect both sets of uranium series computations, done using the standard equations for radioactive decay, to yield results near

25,000 years. But they did not.

When we performed the computations assuming that leaching of uranium took place continuously rather than at the end of the period the bone sample was buried, similar results were obtained. In summary, the hypothesis that uranium leached out of the samples (either all at the end or continuously throughout the period of burial), and that the samples are therefore only

25,000 years old, is not consistent with the activity ratios reported for these samples.

At this point, one might raise the following objection. Admittedly, the date of 25,000 years suggested by Cynthia Irwin-Williams does not give good results in the above analysis. If we assume leaching of uranium 234 and uranium 235 took place, we would expect the computations for the thorium/uranium 234 and protactinium/uranium 235 ratios to yield the same results—25,000 years. But they did not. Then what about some other relatively young date? Could it be possible that using this alternative young date, good agreement might result?

We varied the assumed age for sample MB3, using the continuous leaching model, from 25,000 years through 250,000 years to see at what age the protactinium/uranium 235 age agreed best



with the thorium/uranium 234 age. For assumed protactinium/uranium 235 ages from 25,000 up to 140,000 years the protactinium/uranium 235 ages disagreed with the thorium/uranium 234 ages by more than 30 percent. For a protactinium age of 180,000 years the thorium age disagreed by 20 percent, and the difference dropped as the assumed protactinium age increased. At 235,000 years the two differed by only .2 percent and at 245,000 years they differed by 3.1 percent. Thus the data reported by Szabo strongly support a date of around 235,000 years b.p. for the upper artifact-bearing layer (Unit C) at Hueyatlatco.

The same calculation was performed for sample MB8 using the continuous leaching model. The protactinium age was varied from 25,000 years through

370.0 years. We found that the thorium dates disagreed with the protactinium dates by more than 30 percent from 25,000 up to 260,000 years. At 300,000 years the two disagreed by 16 percent and this difference decreased to .32 percent at

355.0 years. Thus the activity ratios reported by Szabo strongly support an age of about 355,000 years for this sample from the site of El Horno, even if we assume, for the sake of argument, there was continuous leaching of uranium. Szabo pointed out that "sample MB8 was a tooth fragment from a butchered mastodon at El Horno, the oldest known site, and was therefore itself an artifact" (Szabo et al. 1969, p. 240).

Uranium series dating methods were also applied to bone samples from the nearby Caulapan site, yielding dates of about

20.0 years b.p. These agreed nicely with carbon 14 dates of 21,850 and 30,600 years from this site. We should note that the Caulapan carbon 14 date of 21,850 years applies to mollusk shells associated with the single artifact found at this site. Cynthia Irwin-Williams (1978, p. 22 1981, p. 258) maintained that this was the only valid date for any Valsequillo artifact, and it should therefore be used for Hueyatlatco and El Horno as well. But the U.S. Geological Survey team could find no "geological basis by which the relation of the Caulapan deposits to Hueyatlatco can be determined" (Malde and Steen-McIntyre 1981, p. 420). Therefore Irwin-Williams's dating of the Hueyatlatco and El Horno sites at about 25,000 years is not justified.

#### 5.4.4.2 Other Methods Used for Dating Hueyatlatco and El Horno

In addition to the uranium series method, the team of geologists used fission track counting, tephra hydration dating, and mineral weathering analysis to assign dates to the implement-bearing layers at Hueyatlatco and El Horno.

Fission track dating is based on the accumulation of radioactive decay tracks in volcanic mineral crystals as a function of time. The more tracks, the older the crystals. When the age of crystals in a volcanic deposit has thus been determined, it is possible to assign appropriate dates to implements or fossils found beneath the volcanic layer in question. The fission track method was applied to two volcanic layers (the Tetela mud and the Hueyatlatco ash), situated above the most recent Hueyatlatco artifacts. The fission track dates for these layers should give a minimum age for

all of the Hueyatenco tools. The fission track dates were 260,000 to 940,000 years for the Tetela mud and 170,000 to 570,000 years for the Hueyatenco ash. The considerable ranges in the dates were attributed to statistical effects due to the small number of fission tracks that were counted (Malde and Steen-McIntyre 1981, p. 419). The date ranges for the two volcanic layers overlap in the interval from

260,000 years to 570,000 years b.p. Tephra hydration dating is a relatively new technique. It relies on the fact that volcanic glass, or tephra, slowly absorbs water. For this method to be feasible, it is necessary to have independently dated control samples of volcanic glass with the same chemical properties and geological situation as the samples to be dated. In this case, control samples were taken from the nearby La Malinche volcano. The method gave a date of about 250,000 years b.p. for tephra deposits associated with the Hueyatenco artifacts (Steen-McIntyre et al. 1981, p. 13).

The final method of dating, the study of the weathering of a volcanic mineral, hypersthene, gives only a relative measure of age. As time passes, exposed crystals of this mineral are slowly etched, leaving a "picket fence" profile when viewed under a microscope. At the nearby human occupation site of Tlapacoya, this etching was rare and incipient in volcanic deposits dated by carbon 14 to about 23,000 years b.p. In contrast, the etching was pronounced in volcanic deposits associated with the Hueyatenco artifacts. This suggested that the Hueyatenco artifacts must have an age considerably greater than 23,000 years (Steen-McIntyre et al. 1981, p. 11).

A final consideration in the dating of the Hueyatenco artifacts is that they were found buried beneath at least 10 meters (33 feet) of sediment. Geological study showed that these strata had to accumulate before being cut by the nearby Atoyac River, which has carved a valley 50 meters (164 feet) in depth (Steen-McIntyre et al. 1981, p. 10).

In other words, the geological history of the site would go something like this. The artifacts were left on an ancient land surface. Layers of sediment were deposited over them. Then the river began to cut through the layers of sediment.

Given this sequence, it is possible to estimate the age of the tools. Two elements are required. The first is the time required to deposit at least 10 meters of sediments over the tools. The second is the time that the river took to cut its valley, which is now 50 meters deep. If one could estimate the time it took the river to cut its valley and then add the time it took to deposit at least 10 meters of sediments over the tools, then one would have a rough date for the tools.

Since the valley and its side channels have gentle slopes, it is not likely that the river has exhibited an unusually high rate of erosion. But even if we assume a rather high rate of erosion, as in the Colorado River valley, the river Atoyac would have required around 150,000 years to carve out its present channel (Steen-McIntyre et al. 1981, p. 10). Add to this the time originally required to deposit 10 or more meters of sediment over the tools, and it can thus be seen that the geology of Hueyatenco and the Rio Atoyac valley corroborates the ancient date obtained by the four dating methods discussed previously.

We have examined in some detail the cases of Hueyatenco and El Homo in order to show that the dates for stone tools from these sites were solidly based on serious scientific analysis, more rigorous than in many accepted dating studies. However, due to the anomalous character of the 250,000-year figure, this dating has proven to be extremely controversial. The daters declared themselves to be "painfully aware" of the dilemma they had caused and "perplexed" about how to resolve it. Roald Fryxell said: "We have no reason to suppose that over decades, actually hundreds of years, of research in archaeology in the Old and New World our understanding of human prehistory is so inaccurate that we suddenly discover that our past understanding is all wrong. ... On the other hand, the more geological information we've accumulated, the more difficult it is to explain how multiple methods of dating which are independent of each other might be in error by the same magnitude" (Denver Post, November 13, 1973).

According to Cynthia Irwin-Williams, the date of 250,000 years was impossible: "These tools surely were not in use at Valsequillo more than 200,000 years before the date generally accepted for development of analogous stone tools in the Old World, nor indeed more than 150,000 years before the appearance of Homo sapiens" (Szabo et al. 1969. p. 241).

Negative responses to the dating of the Valsequillo sites of Hueyatenco and El Homo arise from acceptance of a theory of human evolution that was established by unwarranted elimination of extensive evidence for the extreme antiquity of humans in both the Old and New Worlds. In light of the total evidence, a date of 250,000 years b.p. for sophisticated stone tools is not greatly surprising. Ironically, in the treatment of the Valsequillo findings by the scientific community, we see the same tendency to suppress unwanted evidence that eliminated the earlier material and thereby rendered the Valsequillo dates unbelievable.

#### 5.4.4.3 Negative Reception of the Hueyatenco Evidence

Virginia Steen-McIntyre has sent us some of her correspondence, which documents the difficulties she had in publishing her findings on Hueyatenco. We shall now introduce excerpts from this correspondence. Our purpose in doing so

Is to clarify how anomalous evidence is treated by the scientific community.

We have already shown that much evidence for the presence of anatomically and culturally modern humans in the Tertiary epoch was suppressed in the late nineteenth and early twentieth centuries, mainly because it conflicted with emerging theories of human evolution. Some might object that we have misinterpreted what went on in that period, taking the normal scientific procedures scientists use in differentiating good evidence from bad as some kind of diabolical plot to distort the truth. Others will maintain that even if good evidence was in fact rejected for reasons that appear unscientific in hindsight, this just does not happen any more. But the case of Hueyatenco (along with Texas Street, Shiguandah, Calico, and Lewisville) demonstrates otherwise.

Among the social processes that discourage acceptance and reporting of anomalous evidence are ridicule and gossip, including attacks on character and accusations of incompetence. Furthermore,

discoveries have almost no impact in the world of science unless they are published in standard journals. The editorial process, especially the practice of anonymous peer review, often presents an insurmountable obstacle. Some submissions are met with a wall of silence. Others are shunted around for months, from editor to editor. Sometimes manuscripts are mysteriously lost in the shuffle. And while positive reports of anomalous evidence are subjected to protracted review and/or rejection, negative critiques are sometimes rushed into print. Occasionally, a maverick report eventually does appear in a journal, but only after it has gone through such extensive modification that the original message has become totally obscured—by editorial deletions and, in some cases, rewriting of data.

Thomas E. Lee's attempts to get articles about Sheguiandah published (Section 5.4.1.2) exemplify what can happen. Also, we heard from a paleontologist at the San Diego Museum of Natural History that a forthcoming paper by other researchers on an incised elephant bone found in the Anza-Borrego Desert of southern California and dated at over 250,000 years would never make it past peer review (Section 2.3). The word was out that the article was coming, and competent authorities had already decided what would happen to it.

Virginia Steen-McIntyre experienced many of the above-mentioned social pressures and obstacles. In a note to a colleague (July 10, 1976), she stated: "I had found out through backfence gossip that Hal [Malde], Roald [Fryxell], and I are considered opportunists and publicity seekers in some circles, because of Hueyatlaco, and I am still smarting from the blow."

The publication of a paper by Steen-McIntyre and her colleagues on Hueyatlaco was inexplicably held up for years. The paper was first presented in 1975 at a joint meeting of the Southwestern Anthropological Association and the *Societe Mexicana de Antropologia* and was to appear in a symposium volume. Four years later, Steen-McIntyre wrote (March 29, 1979) to H. J. Fullbright of the Los Alamos Scientific Laboratory, one of the editors of the forever forthcoming book: "We received your name and address from Dave Snow, who said you were the one to contact about the publication date for the SWAA-SMA symposium volume. We hope that it is soon! I personally have been put in an awkward position by the publication delay. Our joint article on the Hueyatlaco site is a real bombshell. It would place man in the New World 10x earlier than many archaeologists would like to believe. Worse, the bifacial tools that were found in situ are thought by most to be a sign of *H. sapiens*. According to present theory, *H.s.* had not even evolved at that time, and certainly not in the New World."

Steen-McIntyre continued, explaining: "Archaeologists are in a considerable uproar over Hueyatlaco—they refuse even to consider it. I've learned from second-hand sources that I'm considered by various members of the profession to be 1) incompetent; 2) a news monger; 3) an opportunist; 4) dishonest; 5) a fool. Obviously, none of these opinions is helping my professional reputation! My only hope to clear my name is to get the Hueyatlaco article into print so that folks can judge the evidence for themselves. (Geologists have no trouble with it.) The longer the delay, the more archaeologists will be convinced that the whole thing is just a crass attempt of another

egomaniac for publicity. I'm quite certain the archaeologist who was in charge of the excavations and who no longer corresponds with me feels this way."

Steen-McIntyre, upon receiving no answer to this and other requests for information, withdrew the article. Later she got a letter from Roger A. Morris of Los Alamos, who explained that he had taken the liberty of opening a letter addressed to Fullbright, who had been transferred to another group of researchers. Morris said he would return her manuscript, but it never came.

A year later, Steen-McIntyre wrote (February 8, 1980) to Steve Porter, editor of *Quaternary Research*, about having her article printed. She first explained its status. "It's been languishing down in Los Alamos for almost five years, awaiting publication as part of a symposium volume. During that time I have written or called a dozen times to learn the status of the volume only to receive no response. (The original editor was always 'in conference' or 'out of the office' or would 'return my call,' which he never did.) In the meantime, there's been a lot of false information circulated about the site and the work we did there in 1973. Especially damaging is an article by Cynthia Irwin-Williams published in 1978 (Summary of archaeological evidence from the Valsequillo Region, Puebla, Mexico, in *CWFwra/ Continuity in Mesoamerica*, Brownman,

D. L. ed, Mouton).

In it she discounts Szabo's uranium-series dates (concordant) on butchered bone supplied by herself because she doesn't believe in the method. She does the same with Naeser's 2 sigma zircon fission track dates for two tephra layers that we proved by a cross-trench and direct tracing of the stratigraphy to overlie beds exposed in the archaeological trenches. Needless to say, she never showed us a draft of this ms or even told us she planned to publish anything on Hueyatlaço!"

Steen-McIntyre added: "The ms I'd like to submit gives the geologic evidence. It's pretty clear-cut, and if it weren't for the fact a lot of anthropology textbooks will have to be rewritten, I don't think we would have had any problems getting the archaeologists to accept it. As it is, no anthropology journal will touch it with a ten foot pole. Right now I don't even have a copy to send you. The editor's copy is still in Santa Fe and my working copy disappeared into the office of Science 80 (AAAS) months ago and, despite howls and threats, has yet to be returned."

Steve Porter wrote to Steen-McIntyre (February 25, 1980), replying that he would consider the controversial article for publication. But he said he could "well imagine that objective reviews may be a bit difficult to obtain from certain archaeologists." The usual procedure in scientific publishing is for an article to be submitted to several other scientists for peer review. It is not hard to imagine how an entrenched scientific orthodoxy could manipulate this process to keep unwanted information out of scientific journals. The manner in which reports by Thomas E. Lee about the Shiguiandah site were kept out of standard publications provides a good example of this (Section 5.4.1.2).

Steen-McIntyre wrote to Porter (March 4, 1980): "Often It Is next to Impossible to get a controversial paper published that even Indirectly challenges current archaeological dogma; George Carter Is a case In point!" In a letter to Steen-McIntyre, Carter had called the dominant clique of New World archeologists "priests of the High Doctrine" and complained that they bragged among themselves about having blocked him from publishing In the major Journals. He compared his treatment to a modern Inquisition. Steen-McIntyre then stated: "I had thought to circumvent these 'true believers' by publishing In an obscure symposium volume, but no such luck,"

The competence of Steen-McIntyre's associates was also called Into question, Steen-McIntyre Informed Porter: "there's the old saw that Fryx wasn't In his right mind when he did the work, Those folks forget that I saw the stratigraphy too, and once you get Into a cross-trench, It was relatively simple, thanks to a magnesium-stained bed that traced on the excavation wall like a pencil mark!"

On March 30, 1981, Steen-McIntyre wrote to Estella Leopold, the associate editor of Quaternary Research: "The problem as I see It Is much bigger than Hueyatlaco, It concerns the manipulation of scientific thought through the suppression of 'Enigmatic Data,' data that challenges the prevailing mode of thinking, Hueyatlaco certainly does that! Not being an anthropologist, I didn't realize the full significance of our dates back In 1973, nor how deeply woven into our thought the current theory of human evolution had become, Our work at Hueyatlaco has been rejected by most archaeologists because It contradicts that theory, period, Their reasoning Is circular. H sapiens sapiens evolved ca. 30,000-50,000 years ago In Eurasia. Therefore any Hs.s. tools 250,000 years old found In Mexico are Impossible because Hs.s. evolved ca 30,000- . . . etc. Such thinking makes for self-satisfied archaeologists but /ousyscience!"

As demonstrated In this book, the stone tools of Hueyatlaco are not an Isolated example of "impossible" evidence that challenges the recent origin of Homo sapiens by a Darwinian evolutionary process. We have already discussed numerous examples of such impossible evidence from the Pliocene, Miocene, and earlier periods. And there is much more to come in the remainder of this volume. We have simply paused briefly In order to demonstrate that the suppression of such evidence did not end with the nineteenth century—it has continued to the present day. We also take the current examples of suppression of anomalous evidence as confirmation that our Interpretation of what went on in the nineteenth century (and early twentieth century) Is in fact correct.

On May 18, 1981, Steen-McIntyre wrote to Estella Leopold and Steve Porter about "suppression of data on Hueyatlaco and other possible Pre-Wisconsinian Early Man sites in the New World by unethical means." She told how she had submitted a general paper on her dating techniques to be included in a volume in a scientific series. Steen-McIntyre then learned from the editor that "he had decided to 'drastically edit' this manuscript, essentially by deleting most of the section on Hueyatlaco and by treating the remainder in a negative way." In her letter to Leopold and Porter, Steen-McIntyre stated: "I protested strongly, and he agreed to reinsert some of the deleted

material, but only in a way that will hold both me and my research up for laughter and ridicule.” In a note to our researcher, Steve Bernath, dated January 29, 1989, Steen-McIntyre explained that the editor had, in the course of his drastic editing, altered one of her data tables. According to Steen-McIntyre: “when I threatened him he replaced the missing material [In the text] but forgot to retype the table.”

Steen-McIntyre's case is not unique. Some American scientists reporting anomalous evidence for a human presence in North America have found it necessary to publish overseas. Steen-McIntyre said in her letter to Leopold and Porter that “Roy Schlemon, a pedologist who has helped date Calico and who is working at other sites in Southern California . . . had been publishing outside the country.” A pedologist is a scientist who studies soils.

Eventually, *Quaternary Research* (1981) published an article by Virginia Steen-McIntyre, Roald Fryxell, and Harold E. Malde. It upheld an age of 250,000 years for the Hueyatlaco site. Of course, it is always possible to raise objections to archeological dates, and Cynthia Irwin-Williams (1981) did so in a letter responding to Steen-McIntyre, Fryxell, and Malde. Her objections were answered point for point in a counter-letter (Malde and Steen-McIntyre 1981). But Irwin-Williams did not relent. She, and the American archeological community in general, have continued to reject the dating of Hueyatlaco carried out by Steen-McIntyre and her colleagues on the U.S. Geological Survey team.

As in the case of Sheguiandah, the anomalous findings at Hueyatlaco resulted in personal abuse and professional penalties for those who dared to present and defend them in the scientific literature. This involved withholding of funds and loss of job, facilities, and reputation for at least one of the geologists involved in the dating project (Steen-McIntyre, personal communication).

The case of Virginia Steen-McIntyre opens a rare window into the actual social processes of data suppression in paleoanthropology, processes that involve a great deal of hurt and conflict. In general, however, this goes on behind the scenes, and the public sees only the end result—the carefully edited journals and books that have passed the censors.

A final note—we ourselves once tried to secure permission to reproduce photographs of the Hueyatlaco artifacts in a publication. We were informed that permission would be granted only if we gave a date of no more than 30,000 years for the artifacts. But permission would be denied if we intended to cite a “lunatic fringe date” of 250,000 years. We grant that the 250,000-year date may be wrong. But is it really appropriate to apply the term “lunatic fringe” to studies such as the one carried out by Steen-McIntyre and her colleagues?

#### 5.4.5 Sandia Cave, New Mexico (Middle Pleistocene)

In 1975, quite by accident, Virginia Steen McIntyre learned of the existence of another site with an impossibly early date for stone tools in North America— Sandia Cave, New Mexico, U.S.A., where the implements, of advanced type (Folsom points), were discovered beneath a layer of stalagmite considered to be 250,000 years old. One such tool is shown in Figure 5.11.

In a letter to Henry P. Schwartz, the Canadian geologist who had dated the stalagmite, Virginia Steen-McIntyre wrote July 10, 1976): "For the life of me, I can't remember if it was you or one of your colleagues I talked to at the 1975 Penrose Conference (Mammoth Lakes, California). The fellow I spoke to as we waited in line for lunch mentioned a uranium series date on the stalagmite layer above artifacts at Sandia Cave that was very upsetting to him—it disagreed violently with the commonly held hypothesis for the date of entry of man into the New World. When he mentioned a date of a quarter million years or thereabouts, I nearly dropped my tray. Not so much in shock at the age, but that this date agreed so well with dates we have on a controversial Early Man site in Central Mexico. . . . Needless to say, I'd be interested to learn more about your date and your feelings about it!" According to Steen-McIntyre, she did not receive an answer to this letter.

After writing to the chief archeological investigator at the Sandia site for information about the dating, Steen-McIntyre received this reply July 2, 1976): "I hope you don't use this 'can of wonTIs' to prove anything until after we have had a chance to evaluate it."

Figure 5.11. A Folsom blade embedded in the lower surface of a travertine crust from Sandia Cave, New Mexico (Smithsonian Miscellaneous Collections, vol. 99, no. 23, plate 7). The layer of travertine is said to be 250,000

years old.

Four years later, Virginia Steen-McIntyre wrote (February 8, 1980) in a letter to Steve Porter, editor of *Quaternary Research*: "Did you know they now have a 250,000 year date on the stalagmite layer in Sandia Cave, N.M., the one that sealed off sediments that contained leaf-shaped points and fire hearths? I've been trying to get more information out of Vance Haynes, who collected the samples, and Dr. Schwarz at McMaster [University], who ran the date, but so far no

Steen-McIntyre sent us some reports and photos of the Sandia artifacts and said in an accompanying note: "Talk about a study in frustration! Read the enclosed, then look at that picture of the 'folsom' blade imbedded in the travertine crust (stalagmite layer, 250,000 years). The geochemists are sure of their date (oral communication, GSA meeting, 1978), but archaeologists have convinced them the artifacts and charcoal lenses beneath the travertine are the result of rodent activity. The archaeologists who have seen the evidence are sure of the presence of artifacts beneath the crust, but believe the date is wrong! But what about the artifacts cemented in the crust?"



The Sandia Cave discoveries, along with the finds made at Hueyatla (Section 5.4.4), Calico (Section 3.8.3), and Toca da Esperanza (Section 3.8.4), strongly suggest a human presence over 200,000 years ago in the Americas. This challenges not only the orthodox time estimate for the entry of *Homo sapiens* into North America (12,000 years ago) but also the whole picture of human evolution, which has *Homo sapiens* arising from *Homo erectus* in Africa about 100,000 years ago.

## 5.5 Neolithic Tools From The Tertiary Auriferous Gravels Of California

In 1849, gold was discovered in the gravels of ancient riverbeds on the slopes of the Sierra Nevada Mountains in central California, drawing hordes of rowdy adventurers to places like Brandy City, Last Chance, Lost Camp, You Bet, and Poker Flat. At first, solitary miners panned for flakes and nuggets in the gravels that had found their way into the present stream beds. But soon gold-mining companies brought more extensive resources into play, some sinking shafts into mountainsides, following the gravel deposits wherever they led, while others washed the auriferous (gold-bearing) gravels from hillsides with high pressure jets of water.

Occasionally, the miners would find stone artifacts, and, more rarely, human fossils (Section 6.2.6). Altogether, miners found hundreds of stone implements—mortars, pestles, platters, grinders, and so forth. Many of the specimens found their way into the collection of Mr. C. D. Voy, a part-time employee of the California Geological Survey. Voy's collection eventually came into the possession of the University of California, and the most significant artifacts were reported to the scientific community by J. D. Whitney, then the state geologist of California.

The finds occurred in three situations: (1) in surface deposits of gravel; (2) in gravels washed from hillsides by hydraulic mining; and (3) in underground deposits of gravel reached by mine shafts and tunnels. The artifacts from surface deposits and hydraulic mining were of doubtful age, but the artifacts from deep mine shafts and tunnels could be more securely dated because the gold-bearing gravels lay underneath thick layers of volcanic material.

### 5.5.1 The Age of the Auriferous Gravels

J. D. Whitney thought the geological evidence indicated the auriferous gravels, and the sophisticated stone tools found in them, were at least Pliocene in age. But modern geologists think some of the gravel deposits, which lie beneath volcanic formations, are much older.

According to Paul C. Bateman and Clyde Wahrhaftig (1966), R. N. Norris (1976), and William B. Clark (1979), the majority of the gold-bearing gravels were laid down in stream channels during the Eocene and Early Oligocene. These are called the prevolcanic auriferous gravels. During the Oligocene, Miocene, and Pliocene, volcanic activity in the same region covered some of the auriferous gravels with deposits of rhyolite, andesite, and latite.

In particular, widespread andesitic mudflows and conglomerates were deposited during the Miocene. These attained a considerable thickness, varying from more than 3,000 feet along the crest of the Sierras to 500 feet in the foothills. The volcanic flows were so extensive that they almost completely buried the bedrock landscape of the northern Sierra Nevada mountain region.

Although intense at times, the volcanic activity in the Sierra Nevada Mountains was not continuous, allowing rivers to carve new channels and canyons. These rivers often redistributed old gravels laid down in the Eocene and Early Oligocene periods. So below the volcanic formations, the most recent of which are Early Pleistocene (Jenkins 1970, p. 25), there can now be found auriferous gravel deposits that were laid down in stream beds during the Eocene, Oligocene, Miocene, and Pliocene periods. Over the course of time, rivers carved deep channels up to a couple of thousand feet below the level of the prevolcanic gravels. This allowed Gold Rush miners to reach the auriferous gravels by digging horizontal tunnels into the sides of the channels. The advanced stone tools found in these tunnels could be from Eocene to Pliocene in age.

#### 5.5.2 Discoveries of Doubtful Age

Before discussing the most significant discoveries, the ones made in mines extending into gold-bearing gravels beneath ancient lava flows, let us briefly examine why it is not possible to attribute any great age to the artifacts found elsewhere in the gold mining region.

Some stone implements were found in the sluices of the hydraulic mines, where powerful jets of water were directed at entire hillsides. William H. Holmes, of the Smithsonian Institution, pointed out that recently abandoned Indian villages were often found on the slopes above the open mines and that it was quite possible that modern stone implements were washed into the Tertiary gravels below (1899, p. 445).

Other artifacts were found deep within surface deposits of Tertiary gravel. For example, at Gold Springs, a little west of Columbia, Mr. Lot Cannell stated that he discovered stone mortars and platters along with the bones and teeth of mastodons in gold-bearing gravels, approximately 90 feet below the surface (Whitney 1880, p. 262). At first glance, this discovery, so deep in the gravel, promises extreme antiquity. After all, mastodons existed in North America as far back as the Miocene.

But investigators such as William J. Sinclair determined that implements found deep in such deposits might be recent. Sinclair (1908, p. 112) wrote: "The underlying Carboniferous limestone has been eroded into fantastic shapes by percolating waters during or after the deposition of the auriferous wash. ... In a limestone region with underground drainage, it is quite apparent that implements of human manufacture which happened to be scattered on the surface would stand an excellent chance of reaching deeper levels through the many sink holes affording drainage ways to surface waters. "

So although the Gold Springs gravel itself might have been Tertiary in age it is possible, in light of Sinclair's observations about sinkholes, that the implements found deep in the gravels might have worked their way down from the surface in relatively recent times. Therefore, all we can safely conclude is that the stone implements found at Gold Springs might be anywhere from several million to several thousand years old. The same is true of discoveries made at Kincaid Flat, Oregon Bar, and several other localities where the gold-bearing gravels were not capped by volcanic deposits of known age.

### 5.5.3 Tuolumne Table Mountain

Finds from mine shafts can be dated more securely than those from hydraulic mines and surface deposits of gravel. Many shafts were sunk at Table Mountain in Tuolumne County. Whitney and others reported that miners found stone tools and human bones (Section 6.2.6) there. In the gold-bearing gravels sealed beneath thick layers of a volcanic material called latite. In many cases, the mine shafts extended horizontally for hundreds of feet beneath the latite cap at a depth of over 100 feet below the latite (Figure 5.12).

Tuolumne Table Mountain was created by a massive latite flow which moved down the Cataract Channel, a Miocene course of the Stanislaus River, forcing the river into a new channel. According to R. M. Norris (1976, p. 43), the latite lava cap is 9 million years old and is 300 feet thick in the vicinity of the town of Sonora. Slemmons (1966, p. 200) gave dates for the latite cap and underlying strata at Tuolumne Table Mountain (Table 5.3).

Discoveries from the auriferous gravels just above the bedrock are probably 33.2 to 55 million years old, but discoveries from auriferous gravels whose positions are not specified may be anywhere from 9 to 55 million years old.

Figure 5.12. Side view of Table Mountain, Tuolumne County, California, showing mines penetrating into Tertiary gravel deposits beneath the lava cap, shown in black (Holmes 1899, p. 450).

TABLE 5.3

Age of Strata at Tuolumne Table Mountain

Age (millions of years)

Description of Formation

9.0

Table Mountain latite member

9.0-21.1

Andesitic tuffs, breccias, and sediments

21.1-33.2

Rhyolite tuffs

33.2-55.0

Prevolcanic auriferous gravels

>55.0

Bedrock

#### 5.5.4 Dr. Snell's Collection

The more important discoveries from Tuolumne Table Mountain add up to a considerable weight of evidence. Whitney personally examined a collection of Tuolumne Table Mountain artifacts belonging to Dr. Perez Snell, of Sonora, California. About this collection of artifacts, Whitney (1880, p. 264) stated: "In Dr. Snell's collection .. there were several objects which were marked as having come 'from under Table Mountain.'" C. D. Voy said: "Among them was a piece of stone apparently designed as a handle for a bow. It was made of silicious slate and had little notches at the end, which appear to have been fashioned for tying the stone to the bow. There were also one or two spear heads, from six to eight inches long, and several scoops or ladles, with well shaped handles" (Whitney 1880, p. 264).

As can be seen from Whitney's statements about Dr. Snell's collection, there is not much in the way of direct testimony about the discoverers and original stratigraphic positions of the implements. There was, however, one exception. "This was," wrote Whitney (1880, p. 264), "a stone muller, or some kind of utensil which had apparently been used for grinding. It was carefully examined by the writer, and recognized as unquestionably of artificial origin. In regard to this implement Dr. Snell informed the writer that he took it with his own hands from a car-load of 'dirt' coming out from under Table Mountain." A human jaw, inspected by Whitney, was also present in the collection of Dr. Snell. The jaw was given to Dr. Snell by miners, who claimed that the jaw had come from the gravels beneath the basalt cap at Table Mountain in Tuolumne County (Becker 1891, p. 193).

#### 5.5.5 The Walton Mortar

A better-documented discovery from Tuolumne Table Mountain was made by Mr. Albert G. Walton, one of the owners of the Valentine claim. Walton found a stone mortar, 15 inches in

diameter, in gold-bearing gravels 180 feet below the surface and also beneath the latite cap. Significantly, the find of the mortar occurred in a "drift," a mine passageway leading horizontally from the bottom of the main vertical shaft of the Valentine mine. This tends to rule out the possibility that the mortar might have fallen in from above. Furthermore, the vertical shaft "was boarded up to the top, so that nothing could have fallen in from the surface during the working under ground" (Whitney 1880, p. 265). In fact, Walton, who found the mortar, was the carpenter responsible for timbering the shaft. A piece of a fossil human skull was also recovered from the Valentine mine (Section

6.2.6.3).

William J. Sinclair (1908, p. 115) later claimed that many of the drift tunnels from other mines near the Valentine shaft were connected. Sinclair granted that the Valentine vertical shaft may have been, as Whitney stated, securely boarded up to the top, so that nothing could fall in from the surface. But he proposed that objects still could have found their way into the Valentine underground tunnel from some other tunnels. Sinclair did not, however, offer any specific evidence that any tunnels were connected with the Valentine drift tunnels at the time the discoveries were made. In fact, Sinclair admitted that when he visited the area in 1902 he was not even able to find the Valentine shaft. It appears that Sinclair simply used his vague retrospective conjectures about possible invalidating circumstances to dismiss Walton's report of his discovery. Operating in this manner, one could find good reason to dismiss any paleoanthropological discovery ever made.

Another author suggested that prehistoric miners, perhaps from the known culture centers in Mexico or Central America, left the stone artifacts in the course of gold-mining operations conducted in California (Southall 1882, p. 197). We are, however, aware of only a single report of a mine existing before the California Gold Rush of the 1850s. This one mine (Southall 1882, p. 198) is insufficient to explain mortars and other implements found in many separated locations. The proposal that there may have been numerous mines that were collapsed and therefore escaped detection is highly improbable. The Gold Rush miners, being expert in such matters, would most likely have detected them, especially since collapsed mine shafts would have posed a threat to their lives in the form of cave-ins.

Some critics have contended that the mortars were carried by Indians into mines dug during the Gold Rush days. But the Indians of those times did not possess portable mortars (Section 5.5.13). And even if they did possess portable mortars, it is unlikely they would have carried them into the mines. Mortars were generally used for the grinding of raw acorns, a laborious, time-consuming task not likely to have been performed in the cramped, dark, dangerous confines of a working mine shaft.

The mortars found in the mines do resemble those used by some California Indians in the recent past. But the mortars from the mines also resemble those made by primitive people in other parts of the world, at various times in the past. In fact, it is likely that any human beings, living anywhere and at any time, would, when faced with the task of making grinding tools, come up with tools

very much like the mortars and pestles from the mines. Therefore, the resemblance of the mortars found in the mines to those used by California Indians in recent times is not proof the mortars from the mines are also recent. They could have been made, as the evidence suggests, millions of years ago.

#### 5.5.6 The Carvin Hatchet

Another find at Tuolumne Table Mountain was reported by James Carvin in 1871: "This is to certify that I, the undersigned, did about the year 1858, dig out of some mining claims known as the Stanislaus Company, situated in Table Mountain, Tuolumne County, opposite O'Byrn's Ferry, on the Stanislaus River, a stone hatchet ... with a hole through it for a handle, near the middle. Its size was four inches across the edge, and length about six inches. It had evidently been made by human hands. The above relic was found from sixty to seventy-five feet from the surface in gravel, under the basalt, and about 300 feet from the mouth of the tunnel. There were also some mortars found, at about the same time and place" (Whitney 1880. pp. 274-275).

#### 5.5.7 The Stevens Stone Bead

In 1870, Oliver W. Stevens submitted the following notarized affidavit: "This is to certify that I, the undersigned, did about the year 1853, visit the Sonora Tunnel, situated at and in Table Mountain, about one half a mile north and west of Shaw's Flat, and at that time there was a car-load of auriferous gravel coming out of said Sonora Tunnel. And I, the undersigned, did pick out of said gravel (which came from under the basalt and out of the tunnel about two hundred feet in, at the depth of about one hundred and twenty-five feet) a mastodon tooth in a good state of preservation, which afterwards was partly broken, in the hollow of which was sulphuret of iron [iron sulfide, or pyrite]. And at the same time I found with it some relic that resembled a large stone bead, made perhaps of alabaster, about one and a half inches long, and about one and one fourth inches in diameter, with a hole through it one fourth of an inch in size, which no doubt had been used, some time, to put a string through. I also certify that I gave the specimens to C D. Voy, about the year 1864, to put in his collection" (Whitney 1880. p. 266). Voy visited the site and confirmed the geological details.

Whitney (1880. p. 266) later wrote: "The bead was carefully examined by the writer. It is correctly described above, except that the material of which it is made is white marble, not alabaster. It had evidently been much handled, and unfortunately cleaned of the incrusting material; but quite distinct traces of a former filling of the hole with sulphuret of iron were still visible. The mastodon tooth bore, also, as stated by Mr. Stevens, evident marks of an incrustation of the same mineral; and it may be added that several of the bones, which are said to have come from under Table Mountain, have been found to have more or less abundant crystallizations of pyrites in the cellular portions. There can be no question of the artificial character of the so-called bead. It is regularly and symmetrically shaped, and looks as if intended for an ornament. "

William J. Sinclair, of the University of California, objected (1908. p. 115): 'Little dependence, as an evidence of antiquity, can be placed on the presence of pyrite in the hollow of the marble bead

reported by Whitney from the gravels of this mine. The rapidity with which secondary pyrite forms is well known. "

But the real significance of Whitney's remark about the presence of pyrite in the hollow of the bead is not that it proves, in and of itself, great age. Instead, it confirms that the bead examined by Whitney was the same one described by Stevens. And Stevens testified in his affidavit that he personally found the bead in a carload of rock and gravel from deep within the mine, below the latite cap of Table Mountain. In the absence of more exact information, this means that the bead would be at least 9 million years old and perhaps as much as 55 million years old.

But Sinclair (1908. pp. 115-116) objected: "If this degree of association with the gravel is to be accepted as proof of antiquity, we would be Justified in supposing that any object of recent manufacture acquired under similar circumstances was as old as the gravels."

Of course, if one were convinced that an object was "of recent manufacture," then no circumstance of acquisition whatsoever—even the most perfect—would compel one to suppose it was as old as the gravels. But if the object provided no clear evidence for its date of manufacture, then circumstances of acquisition like those encountered in the case of the marble bead would argue strongly in favor of an age equivalent to that of the Tertiary gravels.

So here we have a typical example of the unfair treatment of anomalous evidence. Sinclair attempted to raise unreasonable doubt and suspicion about the origin of the white marble bead, even though the initial report that it came from Tertiary gold-bearing deposits was credible. But in the cases of many accepted discoveries, the circumstances of discovery are similar to that of the marble bead.

For example, at Border Cave in South Africa, *Homo sapiens sapiens* fossils were taken from piles of rock excavated from mines years earlier. The fossils were then assigned dates of about 100,000 years, principally because of their association with certain kinds of rock. The scientists who assigned the dates wrote: "Border Cave 1 and 2 comprise an adult male cranial vault and a partial adult female mandible respectively. These fragments were all displaced from their original contexts. In 1940 during the removal of 'fertilizer' from Horton's Pit. . . . Cooke et al. claimed that the character of the soil adhesions in small interstices of the skull was only matched by a distinctive 'chocolate coloured layer' corresponding to the base of our [layer] IGBS.LR" (Beaumont et al. 1978, p. 414).

The Heidelberg Jaw was discovered by workmen in a gravel pit, with no scientist present, and was assigned a Middle Pleistocene date. Furthermore, most African hominid fossils, including those of Lucy (*Australopithecus afarensis*), were discovered on the surface and were assigned specific dates because of their loose association with certain exposed strata. In Java, also, most of the *Homo erectus* discoveries occurred on the surface, and, in addition, they were found by paid native collectors, who shipped the fossils in crates to distant scientists for study.

If Sinclair's strict standards were to be applied to these finds, they also should have to be rejected as evidence for hominids of any particular antiquity. In other words, most of the evidence upon which the current picture of human evolution is based would have to be thrown out.

And this takes us back to the central theme of this book. We are not promoting any particular discovery or set of discoveries. Rather, we are looking at the entire body of evidence relating to human origins and antiquity and asking for consistent application of standards for acceptance and rejection of evidence. Our historical survey has led us to conclude that up to now scientists have not impartially applied such standards. This raises some legitimate doubts about the trustworthiness of the evolutionary lineages that have been erected upon such a shaky evidential foundation.

Indeed, we find that when all the available evidence is considered impartially, an evolutionary picture of human origins falls to emerge. On the one hand, if we apply the tactic of extreme skepticism equally to all available evidence, we wind up with such an insufficiency of facts that it becomes next to impossible to say anything at all about human origins. On the other hand, if we take a more liberal, yet evenhanded, approach to the totality of evidence, we are confronted with facts demonstrative of a human presence in remote geological ages, as far back as the Eocene, and even further.

#### 5.5.8 The Pierce Mortar

In 1870, Llewellyn Pierce gave the following written testimony (Whitney 1880, p. 266): "This is to certify that I, the undersigned, have this day given to Mr. C. D. Vay, to be preserved in his collection of ancient stone relics, a certain stone mortar, which has evidently been made by human hands, which was dug up by me, about the year 1862, under Table Mountain, in gravel, at a depth of about

200 feet from the surface, under the basalt, which was over sixty feet deep, and about 1,800 feet in from the mouth of the tunnel. Found in the claim known as the Boston Tunnel Company." Whitney (1880, p. 266) said the mortar was 31.5 inches in circumference. Vay visited the site and saw the approximate place where the object was found (Whitney 1880, p. 267).

William J. Sinclair interviewed Llewellyn Pierce in 1902, a good 40 years after the original discovery was made. Sinclair (1908, p. 116) wrote: 'The mortar from the Boston claim was [according to Pierce] as large as a sixteen-gallon milk bucket and would weigh about seventy-five pounds. It was found in hard gravel under the cement, and was taken out by Mr. Pierce while he was sitting on a candle box, breasting [sic] out gravel. . . . The mortar preserved in Vay's collection is an oval boulder of hornblende andesite into which a hole has been worked, about four and three-quarters inches in greatest width, and three and three-quarters inches deep, dimensions to which those of a sixteen-gallon bucket must be regarded as a rather liberal approximation." This last sarcastic remark appears calculated to cast doubt on Pierce's testimony. But it should be recalled that the entire mortar was 31.5 inches in circumference, which is close to the size of the mouth of a tall sixteen gallon milk bucket commonly used in dairies.



Sinclair (1908, p. 117) added: "The deep gravels in the bottom of the Table Mountain channels, tapped by the Boston Tunnel and other workings, are largely inaccessible, but so far as known are not volcanic. The incongruity of associating an andesitic mortar. . with the old prevolcanic gravels is at once apparent. The andesitic sands and gravels of Table Mountain lie above the auriferous gravel channels in which these relics were supposed to occur." If Sinclair is correct that the mortar was found in the prevolcanic gravel, then it would be 33-55 million years old (Table 5.3, p. 371).

But what was the source of the andesite from which the Pierce mortar was made? The prevolcanic auriferous gravels contained boulders of different kinds of rock formed in previous ages, so who can say that there were no isolated andesite boulders in the ancient river channels? Furthermore, there may have been deposits of andesite as old as the prevolcanic gravels in other nearby areas of the Sierra Nevada mountains, and therefore andesite boulders or finished andesite mortars could have been transported by human agency to the region of Tuolumne Table Mountain.

In fact, Durrell (1966, pp. 187-189) reported four nearby sites, all north of Tuolumne Table Mountain, which are just as old as the prevolcanic auriferous gravels and contain deposits of hornblende andesite. These are the Wheatland Formation, at 100 miles; the Reeds Creek Formation, at 100 miles; Oroville Table Mountain, at 140 miles; and the Lovejoy Formation, at 200 miles.

Good portable andesite mortars might have been a valuable trade item, and might have been transported good distances by rafts or boats, or even by foot. In a study of California Indians, R. F. Heizer and M. A. Whipple reported the presence of basalt mortars in Marin County, north of San Francisco. The mortars ranged in weight from 20 to 125 pounds. Heizer and Whipple (1951, p. 298) stated: "Each of these pieces must have been carried to the spot from not less than 25 miles away, no mean task for the slightly built, barefoot Indians. Stone is completely lacking in the alluvial deposits of the valley floodplain of the Sacramento and San Joaquin delta region." According to Heizer and Whipple, such manufactured objects were frequently traded by Indians for unfinished raw materials.

Hence none of Sinclair's arguments are strong enough to invalidate the testimony indicating that the Pierce mortar was deposited in the Table Mountain gravels during Tertiary times. The general tone of Sinclair's paper indicates that he was strongly biased against the possibility of toolmaking humans living in the Tertiary, and that he was searching for any excuse to discredit these discoveries.

According to Sinclair (1908, pp. 116-117), Pierce found another artifact along with the mortar: "The writer was shown a small oval tablet of dark colored slate with a melon and leaf carved in bas-relief. Mr. Pierce claimed to have found this in the same gravels as the mortar, and, he thought, probably at the same time. This tablet shows no signs of wear by gravel. The scratches are all recent defacements. The carving shows very evident traces of a steel knife blade and was conceived and executed by an artist of considerable ability.'

Sinclair stated that this carving could not really have been as old as the Tertiary gravels in which it was discovered. It appears that Sinclair brought the carved tablet into his discussion simply for the purpose of distracting attention from the mortar reported by Pierce. Such tactics are often encountered in critiques of anomalous evidence.

Sinclair provided no account of the exact features of the slate tablet that led him to conclude it had been carved with a steel blade. Therefore, he may have been wrong about the type of implement that was used. Furthermore, the level of human technological achievements in the Tertiary was then, and still is, very much an open question. If the slate tablet was in fact discovered, with the mortar, in prevolcanic gravels deep under the latite cap of Tuolumne Table Mountain, beneath a hard layer of "cement," and if the tablet does in fact display definite signs of carving by a steel blade, then one would be justified in concluding that human beings of a relatively high level of cultural achievement were present between 33 million and 55 million years ago. In fact, the carved tablet could be taken as proof that the artisan used steel tools. Sinclair also said that the tablet showed no signs of wear by gravel. But perhaps it was not moved very far by the action of the Tertiary river and therefore remained unabraded. Or perhaps the tablet could have been dropped into a gravel deposit of a dry channel of a shifting stream. This would also explain why it showed no signs of excessive wear.

#### 5.5.9 The Neale Discoveries

On August 2, 1890, J. H. Neale signed the following statement about discoveries made by him: "In 1877 Mr. J. H. Neale was superintendent of the Montezuma Tunnel Company, and ran the Montezuma tunnel into the gravel underlying the lava of Table Mountain, Tuolumne County. ... At a distance of between 1400 and 1500 feet from the mouth of the tunnel, or of between 200 and 300 feet beyond the edge of the solid lava, Mr. Neale saw several spear-heads, of some dark rock and nearly one foot in length. On exploring further, he himself found a small mortar three or four inches in diameter and of irregular shape. This was discovered within a foot or two of the spear-heads. He then found a large well-formed pestle, now the property of Dr. R. I. Bromley, and near by a large and very regular mortar, also at present the property of Dr. Bromley." This last mortar and pestle are shown in Figure 5.13.

Neale's affidavit continued: "All of these relics were found the same afternoon, and were all within a few feet of one another and close to the bed-rock, perhaps within a foot of it. Mr. Neale declares that it is utterly impossible that these relics can have reached the position in which they were found excepting at the time the gravel was deposited, and before the lava cap formed. There was not the slightest trace of any disturbance of the mass or of any natural fissure into it by which access could have been obtained either there or in the neighborhood" (Sinclair 1908, pp. 117-118).

Figure 5.13. This mortar and pestle (Holmes 1899, plate XIII) were found by J. H. Neale, who removed them from a mine tunnel penetrating Tertiary deposits (33-55 million years old) under Table Mountain, Tuolumne County, California.

The position of the artifacts in gravel "close to the bed-rock" at Tuolumne Table Mountain indicates they were 33-55 million years old. In 1898, William H. Holmes decided to interview Neale and in 1899 published the following summary of Neale's testimony: "One of the miners coming out to lunch at noon brought with him to the superintendent's office a stone mortar and a broken pestle which he said had been dug up in the deepest part of the tunnel, some 1500 feet from the mouth of the mine. Mr. Neale advised him on returning to work to look out for other utensils in the same place, and agreeable to his expectations two others were secured, a small ovoid mortar, 5 or 6 inches in diameter, and a flattish mortar or dish, 7 or 8 inches in diameter. These have since been lost to sight. On another occasion a lot of obsidian blades, or spear-heads, eleven in number and averaging 10 inches in length, were brought to him by workmen from the mine. They had been found in what Mr. Neale called a 'side channel,' that is, the bed of a branch of the main Tertiary stream about a thousand feet in from the mouth of the tunnel. and 200 or 300 feet vertically from the surface of the mountain slope.

Four or five of the specimens were given to Mr. C. D. Voy, the collector. . . . Some had one notch, some had two notches, and others were plain leaf-shaped blades" (Sinclair 1908, pp. 118-119: Holmes 1899, pp. 452- 453).

As can be seen, there are significant differences between the account given by Holmes and the earlier affidavit of Neale. In particular, Holmes (1899, p. 453) said: "In his conversation with me he did not claim to have been in the mine when the finds were made." This might be interpreted to mean that Neale had lied in his original statement. Here, however, the following points need to be carefully considered. The just-quoted passages from Holmes are not the words of Neale but of Holmes (1899, p. 452), who said: "His [Neale's] statements, written down in my notebook during and immediately following the interview, were to the following effect." It is not clear what liberties Holmes may have taken in his representation of Neale's conversations with him. It is interesting that Holmes did not say that Neale denied that he entered the mine; Holmes merely said he did not positively state that he did enter, which leaves open the possibility that perhaps he did. It is thus debatable whether one should place more confidence in Holmes's indirect summary of Neale's words than in Neale's own notarized affidavit signed by him. Significantly, we have no confirmation from Neale himself that Holmes's version of their conversation was correct.

That Holmes may have been mistaken is certainly indicated by a subsequent interview with Neale conducted by William J. Sinclair in 1902. Summarizing Neale's remarks, Sinclair (1908, p. 119) wrote: "A certain miner (Joe), working on the day shift in the Montezuma Tunnel, brought out a stone dish or platter about two inches thick. Joe was advised to look for more in the same place. At the time, they were working in caving ground. Mr. Neale went on the night shift and in excavating to set a timber, 'hooked up' one of the obsidian spear points. With the exception of the one brought out by Joe, all the implements were found personally by Mr. Neale, at one time, in a space about six feet in diameter on the shore of the channel. The implements were in gravel close to the bed-rock and were mixed with a substance like charcoal." When all the testimony is duly weighed, it appears that Neale himself did enter the mine and find stone implements in place in the gravel.

As in the case of the Pierce discoveries, Sinclair (1908, p. 119) observed "there is involved the anomaly of two late volcanic rock types, andesite and obsidian, occurring in the pre-volcanic gravels." Holmes raised the same objection. He asserted that according to geologists andesite "is not found in the formations of the particular region" until a period long after the gold-bearing gravels were deposited (Holmes 1899, p. 426). He added: "The objects being generally large, it is not to be supposed for a moment that they could have been brought from a distance" (Holmes 1899, p. 426). About obsidian, he stated it "is known only as a late product, having its origin in the most recent flows of the Sierra" (Holmes 1899, p. 426).

Concerning the andesite, we have already noted that there occur in the same region andesite deposits of the same age as the pre-volcanic gravels at Tuolumne Table Mountain. Furthermore, the fact that andesite artifacts were found in more than one mine shaft under Tuolumne Table Mountain strengthens the supposition that boulders of andesite may have been present in the rivers that deposited the pre-volcanic gravels. Furthermore, andesite mortars, although heavy, may have been transported by boat or raft, or even by foot.

As far as the obsidian spearheads are concerned, it is well established that Neolithic cultures all over the world have traded such objects over extended areas. Thus even if no raw obsidian was locally available, that would pose no obstacle to the presence of finished obsidian blades in the lowermost pre-volcanic gravels at Tuolumne Table Mountain, which are 33-55 million years old.

In countering Neale's direct testimony that he found stone tools in pre-volcanic gravels at Tuolumne Table Mountain, Holmes and Sinclair could, in the end, raise only the vague suspicion that the objects had somehow been recently introduced into the Montezuma mine. Sinclair (1908, p. 120) stated: "There was every indication of a former Indian camp site in this vicinity. Half an hour's search resulted in the discovery of a pestle and a flat stone muller, a few yards north of the mine buildings. Similar discoveries were reported by Holmes. South of the tunnel, a large permanent mortar was found. The material of this mortar is lime from the cliff above. It is quite possible that the implements mentioned by Mr. Neale came from this Indian camp."

In similar fashion, Holmes (1899, pp. 451-452) questioned: "Is it not more reasonable to suppose that some of the typical implements of the Indians living at the mouth of Montezuma mine should have been carried in for one purpose or another, embedded in the gravels, and afterwards dug up and carried out to the superintendent than that the implements of a Tertiary race should have been left in the bed of a Tertiary torrent to be brought out as good as new, after the lapse of vast periods of time, into the camp of a modern community using identical forms?" But the reasonableness of Holmes's supposition is questionable. There is, in fact, ample reason to believe that the implements found by Neale were not carried into the Montezuma shaft but were deposited in Tertiary times. First of all, in the passage quoted above, Sinclair referred to a large, immovable, permanent mortar found near the mine entrance, but the mortars found by Neale in the mine were portable mortars. Also, mortars much like those found in the California mines have been discovered at various sites around the world, including Jarmo and Beidha in the Middle East.

This shows that such stone mortars are likely to have been made by any people, living at any time or place.

Furthermore, it has been shown in Africa that modern tribes use the same kind of cobble implements found in the lower levels of Olduvai Gorge. The similarity of the Olduvai Implements to modern ones in use in the same region did not prevent acceptance of their Early Pleistocene antiquity. Therefore, the similarity of Implements found in the pre-volcanic gravels at Tuolumne Table Mountain to those found on the surface in the same region should not be taken as sufficient cause to deny their great age.

About the obsidian spearheads found by Neale, Holmes (1899, p. 453) reported: "Desiring to find out more concerning these objects . . . he [Neale] showed them to the Indians who chanced to be present, but, strangely enough, they expressed great fear of them, refusing to touch them or even speak about them; but finally, when asked whether they had any idea whence they came, said they had seen such implements far away in the mountains, but declined to speak of the place further or to undertake to procure others."

Holmes (1899, p. 453) then stated: "I was not surprised when a few days later it was learned that obsidian blades of identical pattern were now and then found with Digger Indian remains in the burial pits of the region. The inference to be drawn from these facts is that the implements brought to Mr. Neale had been obtained from one of the burial places in the vicinity by the miners.' Here we must discount Holmes's inference that the implements were brought to Neale by miners. We have established that Neale's statements in his original affidavit, as confirmed by his later statements recorded by Sinclair, are deserving of credence, and these statements show quite clearly that Neale himself found the implements in the gravels. Holmes (1899, p. 453) then stated: "How the eleven large spearheads got into the mine, or whether they came from the mine at all, are queries that I shall not assume to answer."

Using Holmes's methods, it is clear that one could discredit any paleoanthropological discovery ever made: one could simply refuse to believe the evidence as reported, and put forward all kinds of vague alternative explanations, without answering legitimate questions about them. In the case under consideration, there is credible testimony by a reliable observer, Neale, that the implements were in fact found in the mine: therefore Holmes should not have failed to assume the burden of answering the queries he raised. Indeed, his failure to do

so raises justifiable doubt about the value of his queries.

Holmes (1899, p. 453) further wrote about the obsidian implements: "that they came from the bed of a Tertiary torrent seems highly improbable; for how could a cache of eleven, slender, leaf-like implements remain unscattered under these conditions: how could fragile glass blades stand the crushing and grinding of a torrent bed; or how could so large a number of brittle blades remain unbroken under the pick of the miner working in a dark tunnel?" As often as such objections are raised, we can answer, first of all, that one can imagine many circumstances in which a cache of implements might have remained undamaged in the bed of a Tertiary stream. Just for example, let

us suppose that in Tertiary times a trading party, while crossing or navigating a stream, lost a number of obsidian blades securely wrapped in hide or cloth. The package of obsidian blades may have been rather quickly covered by gravel in a deep hole in the stream bed and remained there relatively undamaged until recovered tens of millions of years later. As to how the implements could have remained unbroken as they were being uncovered, answering that question poses no insuperable difficulties. As soon as Neale became aware of the presence of the blades, he could have, and apparently did, exercise sufficient caution to preserve the obsidian implements intact. Maybe he even broke some of them.

In a paper read before the American Geological Society and published in its Journal, geologist George F. Becker (1891, pp. 192-193) said: "It would have been more satisfactory to me individually if I had myself dug out these implements, but I am unable to discover any reason why Mr. Neale's statement is not exactly as good evidence to the rest of the world as my own would be. He was as competent as I to detect any fissure from the surface or any ancient workings, which the miner recognizes instantly and dreads profoundly. Some one may possibly suggest that Mr. Neale's workmen 'planted' the implements, but no one familiar with mining will entertain such a suggestion for a moment. . . . The auriferous gravel is hard picking, in large part it requires blasting, and even a very incompetent supervisor could not possibly be deceived in this way. . . . In short, there is, in my opinion, no escape from the conclusion that the implements mentioned in Mr. Neale's statement actually occurred near the bottom of the gravels, and that they were deposited where they were found at the same time with the adjoining pebbles and matrix."

#### 5.5.10 The King Pestle

Although the tools discussed so far were found by miners, there is one case of a stone tool being found in place by a scientist. In 1891, George F. Becker told the American Geological Society that in the spring of 1869, Clarence King, director of the Survey of the Fortieth Parallel, and a respected geologist, was conducting research at Tuolumne Table Mountain. Becker (1891, pp. 193-194) stated: "At one point, close to the high bluff of basalt capping, a recent wash had swept away all talus and exposed the underlying compact, hard, auriferous gravel beds, which were beyond all question in place. In examining the exposure for fossils he [King] observed the fractured end of what appeared to be a cylindrical mass of stone. The mass he forced out of its place with considerable difficulty on account of the hardness of the gravel in which it was tightly wedged. It left behind a perfect cast of itself in the matrix and proved to be part of a polished stone implement, no doubt a pestle [Figure 5.14J]." The facts recorded by Becker tend to rule out the phenomenon of secondary deposition—

i. e., that the pestle had fallen from a higher, more recent layer and become recemented in the lower, older layer. Becker (1891, p. 194) added: "Mr. King is perfectly sure this implement was in place and that it formed an original part of the gravels in which he found it. It is difficult to imagine a more satisfactory evidence than this of the occurrence of implements in the auriferous, pre-glacial, sub-basaltic gravels." From this description and the modern geological dating of the Table Mountain strata, it is apparent that the object was over 9 million years old.

Figure 5.14. Left: Broken stone pestle found by Clarence King of the U.S. Geological Survey (Holmes 1899, p. 455). King personally extracted it from Tertiary deposits at Table Mountain, Tuolumne County, California. Right: A modern Indian pestle.

Even Holmes (1899, p. 453) had to admit that the King pestle, which was placed in the collection of the Smithsonian Institution, "may not be challenged with impunity." Holmes searched the site very carefully and noted the presence of some modern Indian mealing stones, but nothing else. He stated: "I tried to learn whether it was possible that one of these objects could have become embedded in the exposed tuff deposits in recent or comparatively recent times, for such embedding sometimes results from resetting or recementing of loose materials, but no definite result was reached" (Holmes 1899, p. 454). One may rest assured that if Holmes had found the slightest evidence of recementing, he would have seized the opportunity to cast suspicion upon the pestle discovered by King.

Unable, however, to find anything to discredit the report, Holmes (1899, p. 454) was reduced to wondering "that Mr. King failed to publish it—that he failed to give to the world what could well claim to be the most important observation ever made by a geologist bearing upon the history of the human race, leaving it to come out through the agency of Dr. Becker, twenty-five years later." But Becker (1891, p. 194) noted in his report: "I have submitted this statement of his discovery to Mr. King, who pronounces it correct."

Sinclair (1908, pp. 113-114) nevertheless attempted to raise doubts about the King pestle. He stated: "As a geologist, Mr. King was a reliable observer and able to determine whether or not the implement was in place and formed an integral part of the mass of gravel in which it was imbedded. Secondary cementation does not seem to have been taken into consideration. On many of the outcrops of andesitic sandstone in the vicinity of this locality, secondary cementation is in progress, indurating the soft sands into a hard rock to the depth of at least an inch. It is unfortunate that the matrix containing the impression of this relic was not preserved. As it is, there is no way of confirming the discovery. We have nothing but the specimen and the published account to work from."

In response to Sinclair's insinuations, the following points may be made. First of all, if as Sinclair himself stated, King was "a reliable observer," it is extremely unlikely, next to impossible, that he did not consider the obvious likelihood of recementing. Second, the original description of the find, recorded by Becker, and attested to by King, stated that at first only the cylindrical broken end of the pestle was visible and that the rest of the pestle was embedded in the hardened gravel. A photograph of the pestle (Holmes 1899, plate XIV), shown three-quarters size, indicates that the pestle was at least four inches long. This means that the pestle was probably embedded at least a couple of inches in the hardened gravel. Sinclair, searching for evidence of secondary cementation, noted that this occurred to a depth of "at least one inch on some of the rock surfaces. The recemented material was of sand. But the pestle found by King was embedded in a hard deposit of auriferous gravel that had only recently been exposed. Sinclair said it was unfortunate that the gravel matrix containing the cast of the pestle was not available for

inspection, thereby implying that perhaps the pestle was embedded in something other than the auriferous gravel. But King's statements, as recorded by Becker, make it clear that the pestle was found in the hard gravel deposits. Even Holmes, it should be remembered, hesitated to affirm that the pestle had been recemented onto the gravel in recent times.

Sinclair asserted that there was no way to confirm the authenticity of the discovery of the King pestle because all we now have is the specimen and the published report! The absurdity of this statement becomes apparent when we consider that specimens and reports of the circumstances of their discovery are all that we have to work from in almost all paleoanthropological discoveries ever made. Using Sinclair's logic, we could assert that there is no way to confirm any of them. For example, *Pithecanthropus erectus* was discovered by Dubois in Java during the 1890s. By 1908, when Sinclair wrote about the King pestle, all that was left of Java man were the specimens, stored in a Dutch museum, and the published reports. Therefore the Java man discovery might also have been judged unconfirmable. But this Sinclair did not do. Why? Favored evidence, it appears, can pass where unfavored evidence cannot. That is one of the main messages of this book. Paleoanthropologists frequently apply very subjective standards in the process of accepting and rejecting evidence.

#### 5.5.11 Finds at San Andreas and Spanish Creek

The next set of reports describes discoveries that were made under intact volcanic layers at places other than under the latite cap of Tuolumne Table Mountain. Whitney (1880, pp. 273-274) described some of these discoveries and their geological setting as follows: 'The fact that human implements had been found in some of the mining claims near San Andreas, in gravel under the volcanic strata, was repeatedly mentioned to the writer by persons living in that vicinity, and Mr. Voy was successful in finding some of the parties personally concerned in these finds, and getting their written testimony in regard to them. . . . Through all the higher southeastern portion of this county [Calaveras] the streams run in deep parallel canons, quite close to each other, and having the ridges between them capped with volcanic overflows, all seeming to form part of the grand lava system which has spread far down the Sierra slope from the vicinity of Silver Mountain. In the vicinity of San Andreas the volcanic accumulations consist of alternating layers of sand, gravel, and volcanic ashes and conglomerates, overlying, as usual in the Sierra, gravel deposits more or less auriferous, the pay gravel being usually quite thin, and the whole series of detrital and volcanic materials reaching a thickness, in places, of from 150 to 200 feet."

Some evidence for the San Andreas discoveries came from R. D. Hubbard and John Showalter, who on January 3, 1871 provided C. D. Voy with the following statement: 'This is to certify that we, the undersigned, proprietors of the Gravel claims known as Marshall & Company's, situated near the town of San Andreas, do know of stone mortars and other stone relics, which had evidently been made by human hands, being found in these claims, about the years 1860 and 1869, under about these different formations:

Feet



TOC \o "1-5" \h \z Coarse gravel..... 5

2. Sand and gravel 100

3. Brown gravel..... 20

4. 'Cement' sand..... 4

5. Bluish volcanic sand 15

6. Pay gravel ^ ^ ^ ^ , 6

Total..... 150

The above [mentioned relics] were found in bed No. 6" (Whitney 1880, p. 274).

According to George Saucedo, a geologist of the California State Division of Mines and Geology, the pay gravels at the Marshall mine are probably Miocene or older (personal communication, 1989). The artifacts found in Bed 6 would, therefore, be in excess of 5 millionyears old.

W. O. Swenson, the justice of the peace who notarized the statement of Hubbard and Showalter, added: "I certify that I have seen one of the above described mortars, taken from said claims, and know the above to be true."

Another set of discoveries was made nearby. Whitney (1880, p. 274) stated: "In Smilow & Company's claim, on Gold Hill, about one mile west of Marshall

& Company's, stone mortars were found at a depth of about one hundred feet in pay gravel. under the volcanic. the fonnation being closely similar to that of the last-mentioned locality [San Andreas]. This find is vouched for by Mr. Smilow himself."

From El Dorado County came a report, by a Mr. Ford, that "near the head of Spanish Creek a perfect mortar and pestle were once found in the gravel beneath the volcanic matter" (Whitney 1880, p. 277).

#### 5.5.12 Discoveries at Cherokee

In 1875, Amos Bowman, a part-time assistant to the Geological Survey of California, told of finds made at Cherokee, a fewmiles north of Oroville. in Butte County: "One of the mortars, found by Mr. R. C. Pulham, of the Spring Valley Mining Company. was taken out of a shaft he dug himself in 1853. and was found, according to his testimony, twelve feet underneath undisturbed strata. . . . About 300 feet east of this shaft Mr. Frederic Eaholtz took out in 1853 a similar mortar at a greater depth. I visited both places withMr. Pulham, and found several mortars still lying around on the top of the blue-gravel bench which is not yet mined away." The blue gravels, in which Pulham and Eaholtz discovered mortars, were "immediately underlying the auriferous gravel formation and the volcanic outflows" near Cherokee (Whitney 1880, p. 278).

Eaholtz gave information of further discoveries at another site near Cherokee. Bowman stated: "he told me further that. in 1858, while engaged with Wilson and Abbott in mining in the southwesterly part of the Sugar Loaf, he found in place, forty feet under the surface, a mortar of the same sort in unbroken blue gravel. This blue gravel nowhere comes to the surface, and It extends with the before-mentioned white and yellow gravel. under the Sugar Loaf. and under the Oroville volcanic mesa. It appeared only on the bottom of this claim. He was picking the blue gravel to pieces with a pick, when he found the mortar, which was a portion of the mass of cemented boulders and sand. He picked it out with his own hands" (Whitney 1880, p. 278). There were similar cases from Trinity and Siskiyou counties (Whitney 1880, p. 278).

George Saucedo of the California Division of Mines and Geology (personal communication, 1989) reported that the blue gravel is older than 23.8 million years. According to a study by R. S. Creely (1965), published in the bulletin of the California Division of Mines and Geology, the blue gravel is Eocene. or over 38 million years old. The implements found within the blue gravel would thus appear to be at least 23 million years old.

#### 5.5.13 Evolutionary Preconceptions of Holmes and Sinclair

In light of the evidence we have presented, it is hard to justify the sustained opposition to the California finds by Holmes and Sinclair. who, as we have seen, were very reluctant to accept them as evidence of humans living in Tertiary times. Let us now review their five principal arguments.

(1) Holmes and Sinclair proposed that the discoveries of stone artifacts may have been the result of trickery by miners. But it is hard to see how or why such practical Jokers could have slipped unseen into dozens of different mines over a distance of 100 miles. depositing numerous stone artifacts over a period of many years, or that so many miners would have assisted persons engaged in such trickery by not reporting them. Presumably. the motive would have been to deceive anthropologists, but this would have been a lot of work (some of the artifacts weighed 30 or more pounds) for what would seem to be an insignificant reward.

(2) Holmes (1899, p. 471) stated that none of the stone mortars showed evidence of unusual age or evidence of "wear and tear that would come from transportation in Tertiary torrents." But one would not expect such simple, durable mortars to show much evidence of age; once buried they could remain undamaged for millions of years. As far as "Tertiary torrents" are concerned, why should we assume that rivers were always torrents during Tertiary times? Perhaps, as in the case of most other rivers within our experience, these rivers sometimes flowed swiftly and fiercely but at other times slowly and calmly. Furthennore, it is possible that stone implements were dropped into the streams at a point very close to the place where they became lodged in the gravels, or perhaps they were dropped on the banks of the streams. In either case, one would not expect to find many signs of "wear and tear."

(3) Were the stone mortars perhaps carried into the mines by Indians living nearby? Holmes (1899. pp. 449-450) suggested this was the case: "the mountain Indians were in those days very numerous about the mining camps. The men were employed to

a considerable extent in the mines, and it is entirely reasonable to suppose that their implements and utensils would at times be carried into the mines, perhaps to prepare or contain food, or perhaps as a natural proceeding with half-nomadic peoples habitually carrying their property about. ”

But Whitney (1880, p. 279) said of the portable mortars found in the mine shafts: “They are not in use at present among the Indians of that part of California where the implement in question is so abundantly found. The Digger Indians seem now, for some unknown reason, to prefer cavities worn in the rock in place, and in these the writer has often seen them crushing their acorns: but never once has he found them using the portable mortar.”

Holmes (1899, p. 447) countered that Whitney “made the mistake of supposing they used only fixed mortars, that is, those worked in the surface of large masses or outcrops of rock. The fact is that portable mortars and grinding stones of diversified form are and have been used by Indians in all parts of California.”

But modern authorities agree with Whitney. Glenn J. Farris, a California state archeologist, wrote to our researcher Steve Bernath: “Generally speaking the Indians of the gold rush period used bedrock mortars rather than the portable cobble mortars. The only instance I know of use of portable mortars in this area was to grind pine seeds into a pine nut butter, but I would see no reason for them to be carried into the mines” (personal communication, April 11, 1985).

In another letter, W. Turrentine Jackson, professor of history at the University of California at Davis, stated: “the Indians rarely transported a mortar for the grinding of acorns because of the heavy weight.” Jackson also contradicted the proposition, voiced by Holmes thirty or forty years after the fact, that Indians remained in the mining region: “During the gold rush era the Indians were driven from the mining region, and they seldom came into contact with the forty-niners. I seriously doubt that any Indians had mortars of a portable nature in the mining areas. Certainly they would not have taken them onto a property while the miners were still operating” (personal communication, March 19, 1985). All in all, Holmes's arguments against the Tertiary age of the stone artifacts from the auriferous gravels are not very convincing.

(4) Holmes and Sinclair were unable to believe that humans of the modern type could have existed millions of years ago. And even if they did exist, their implements could not, they believed, have remained the same from then until now. The implements from the ancient gold-bearing gravels closely resembled those used by Indians in relatively recent times. According to evolutionary principles, the implements should have been much different. This suggested to Holmes, Sinclair, and Hrdlicka that the implements from the gold-bearing gravels were in fact of recent manufacture. But on examining these implements, both the ones from the gold-bearing gravels and those known to have been made by Indians in recent historical times, we see that they are simple artifacts of a kind that would naturally have been manufactured by any Neolithic-type culture anywhere in the world and at any time down through history. For example, stone artifacts from Neolithic sites at Beidha in the Middle East and the Nakura site in East Africa (Figure 5.15) are very much like those known to have been made by California Indians in recent historical times.

According to standard views of human prehistory, the cultures of the ancient Middle East and East Africa have no direct relation to those of the American Indians. This means that the mortars of California and the Middle East, although very similar in appearance, were developed independently. So if different peoples separated by thousands of miles independently developed similar implements, this suggests that different peoples separated by millions of years could have done the same.

(5) A final objection was that the artifacts were generally found by inexperienced persons who could have been deceived by fraud, but George F. Becker, a professional geologist, disagreed. "Now, so far as the detection of a fraud is concerned," said Becker (1891, pp. 192-193), 'a good miner, regularly employed in superintending the workings would be much more competent than the average geological visitor. The superintendent sees day by day every foot of new ground exposed, and it is his business to become thoroughly acquainted with its character, while he is familiar with every device for 'salting' a claim. ... It is therefore an argument in favor of the authenticity of the implements that they have been found by miners.' " Deception by miners was unlikely, especially in the many cases of tools found firmly embedded in the compact gravel.

S. Laing agreed with Becker about the unlikelihood of deception. Laing (1894, p. 387) wrote: 'A conspiracy has been imagined of many hundreds of ignorant miners, living hundreds of miles apart, to hoax scientists, or make a trade of forging implements, which is about as probable as the theory that the paleolithic remains of the Old World were all forged by the devil, and buried in Quaternary strata in order to discredit the Mosaic account of creation.' Regarding forgery, it is significant that no money was ever asked for any one of the artifacts.

Having closely examined the arguments put forward by Sinclair and Holmes, we find it apparent that their positions were based more on prejudice than on sound scientific reasoning. One might ask why Holmes and Sinclair were so determined to discredit Whitney's evidence for the existence of Tertiary humans. The following statement by Holmes (1899, p. 424) provides an essential clue: 'If these forms are really of Tertiary origin, we have here one of the greatest marvels yet encountered by science; and perhaps if Professor Whitney had fully appreciated the story of human evolution as it is understood to-day, he would have hesitated to announce the conclusions formulated, notwithstanding the imposing array of testimony with which he was confronted.' In other words, if the facts do not fit the favored theory, the facts, even an imposing array of them, must go. A more reasonable attitude was taken by Becker (1891, p. 190), who wrote of the implements found in the pre-volcanic auriferous gravels of California: 'If such an association of remains actually occurs, theories must be modified to fit the fact.'

In his reports about the Tertiary discoveries in California, Whitney mentioned evidence from other parts of the world indicating the existence of culturally advanced humans in the Pliocene and Miocene periods. In 1871, according to Whitney (1880, p. 282), Portuguese geologist Carlos

Ribeiro published a report (Section 4.1) that 'cut flints, evidently the work of human hands, have been found in abundance in the Pliocene and Miocene even, of Portugal.' Whitney faulted Charles Lyell for not mentioning Ribeiro's report in his authoritative survey, *The Antiquity of Man*. This is a valid criticism, demonstrating that right from the beginning of the scientific study of human evolution, uncomfortable evidence was simply ignored.

Alfred Russell Wallace, who shares with Darwin the credit for formulating the theory of evolution by natural selection, expressed dismay that evidence for anatomically modern humans existing in the Tertiary tended to be "attacked with all the weapons of doubt, accusation, and ridicule' ' (1887, p. 667).

In a detailed survey of the evidence for the great antiquity of humans in North America, Wallace gave considerable weight to Whitney's record of the discoveries in California of human fossils and stone artifacts from the Tertiary. In light of the incredulity with which the auriferous gravel finds and others like them were received in certain quarters, Wallace (1887, p. 679) advised that "the proper way to treat evidence as to man's antiquity is to place it on record, and admit it provisionally wherever it would be held adequate in the case of other animals; not, as is too often now the case, to ignore it as unworthy of acceptance or subject its discoverers to indiscriminate accusations of being impostors or the victims of impostors."

Wallace, an evolutionist, charged scientists who automatically rejected evidence for the extreme antiquity of anatomically modern humans with playing into the hands of those who can adduce his recent origin and unchangeability as an argument against the descent of man from the lower animals" (Wallace 1887, p. 679). But, humanity's extreme antiquity, as demonstrated by the evidence cited by Wallace, also challenges the idea of human evolution from animals. This was certainly recognized by critics such as W. H. Holmes.

It is not hard to see why a supporter of the idea of human evolution, such as Holmes, would want to do everything possible to discredit information pushing the existence of humans in their present form too far into the past. Why did Holmes feel so confident about doing so? One reason was the discovery in 1891, by Eugene Dubois, of Java man (*Pithecanthropus erectus*), hailed as the much sought after missing link connecting modern humans with supposedly ancestral apelike creatures. Holmes (1899, p. 470) stated that Whitney's evidence "stands absolutely alone" and that "it implies a human race older by at least one-half than *Pithecanthropus erectus* of Dubois, which may be regarded as an incipient form of human creature only." For those who accepted the controversial Java man (Chapter 7), any evidence suggesting the modern human type existed before him had to be cut down, and Holmes (1899, p. 448) was one of the principal hatchet men. Holmes stated about the California finds: "It is probable that without positive reinforcement the evidence would gradually lose its hold and disappear: but science cannot afford to await this tedious process of selection, and some attempt to hasten a decision is demanded."

Holmes and his partner Hrdlicka warred long and hard to discredit all evidence for a human presence in the Americas any further back than four or five thousand years ago. During the nineteenth century, an extensive amount of evidence demonstrating a human presence far into

the Tertiary had been amassed. But by the beginning of the twentieth century, it had become apparent to many American scientists that the decks had to be cleared.

Sinclair assisted in this task. In his introductory remarks to his paper on the California finds. Sinclair (1908, p. 108) wrote: "In working on the general problem of the time of man's appearance in the California region, the Department of Anthropology of the University of California has taken up, as a necessary part of the investigation, a review of the evidence relating to the so-called auriferous gravel relics. The writer was commissioned to visit the localities where the discoveries of human remains reported by Whitney and others were made."

Translation: Responsible scientists had concluded that modern human beings evolved from *Pithecanthropus erectus*, discovered in Java in 1891, in Middle Pleistocene formations. It was therefore an embarrassment to the University of California that it had in its collections stone implements said to date back well into the Tertiary. Further complicating the matter was the fact that the Tertiary age of these implements (and their human manufacturers) was vigorously advocated by the state geologist of California and other scientists. This contradicted the emerging picture of human evolution in general, as well as the increasingly accepted view that humans entered the Americas only recently. Sinclair was thus "commissioned" to do the required demolition job, and he did it well.

As might be guessed, Sinclair shared the evolutionary bias of Holmes, and it was this bias, more than anything else, that determined his negative attitude toward the California evidence. Sinclair (1908, pp. 129-130) wrote: "The occurrence in the older auriferous gravels of human remains indicative of a state of culture and a degree of physical development equal to that of the existing Indians of the Sierra Nevada would necessitate placing the origin of the human race in an exceedingly remote geological period. This is contrary to all precedent in the history of organisms, which teaches that mammalian species are short-lived. In North America, there are abundant remains of the lower animals preserved in deposits ranging from the Eocene to the Pleistocene. In all these deposits, excepting those of late Pleistocene age, the remains of man or any creature directly ancestral to man are conspicuously absent. No remains of *Anthropoidea* (from which man is doubtless derived), are known on this continent. "

There are a number of comments that can be made about Sinclair's statements. As far as the history of organisms is concerned, we propose that our investigation of human antiquity shows that the history of other organisms might be different than Sinclair supposed. We began this present study for the purpose of evaluating the claim that all available evidence supports an evolutionary view of human origins, with modern humans descending quite recently from more apelike predecessors. We have determined, after thorough investigation, that such is not the case, that there is in fact abundant evidence that human beings of modern type have coexisted with more apelike creatures as far back in time as we care to extend our research. This clearly contradicts the usual claims made by evolutionists. We are therefore not certain what an objective evaluation of the fossil evidence for the history of other mammalian species might reveal.

Sinclair further maintained that human remains are absent from all North American deposits “excepting those of late Pleistocene age.” He also stated: “It has been reported on the preceding pages that a large proportion of the implements reported from the gravels are from those of the rhyolitic and intervalcanic epochs. This would mean that man of a type as high as the existing race was a contemporary of the three-toed horse and other primitive forms of the late Miocene and early Pliocene, a thesis to which all geological and biological evidence is opposed” (Sinclair 1908, p. 130).

But if one were to agree that the California auriferous gravel discoveries should be rejected just because nothing like them had been discovered before, one would then be obliged to reject any fundamentally new paleontological discoveries whatsoever. Of course, it should be pointed out that at the time Sinclair was writing there was in fact abundant evidence, from North America, South America, and Europe, attesting to a human presence in the Early Pleistocene, Pliocene, Miocene, and earlier geological periods. If Sinclair was aware of this evidence, which he should have been, he simply chose to ignore it. Concerning Sinclair's assertion that no evidence of any anthropoid creatures had been found in North America, even this is contrary to the facts (Chapter 10).

In short, there was good reason to accept the California finds, as well as the many other discoveries we have reviewed in our discussion of anomalous stone tool industries. Nevertheless, in the early part of the twentieth century, the intellectual climate favored the views of Holmes and Sinclair. Tertiary stone implements just like those of modern humans? Soon it became uncomfortable to report, unfashionable to defend, and convenient to forget such things. Such views remain in force today, so much so that discoveries that even slightly challenge dominant views about human prehistory are effectively suppressed.

Concluding our study of anomalous stone tool industries, let us review some of the main points of interest. (1) Anomalous stone tool industries are not rare, isolated occurrences. The cases we have discussed form a massive body of evidence. Although many discoveries occurred in the nineteenth and early twentieth centuries, they have continued to occur up to the present. (2) Anomalous stone tool industries from very early times are not limited to eoliths, the human manufacture of which is considered by some as doubtful. Artifacts of undoubted human manufacture, similar to those of the finest Neolithic craftsmanship, are known to occur in geological contexts of extreme antiquity, as demonstrated by the California discoveries. (3) The much-debated eoliths are comparable to many unquestioningly accepted crude stone tool industries. Furthermore, eoliths appear to bear signs of intentional work not encountered in rocks broken by purely natural forces. (4) The scientific reporting of anomalous stone tool industries, even in the nineteenth century, was rigorous and of high quality.

(5) It is apparent that preconceptions about human evolution have played an important role in the suppression of reports of anomalous stone tool industries. Such suppression continues to the present day.

Anomalous Human Skeletal Remains

In the nineteenth and early twentieth centuries quite a number of scientists found stone Implements and other artifacts in Tertiary and early Quaternary formations. Scientists also discovered anatomically modern human skeletal remains in similarly ancient geological contexts.

Although these human bones originally attracted considerable attention, they are now practically unknown. Most current literature gives one the impression that after the discovery of the first Neanderthal in the 1850s no significant skeletal finds were made until the discovery of Java man in the 1890s.

For example, in describing the aftermath of the first Neanderthal find in 1856, anthropologist Jeffrey Goodman (1982, p. 56) wrote: 'In the decades that followed, only discoveries of very old and crudely fashioned stone tools were made.' As we shall see, this is simply not true. Why, then, do we rarely, if ever, encounter discussions of the skeletal finds of this period in modern paleoanthropological literature? One reason may be that these finds contradict the current scenario of human evolution.

We shall now consider these skeletal remains, some more challenging to the accepted views of human evolution than others. These anomalous discoveries are not numerous, but then the accepted hominid skeletal remains enshrined in museums around the world are also limited in number. More than one author has declared that the essential skeletal evidence supporting the idea that human beings evolved from apelike creatures would fit on a billiard table or two.

R. N. Vasishat (1985, p. 1) stated that the 'fossil primate record is poor and within it, the record of fossil man still poorer.' He added, however, that the few remains that have been recovered 'when considered in the context of evolutionary evidences, known for other vertebrates with a better fossil record, allow us justifiable efforts at primate phylogenetic restorations' (Vasishat 1985, p. 1).

At first glance, the hominid fossils mentioned by Vasishat seem to support the phylogenetic restorations one usually encounters in textbooks and museums, but these restorations of evolutionary lineages fall apart when we include the human skeletal remains presented in this chapter.

In discussing these human bones, we shall focus first on their circumstances of discovery and the resulting stratigraphic age determinations, which are beyond the range modern evolutionary theory would permit. We shall begin with the least anomalous discoveries and then discuss those that are more so. In Appendix 1, we will review negative critiques by modern scientists, who have used chemical and radiometric methods to discredit the anomalously old stratigraphic dates assigned to some of the skeletal remains.

## 6.1 Middle and early Pleistocene discoveries

The first finds we shall consider are from the Middle and Early Pleistocene. The Trenton femur, if correctly dated, would be about 100,000 years old, which is anomalous for North America. The Galley Hill skeleton, from England, and the Moulin Quignon Jaw, the Clichy skeleton, and the La



Denise skull fragment, from France, are of ambiguous age, but are nevertheless relevant to our study of how scientists treat paleoanthropological evidence. The Ipswich skeleton appears to place anatomically modern humans in England during the Hoxnian interglacial, over 300,000 years ago. Many other Middle Pleistocene sites in Europe are linked with *Homo erectus*, even though no skeletal remains have been found. We argue that the tools and other artifacts found at these sites could just as well be attributed to anatomically modern *Homo sapiens*. A fully modern skull found by workmen excavating a dry dock in the harbor of Buenos Aires, Argentina takes us back to the Early Pleistocene. We shall also discuss a very primitive skullcap from Brazil, indicating the presence of creatures resembling *Homo erectus* in South America.

In our discussion of twentieth-century African discoveries (Chapter 11), we shall review three additional finds. These are the human skeleton recovered by H. Reck from an Early Pleistocene formation at Olduvai Gorge, Tanzania (Section 11.2), the human jaw discovered by Louis Leakey in an Early Pleistocene formation at Kanam, Kenya (Section 11.3), and the human skull fragments discovered by Louis Leakey in a Middle Pleistocene formation at Kanjera, Kenya (Section 11.3). We have chosen to discuss these three anomalous cases in Chapter 11 rather than here because they are closely connected to the accounts of conventionally accepted African finds.

#### 6.1.1 The Trenton Human Bones (Middle Pleistocene)

On December 1, 1899, Ernest Volk, a collector working for the Peabody Museum of American Archaeology and Ethnology at Harvard University, discovered a human femur in a railroad cut south of Hancock Avenue within the city limits of Trenton, New Jersey. The femur was found lying on a small ledge, 91 inches beneath the surface. Volk (1911, p. 115) stated: "About four inches over or above the bone . . . was a place about the length of the bone where it evidently had fallen out of." This impression was overlain by the following strata:

7 inches of surface black soil, 16 -20 inches of yellow loam with water-worn pebbles, 44 inches of coarse gravel cemented together with red clay, and 21 inches of clean sand with red bands lying close together (Volk 1911, p. 116).

The human femur was found towards the bottom of the clean sand stratum, and it was photographed in that spot by Volk, who declared that the overlying strata immediately above and for some distance on either side of the find were undisturbed.

The fossil femur from Trenton was examined by two famous anthropologists, F. W. Putnam of the Peabody Natural History Museum at Harvard University and A. Hrdlicka of the Smithsonian Institution. Both of them declared the bone to be human. According to Hrdlicka (1907, p. 46), Putnam reported on the femur to the American Association for the Advancement of Science. Volk (1911, p. 117) wrote: "It was found to be part of the left femur of a human being, that had been cutoff square at one end; the cellular structure had been gouged out to enlarge the opening and it had been perforated in two places; it had apparently been the handle of some implement." Volk said that the femur was thoroughly fossilized.

On December 7, 1899, Volk returned to the railway cut. About 24 feet west of the spot where he found the fossilized femur, and in the same layer, Volk recovered two fragments of a human skull (part of the parietal). The strata immediately overhead and for some distance on either side were said to be undisturbed.

Volk (1911, p. 118) stated: "That these human bones did not come from the upper deposits is made more probable by the fact that wherever. .. human skeletons have been found they have invariably been stained by the deposit in which they had been lying, but these fragments were nearly white and chalky." The upper deposits were reddish and yellowish.

Hrdlicka (1907, p. 46) stated that the stratum in which the Trenton femur was found lay underneath a deposit of glacial gravel. This would put the Trenton femur well back into the Pleistocene period. We have already discussed the views of Hrdlicka (Section 5.1.2), who labored hard to prove that human beings entered North America and South America only quite recently, during the Holocene. Since the Trenton femur was like that of modern humans, Hrdlicka suspected it was of recent age. He expected that a genuinely ancient human femur should display primitive features. Hrdlicka (1907, p. 46) therefore said about the Trenton femur: "The antiquity of this specimen must rest on the geological evidence alone.'

Hrdlicka, however, was apparently unable to point out anything strikingly wrong with the geological evidence. The femur had been found in undisturbed Pleistocene interglacial deposits by a reputable collector for a prestigious university. Consequently, Hrdlicka did not directly dispute the femur's Pleistocene interglacial age, but one gets the impression he felt further research would prove it recent. Hrdlicka (1907) did not mention the skull fragments Volk found.

In a letter dated July 30, 1987, Ron Witte of the New Jersey Geological Survey told us that the stratum containing the Trenton femur and skull fragments is from the Sangamon interglacial and is about 107,000 years old. According to standard ideas, human beings of modern type arose in southern Africa about 100,000 years ago and migrated to America at most

30,000 years ago.

#### 6.1.2 Some Middle Pleistocene skeletal remains from Europe

During the nineteenth century, several discoveries of human skeletal remains were made in Middle Pleistocene formations in Europe. The reports we have studied raise doubts about the true age of these bones. We have nevertheless included them in our discussion for the sake of completeness. The presence of these skeletons in Middle Pleistocene strata could be attributed to recent intrusive burial, mistakes in reporting, or fraud. Nonetheless, there are reasons for thinking that the skeletons might in fact be of Middle Pleistocene age. We shall now briefly review some of the more noteworthy cases.

##### 6.1.2.1 Galley Hill

In 1888, workmen removing deposits at Galley Hill, near London, England, exposed a bed of chalk. The overlying layers of sand, loam, and gravel were about 10 or 11 feet thick. One workman, Jack Allsop, informed Robert Elliott, a collector of prehistoric items, that he had discovered a human skeleton firmly embedded in these deposits about 8 feet below the surface and about 2 feet above the chalk bed (Keith 1928, pp. 250-266).

According to Elliott, Allsop had removed the skull but left the rest of the skeleton in place. Elliott stated that he saw the skeleton firmly embedded in the stratum: "I carefully examined the section on either side of the remains, for some distance, drawing the attention of my son, Richard, who was with me, and of Jack Allsop to it. It presented an unbroken face of gravel, stratified horizontally in bands of sand, small shingle, gravel, and, lower down, beds of clay and clayey loam, with occasional stones in it—and it was in and below this that the remains were found. We carefully looked for any signs of the section being disturbed, but failed: the stratification being unbroken, and much the same as the section in the angle of the pit remaining to this day" (Keith 1928, p. 253).

Elliott then removed the skeleton and later gave it to E. T. Newton (1895), who published a report granting it great age. An independent observer, a school master named M. H. Heys, reported that he had also seen the bones embedded in undisturbed deposits before Elliott removed the skeleton. Heys did not know Elliott at the time he examined the bones, and in fact he saw the bones before Elliott saw them. Heys reported that he saw the skull *in situ* just after it was exposed by a workman excavating the deposits and before it was removed from them.

Heys said about the bones: "No doubt could possibly arise to the observation of an ordinary intelligent person of their deposition contemporaneously with that of the gravel. for there was a bed of loam, in the base of which these human relics were embedded. The underneath part of the skull, as far as I could see, was resting on a sandy gravel. The stratum of loam was undisturbed. This undisturbed state of the stratum was so palpable to the workman that he said, 'The man or animal was not buried by anybody.' The gravel underneath the skull, of which I took particular notice, was stratified and undisturbed" (Keith 1928, p. 255).

Numerous stone tools were recovered from the Galley Hill site. Newton (1895, p. 521) reported: "Mr. Elliott has obtained several types of implements from this pit; namely, tongue or spear-shaped forms, ovoid implements, hand hatchets, chipping tools, drills or borers, and flakes of various kinds." Newton (1895, p. 521) added: "There are also many rude flakes and roughly-chipped flints. In this gravel, the human origin of which might be doubted if they were found alone; and occasionally deeply-stained primitive forms are met with, similar to those found by Mr. B. Harrison on the high plateau near Ightham."

According to Stuart Fleming (1976, p. 189), the stratum in which the Galley Hill skeleton was discovered is more than

100,000 years old. K. P. Oakley and M. F. A. Montagu (1949, p. 34) commented that this stratum is Middle Pleistocene and is "broadly contemporary with the Swanscombe skull." Oakley (1980, p.

26) and Gowlett (1984, p. 87) considered the Swanscombe skull, found only a short distance from Galley Hill, to be from the Holstein interglacial, which occurred about

330,000 years ago. The Galley Hill skeleton, if roughly contemporary with Swanscombe, would be of the same age.

In terms of anatomy, the Galley Hill skeleton was judged to be of the modern human type (Newton 1895, Keith 1928, Oakley and Montagu 1949). Most scientists now think that anatomically modern humans (*Homo sapiens sapiens*) originated in Africa around 100,000 years ago. They say that *Homo sapiens sapiens* eventually entered Europe in the form of Cro-Magnon man approximately 30,000 years ago, replacing the Neanderthals (Gowlett 1984, p. 118). Fully modern and found in strata contemporaneous with the Swanscombe site (about 330,000 years b.p.), the Galley Hill skeleton thus presents an anomaly.

Just what do modern paleoanthropologists say about the Galley Hill skeleton? Despite the stratigraphic evidence reported by Heys and Elliott, Oakley and Montagu (1949) concluded that the skeleton must have been recently buried in the Middle Pleistocene deposits. They considered the bones, which were not fossilized, to be only a few thousand years old. This is also the opinion of almost all anthropologists today.

The Galley Hill bones had a nitrogen content similar to that of fairly recent bones from other sites in England. Nitrogen is one of the constituent elements of protein, which normally decays with the passage of time. But there are many recorded cases of proteins being preserved in fossils for millions of years. Because the degree of nitrogen preservation may vary from site to site, one cannot say for certain that the relatively high nitrogen content of the Galley Hill bones means they are recent. The Galley Hill bones were found in clayey sediments known to preserve protein.

Oakley and Montagu (1949) found the Galley Hill human bones had a fluorine content similar to that of Late Pleistocene and Holocene (recent) bones from other sites. It is known that bones absorb fluorine from groundwater. But the fluorine content of groundwater may vary widely from place to place and this makes comparison of fluorine contents of bones from different sites an unreliable indicator of their relative ages.

Later, the British Museum Research Laboratory (Barker and Mackey 1961) obtained a carbon 14 date of 3,310 years for the

Galley Hill skeleton. But this test was performed using methods now considered unreliable. Also, it is highly probable that the Galley Hill bones, kept in a museum for 80 years, were contaminated with recent carbon, causing the test to give a falsely young date.

For a more detailed discussion of the above-mentioned tests, see Appendix 1. Although modern paleoanthropologists have great confidence in these tests, there are good reasons for thinking that they are at least as imperfect and subject to error as older methods of dating, such as stratigraphic observation. Thus chemical and radiometric test results do not automatically invalidate stratigraphic observations with which they may be in disagreement.

In attempting to discredit the testimony of Elliott and Heys, who said no signs of burial were evident at Galley Hill, Oakley and Montagu (1949) and Oakley (1980) offered several arguments. In addition to their chemical and radiometric tests.

Oakley and Montagu suggested (1949, p. 37) that by the time Elliott and Heys saw the skeleton "it is probable that the bulk of any evidence of burial had already been destroyed by the gravel digger."

Oakley and Montagu (1949, p. 36) also stated that many fragments of animal bones had been found in the sands and gravels at the Barnfield and Rickson's pits (both are a half mile from Galley Hill), whereas no animal bones had been found at the Galley Hill pit. From these facts, they concluded that originally there may have been a substantial number of animal bones in the Galley Hill deposits. They hypothesized that later these animal bones were all decalcified, or dissolved away, by the groundwaters. Hence the Galley Hill skeleton must have been recently introduced into the Middle Pleistocene gravels, after all the genuine Middle Pleistocene bones had been dissolved away. If the skeleton were really of Middle Pleistocene antiquity, it should have been dissolved away like the rest of the bones.

According to Oakley and Montagu (1949, p. 36), "This point does not seem to have been considered by previous investigators." That is not, however, accurate. E. T. Newton, the scientist who published the original report about the Galley Hill skeleton, was well aware of the significance of the absence of animal bones at Galley Hill and other places in the immediate vicinity. Newton wrote (1895, p. 524): "The rarity of bones in these high-level gravels suggests the possibility of their having been removed by the continued percolation of water during the long period which has elapsed since they were deposited. It still further suggests that, if any human bones had been deposited with the gravel in Paleolithic times, they would long since have disappeared. However bones of certain extinct mammals, Elephas. Rhinoceros. Hippopotamus. and Felis leo. have occasionally been found, although generally in a much decayed condition, and the circumstances sufficiently favourable for their preservation may have obtained in other places also."

Elliott reported that when the Galley Hill skeleton was uncovered "the bones were so friable and fragile that many went to pieces as soon as touched" (Newton 1895, p. 518; Keith 1928, p. 253). The decayed condition of the human bones thus matches that of the other rare occurrences of genuinely old mammalian bones in the immediate vicinity of the Galley Hill site.

Oakley and Montagu considered the possibility that the Galley Hill skeleton had been protected from percolating groundwaters by the loam layer in which it was embedded, but they concluded that this loam layer was "permeable." Yet Newton (1895, p. 524) stated: "it is clear from Mr. Elliott's letter, and from my own observation in the pit, that patches of more clayey deposit do here and there occur, one such having been noticed very near where the skeleton was found." Clay is less permeable to water than loam, and, as noted in Appendix 1, is responsible for many remarkable cases of organic preservation.

Oakley and Montagu argued that the relatively complete nature of the Galley Hill skeleton was a sure sign that it was deliberately buried. The postcranial bones found were two partial humeri, two partial femurs, two partial tibiae, and some small fragments of the ribs and hip. Completely missing were almost all of the ribs, the backbone, the forearms, hands, and feet. In the case of Lucy, the most famous specimen of *Australopithecus afarensis*, more of the skeleton was preserved (Section 11.9.3). And no one has yet suggested that *Australopithecines* buried their dead. Scientists have also discovered fairly complete skeletal remains of *Homo erectus* (Section 7.1.8) and *Homo habilis* (Section 11.7) individuals.

These cases, as all paleoanthropologists would agree, definitely do not involve deliberate burial. It is thus possible for relatively complete hominid skeletons to be preserved apart from burial.

Throughout their report, Oakley and Montagu returned to the suggestion that the Galley Hill skeleton must have been a burial, and this may in fact be true. But the burial may not have been recent. Sir Arthur Keith (1928, p. 259) suggested: "Weighing all the evidence, we are forced to the conclusion that the Galley Hill skeleton represents a man . . . buried when the lower gravel formed a land surface."

To sum it up, the arguments presented by Oakley and Montagu suggest the Galley Hill skeleton may have been a recent burial. But these arguments are not conclusive enough to invalidate the stratigraphic observations of Elliott and Heys, who, like Keith, were convinced the Galley Hill skeleton was genuinely ancient.

As can be seen, old bones point beyond themselves, quite obliquely, to events in the remote and inaccessible past. Controversy about their age is almost certain to arise, and in many cases the available evidence is insufficient to allow disputes to be definitely settled. This would appear to be true of Galley Hill. Since

1949, most scientists have, however, followed the lead of Oakley and Montagu in assigning the Galley Hill skeleton a recent date.

#### 6.1.2.2 The Moulin Quignon Jaw: A Possible Case of Forgery

In 1863, Boucher de Perthes discovered an anatomically modern human jaw in the Moulin Quignon pit at Abbeville, France. He removed it from a layer of black sand and gravel that also contained stone implements of the Acheulean type (Keith 1928, p. 270). This black layer was 16.5 feet below the surface of the pit. According to Gowlett (1984, p. 88), the Acheulean sites at Abbeville are of the same age as the Holstein interglacial, and would thus be about

330,000 years old.

Upon hearing of the discovery of the Abbeville Jaw and tools, a group of distinguished British geologists visited Abbeville and were at first favorably impressed (Keith 1928, p. 271). Later, however, it was alleged that some of the stone implements in Boucher de Perthes's collection were forgeries "foisted on him by the workmen" (Keith 1928, p. 271). The British scientists began

to doubt the authenticity of the jaw. Taking a tooth found with the jaw back to England, they cut it open and were surprised at how well preserved and fresh it appeared. Also they determined that it contained 8 percent "animal matter" (organic matter in today's terms). Sir Arthur Keith pointed out, however, that in the same museum where the scientists met there were animal bones of Pleistocene age, prepared for display by John Hunter in 1792, containing up to 30 percent animal matter.

There are also reports of bones from the Late Pliocene Red Crag formation with up to 8 percent animal matter (Osborn 1921, p. 568).

It is not clear exactly how old the bones prepared by Hunter actually were—they might have been Late Pleistocene, perhaps as little as 10,000 years old. Even so, the general point that Keith was making is relevant. As we show in Appendix 1, there is much evidence that the amount of organic matter remaining in a bone (as measured by nitrogen content) is not always a reliable indicator of a bone's age. Neither is the degree of fossilization. The rate at which a bone's organic matter decays, or the rate at which minerals accumulate in a bone, varies greatly from one location to another.

According to Ronald Millar (1972, p. 72), the Moulin Quignon Jaw had a coloring 'which was found to be superficial' and "was easily scrubbed from one of the portions of bone, revealing a surface which bore little of the erosion common in old bones." Some took this to be an indication of forgery. But Keith (1928, p. 272) interpreted this differently: 'The mandible was originally covered by the black specks of the stratum in which it lay. Mr. Busk found he could brush these specks off; that does not invalidate its authenticity.'

Prestwich was also said to have discovered that the flint tools from Moulin Quignon had a superficial coloring that could easily be washed off. But other pieces of flint (not artifacts) from the same site had a coloring that could not be scrubbed off. This was also taken by British scientists as an indication of forgery.

In May 1863, British geologists met with their French counterparts in Paris to jointly decide the status of the jaw. According to Keith, the French maintained the jaw was authentic despite arguments by the British that it was a forgery.

Keith (1928, p. 271) stated: 'French anthropologists continued to believe in the authenticity of the jaw until between 1880 and 1890, when they ceased to include it in the list of discoveries of ancient man. At the present time opinion is almost unanimous in regarding the Moulin Quignon jaw as a worthless relic. We see that its relegation to oblivion begins when the belief became fixed that Neanderthal man represented a Pleistocene phase in the evolution of modern races. That opinion, we have seen, is no longer tenable.'

In other words, scientists who believed the Neanderthals were the immediate ancestors of Homo sapiens could not accommodate the Moulin Quignon Jaw because it would have meant that anatomically modern human beings were in existence before the Neanderthals. Today, the idea

that the Neanderthals were the direct ancestors of the modern human type is out of vogue, but this in itself does not clear the way for acceptance of the Abbeville Jaw, which if genuine, would be over 300,000 years old.

From the information we now have at our disposal, it is difficult to form a definite opinion about the authenticity of the Moulin Quignon Jaw. Even if we accept that the Jaw and the many flint implements found along with it were fakes, what does this tell us about the nature of paleoanthropological evidence? As we shall see, the Moulin Quignon Jaw and tools, if they were forgeries, are not alone. Piltdown man (Chapter 8) was accepted for 40 years before being dismissed as an elaborate hoax.

#### 6.1.2.3 The Clichy Skeleton

In 1868, Eugene Bertrand reported to the Anthropological Society of Paris that on April 18 of that year he found parts of a human skull, along with a femur, tibia, and some foot bones, in a quarry on the Avenue de Clichy. According to Keith (1928, pp. 276-277), the bones were found 5.25 meters (17.3 feet) beneath the surface, in a grey loam. Bertrand (1868, pp. 329-330) reported a similar depth but said that the bones were found in a reddish clayey sand layer within the grey loam. MacCurdy (1924a, p. 413) said that the bones were found in "a band of reddish sand at the base of the gray diluvium." A workman at the site reported that this reddish band was 10 or 20 centimeters (about 4 -8 inches) in thickness (Bertrand 1868, p. 332). Keith believed that the age of the stratum in which the human bones were found was roughly the same age as the layer in which the Galley Hill skeleton was discovered. We recall from Section 6.1.2.1 that this layer is, according to current estimates, approximately 330,000 years old. The depth at which the Clichy human fossils were found (over 17 feet) argues against the recent intrusive burial hypothesis, and furthermore there was no mention of any disturbance in the overlying strata.

But Gabriel de Mortillet (Bertrand 1868, p. 332) said that a workman at the quarry on the Avenue de Clichy told him that he had stashed a skeleton in the pit. Skeptical about the reliability of testimony by workmen, de Mortillet asked him for proof. According to de Mortillet, the workman responded by telling him that he had taken the skeleton from a layer of reddish sediments in the upper part of the quarry. This explained why the bones found by Bertrand were reddish in color. The workman said that the layer of reddish material in the lower levels of the quarry was too thin to contain the bones.

According to de Mortillet, the workman further said that in the upper levels of the quarry, bones from the same animal were sometimes found together, whereas in the lower levels the fossil remains of mammals were always mixed and scattered. Thus the fact that the human bones were found "piled up in a little space" indicated they were not originally part of the layer in which they



were found. On the basis of the workman's statements, de Mortillet concluded that the skeleton said to have been stashed by the workman was the same as the one found by Bertrand.

There is, of course, no guarantee that the workman was speaking the truth—de Mortillet himself frankly admitted the unreliability of testimony from workmen. On the other hand, it is possible that the workman was being honest, and if this is the case then the Clichy skeleton could be only a few thousand or a few hundred years old.

De Mortillet was convinced by the workman's report, but the facts the workman reported can be interpreted differently. The workman suggested that the layer in which Bertrand said he found the bones was too thin to have colored them. But according to the workman, the maximum depth of the layer in question was 8 inches, which seems enough to have accommodated the fragmentary skeletal remains reported by Bertrand.

The workman's objection that it was unprecedented for several bones of the same creature to be found together in the lower layers of the quarry is of questionable significance. Eugene Bertrand (1868) said that he had evidence, which he planned to show to the Anthropological Society, that it was common for bones of the same animal to be found together in the lower layers as well as the upper layers.

Even after hearing de Mortillet relate the workman's story about stashing the bones of the Clichy skeleton, a number of scientists remained convinced Bertrand's discovery was genuine. For example, Professor Hamy (Bertrand 1868, p. 335) said: "Mr. Bertrand's discovery seems to me to be so much less debatable in that it is not the first of this kind at Avenue de Clichy. Indeed, our esteemed colleague, Mr. Reboux, found in that same locality, and almost at the same depth (4.20 meters), human bones that he has given me to study. "

Hamy was not alone in accepting the Clichy find. Keith (1928, p. 276) reported that almost all authorities in France believed that the Clichy skeleton was as old as the layer in which Bertrand said it was found. Keith mentioned, however, that later on, after accepting the Neanderthals as the Pleistocene ancestors of modern humans, French anthropologists dropped the Clichy skeleton, which predated the Neanderthals, from the list of bona fide discoveries. A representative of the modern human type should not have been existing before his supposed ancestors. The Neanderthals are thought to have existed from

30,000 to 150,000 years ago. If the Clichy skeleton is about the same age as the Swanscombe skull, as suggested by Keith, it would be over 300,000 years old.

In his remarks to the Anthropological Society, Bertrand provided additional evidence for the great antiquity of the Clichy skeleton. He stated that he found a human cubitus, or ulna, in the stratum containing the other bones of the Clichy human skeleton. The ulna, the larger of the two long bones of the forearm, is located on the side opposite the thumb. When Bertrand tried to extract the ulna it crumbled into dust. He offered this as proof that the Clichy human skeleton must have been native to the layer in which it was found. Apparently, Bertrand reasoned that a bone as

fragile as the decayed ulna could not possibly have been removed from an upper layer of the quarry and inserted into the lower layer in which he found it—it would certainly have been destroyed in the process. This indicated that the ulna belonged to the stratum in which

Bertrand found it, as did the other human bones.

So in the case of the Clichy site, we have testimony indicating a recent age for the human skeleton found there, but at the same time there are good arguments that it was Middle Pleistocene.

#### 6.1.2.4 La Denise, France

In the 1840s, pieces of human bone were discovered in the midst of volcanic strata at La Denise, France. Of particular interest was the frontal of a human skull. Keith (1928, p. 279) stated that the frontal "differs in no essential particular from the frontal bone of a modern skull."

The frontal was reported to have been taken from a limonite bed of considerable age. De Mortillet (1883, p. 241) wrote: "That the human frontal bone, now in the collection of M. Pichot, was in fact from the bed of argillaceous [clayey] limonite is perfectly established by deep incrustations of limonite on the interior of the bone."

In 1926, the French researcher C. Deperet reported to the French Academy of Science on the stratigraphy at La Denise. Deperet (1926, pp. 358-361) said the source of the human fossils was a layer of sediment deposited in a lake that formed after a Pliocene volcanic eruption and before the resumption of volcanic activity in the Pleistocene. According to Deperet, river deposits over the basalt from the last eruptions contained an Aurignacian fauna—horses, rhinoceroses, mammoths, hyenas, etc. This meant the last eruptions at La Denise occurred not later than the Late Pleistocene. Deperet's report thus suggests the presence of humans of the modern type at La Denise at some time during the Pleistocene, between 30,000 years ago (the latest time for the last eruptions) and 2 million years ago (the earliest time for the first eruptions). We would like to obtain more detailed stratigraphic evidence for the age of the argillaceous limonite layer from which the La Denise Jaw was taken. Over a century after the La Denise fossils were discovered, they were tested chemically by K. P. Oakley. We shall discuss these test results, said to confirm a recent age for the fossils, in Appendix I.

#### 6.1.3 The Ipswich Skeleton (Middle Middle Pleistocene)

In 1911, J. Reid Moir discovered an anatomically modern human skeleton beneath a layer of glacial boulder clay near the town of Ipswich, East Anglia, in England. Reading through various secondary accounts, we learned that J. Reid Moir later changed his mind about the skeleton, declaring it recent. We thus did not consider the Ipswich skeleton for inclusion in this book. But after further investigation, we determined that the Ipswich skeleton could be genuinely old.

The key fact reported by Moir was that the Ipswich skeleton was found below a layer of boulder clay. The boulder clay of East Anglia overlies the Middle Pleistocene Cromer Forest Bed formation,

which in turn overlies the Late Pliocene Red Crag. According to modern opinion, the boulder clay (a glacial deposit) could be as much as .4 million years old (Table 2.1, p. 78).

The Ipswich skeleton was discovered in a pit located at a brick field overlooking the valley of the river Glpping. Sir Arthur Keith (1928, p. 293) stated: ' Passing northwards through Ipswich the traveller soon leaves the town and valley and finds himself on a plateau, about 150 feet above the level of the sea, and covered everywhere by a thick stratum of chalky boulder clay, varying in depth from 15 to 25 feet. . . . At the brick-field the chalky boulder clay has become reduced to astratum of about 4 feet in thickness. . . . That the stratum at the brickfield represents a direct extension of the great sheet of boulder clay, Mr. Moir proved by sinking a series of pits from the brick-field to the crown of the plateau. In the map prepared by the officers of the Geological Survey the chalky boulder clay is shown to extend to the pit."

The skeleton was found at a depth of 1.38 meters (about 4.5 feet), between the boulder clay and some underlying glacial sands. Moir was aware of the possibility that the skeleton might represent a recent burial. Therefore, according to Keith (1928, pp. 294-295), Moir 'took every means of verifying the unbroken and undisturbed nature of the stratum in and under which the skeleton lay."

Keith supervised the removal of the skeleton from its matrix at the Royal College of Surgeons. Keith (1928, p. 295) stated that "a whole skeleton was represented, and that it was placed on the right side in the ultra-contracted posture." To Keith the evidence suggested a burial from an ancient land surface. "At least it was not made from the present land surface," he said, "for the overlying stratum was intact" (Keith 1928, p. 295).

As for the condition of the bones, Keith said it was similar to that of Pleistocene animal fossils found elsewhere in the glacial sands. He noted: "The substance of the bones is grey and chalky in appearance, crumbling to white dry dust on pressure. The bones, when dissolved in hydrochloric acid, leave no animal matrix behind" (Keith 1928, p. 296).

The Ipswich skeleton was that of a man about 5 feet 10 inches tall. The brain capacity was 1430 cc, about average for modern humans, and according to Keith (1928, p. 297) "all the characters of the skull are those we are familiar with in modern man."

The discovery, however, inspired intense opposition. Keith (1928, p. 299) questioned: 'if ... the Ipswich skeleton had shown characters as distinctive as those of Neanderthal man . . would anyone have doubted its age was older than the deposition of the boulder clay?" Keith (1928, p. 299) answered: "I do not think the age would then have been called into question. But under the presumption that the modern type of man is also modern in origin, a degree of high antiquity is denied to such specimens.'

Keith (1928, p. 299) suggested: "It is, therefore, all the more important that every discovery of human remains, made in circumstances which make their high antiquity a reasonable presumption, should be placed on record, with no fact kept back." We fully agree with Keith on

this point, and indeed, his suggestion has been one of the operating principles governing the compilation of the material in this book. The nature of paleoanthropological evidence is that it is never absolutely conclusive. There is always a chance that new evidence or new methods of analysis might result in a reevaluation of previous discoveries. It is therefore valuable to keep the details of controversial finds readily at hand for future generations of researchers.

Despite opposition, Moir initially stuck to his guns, holding that the Ipswich skeleton was genuinely old. What then happened to change his mind? His own report, published in *Nature* in 1916, tells the story. Moir (1916, p. 109) conducted further excavations in the area and reported: 'These investigations have shown that at about the level at which the skeleton rested the scanty remains of a 'floor' are present, and that the few associated flint implements appear to be the same as others found on an old occupation-level in the adjacent valley. This occupation-level is in all probability referable to the early Aurignac period, and it appears that the person whose remains were discovered was buried in this old land surface. The material which has since covered the ancient 'floor' may be regarded as a sludge, formed largely of re-made boulder clay, and its deposition was probably associated with a period of low temperatures occurring in postchalky boulder clay times.' The Aurignacian stage in Europe occurred about

30,000 years ago (Gowlett 1984, p. 122) and is identified with *Homo sapiens sapiens*.

In Moir's statements we find nothing that compels us to accept a recent age for the skeleton. We knew from our discussion of stone tools (Section 3.3.1) that Moir believed in a temporal succession of tool types, the older being more primitive than the recent. In other words, Moir was operating under the influence of an evolutionary preconception. But our own review of stone tools led us to the conclusion that it is not possible to place such implements in a temporal sequence simply on the basis of their degree of sophistication. Modern human beings are known to make very crude stone tool implements, and we have evidence that very sophisticated tools, comparable to those of Aurignacian Europe, turn up all over the world, in very distant times. In the 1960s, such implements were discovered at Hueyatlaco, Mexico, in strata yielding a uranium series age of over 200,000 years (Section 5.4.4). During the nineteenth century, very advanced stone objects turned up in the California gold mines, in gravels that might be as old as the Eocene (Section 5.5). Therefore, we cannot agree with Moir that the discovery of tools of advanced type at the same level as the Ipswich skeleton was sufficient reason to reinterpret the site stratigraphy to bring the age of the skeleton into harmony with the supposed age of the tools.

Moir's reinterpretation of the boulder clay over the skeleton was not, it appears, based on any compelling geological evidence. In fact, he gave no geological reasons whatsoever in support of his conclusion that the boulder clay was a recently deposited sludge. Therefore, the simplest hypothesis is that it really was a layer of intact glacial boulder clay, as originally reported by Moir and recorded by the British Geological Survey on its detailed map of the region.

The age of the skeleton thus depends on the age of the boulder clay. Over the years, the age of the boulder clay in East Anglia has, however, been a matter of controversy. During the 1920s, Moir proposed that there were two glacial boulder clays in East Anglia, one laid down during the Mindel

glaciation, and the other during the subsequent Riss glaciation (Keith 1928, pp. 302-303). If this scheme is accepted, the boulder clay that covered the Ipswich skeleton would belong to the Riss glaciation. We note, however, that the Riss glacial period extended from about 125,000 to over 300,000 years ago, which would still give a considerable antiquity to the Ipswich skeleton.

But It appears that Moir was wrong about there being evidence for a Riss glaciation at Ipswich. The English glaciation equivalent to the European Mindel glaciation is the Anglian. The Anglian was followed by the Hoxnian interglacial. The next English glacial period, corresponding to the European Riss glaciation, used to be called the Gippingian, following Moir's interpretation of the glacial deposits by the Gipping River near Ipswich. But according a modern English authority, D. Q. Bowen (1980, p. 420), geologists have 'shown that the Gipping Till of Essex was Anglian in age.'

The 'necessary replacement' (Bowen 1980, p. 420) for the Gippingian glaciation was the Wolstonian. The clearest evidence for the Riss-equivalent Wolstonian glaciation is in the region of Birmingham, quite far from Ipswich, but there are some rather unclear signs of the Wolstonian in Mildenhall, about

40 miles to the northwest of Ipswich (Bowen 1980, p. 420). There is no sign of the Wolstonian at Ipswich itself. The final English glaciation, corresponding to the Wurm glaciation of continental Europe, was the Devensian, which did not come down as far as Ipswich (Bowen 1980, p. 421; Nilsson 1983, p. 113). A modern authority on the Pleistocene geology of England stated: "nowhere in East Anglia is it possible to demonstrate post-Hoxnian and pre-Devensian glaciation on stratigraphic grounds" (Bowen 1980, p. 420).

In other words, there was, in the opinion of modern authorities, no Riss-equivalent glaciation (the Wolstonian) at Ipswich. Neither did the subsequent Devensian ice sheet reach Ipswich. What this means is that the boulder clay at Ipswich can only be referred to the Anglian glaciation. Therefore the glacial sands in which the Ipswich skeleton was found must have been laid down between the onset of the Anglian glaciation, about 400,000 years ago, and onset of the Hoxnian interglacial, about 330,000 years ago. It would thus appear that the Ipswich skeleton is between 330,000 and 400,000 years old. Some authorities (Gowlett 1984, p. 87) put the onset of the Mindel glaciation (equivalent to the Anglian) at about 600,000 years, which would give the Ipswich skeleton an age potentially that great. Yet human beings of modern type are not thought to have appeared in Western Europe before 30,000 years ago (Gowlett 1984, p. 118).

From the story of the Ipswich skeleton, we learn that discoverers of anomalies, such as Moir, can be the victims of prejudices as strong as those of their opponents. For mavericks and establishment figures alike, evolutionary preconceptions block proper evaluation of paleoanthropological evidence. On the one hand, we find ideas about the recent evolution of the modern human form prevented certain scientists from believing that the Ipswich skeleton might be truly ancient. And on the other hand, ideas about the progressive evolution of stone tools influenced Moir to revise downward the age of his own discovery.

#### 6.1.4 Possible Early Man Sites With No Skeletal Remains

If anatomically modern humans were present during the Middle and Early Pleistocene in Europe and elsewhere, why, one might ask, are scientists no longer finding any evidence of this? Instead, today's scientists are continually finding *Homo erectus* sites of Middle and Early Pleistocene age.

But here we run into a problem. There are many Middle and Early Pleistocene sites at which scientists have found stone tools but no hominid bones. The artifacts at these sites, mostly of a type called Acheulian, are nevertheless attributed to *Homo erectus*. But from a strictly objective point of view, in the absence of hominid fossils, the Acheulian artifacts could just as well be attributed to *Homo sapiens sapiens*. This is true regardless of the level of sophistication of the tools, since anatomically modern humans are known to make and use tools of the crudest sort.

Let us now review some cases demonstrating the difficulties one encounters in determining who made artifacts found at a site. In Section 5.4.3, we considered Acheulian tools from Timlin, New York. Mainstream authorities would surely attribute these to anatomically modern humans. The same is true the Acheulian-type tool from at Black's Fork River, Wyoming, U.S.A. (Section 4.8). Likewise, it should be possible to attribute European Acheulian tools to *Homo sapiens sapiens* at sites with no hominid skeletal remains.

In Chapter 3, we discussed the Alabama pebble tools from the U.S.A., which are similar to the crude Oldowan tools of Africa (Section 3.8.5). The pebble tools at Olduvai Gorge are said to be the work of *Homo habilis* and are considered to be close to 2 million years old. Mainstream authorities, if they recognized the Alabama tools at all, would say they were not more than 12,000 years old and would attribute them to *Homo sapiens sapiens*.

We have also discussed the Late Pleistocene site at Monte Verde, Chile, where scientists found Oldowan-type tools hafted to wooden handles, along with other cultural remains typical of modern humans (Section 3.8.6). The preservation of wood in such circumstances is rare. It is therefore possible that the tools could have been found without the wooden handles and other perishable artifacts indicating a high level of culture. By analogy, we should be open to the possibility that at other sites where Oldowan tools are found, perishable artifacts typical of humans with a high degree of culture have been lost. This may be true even for Early Pleistocene sites. Normally, Oldowan tools, if found alone in an Early Pleistocene context, would not be attributed to anatomically modern humans. But the example of Monte Verde demonstrates that for sites where Oldowan tools alone are discovered it is possible that a variegated *Homo sapiens sapiens* culture was originally present but was not entirely preserved.

But even when signs of higher culture are present, scientists with strongly held preconceived ideas often fail to imagine that beings on the level of *Homo sapiens sapiens* might have been responsible for them. This is true for the Middle Pleistocene site of Terra Amata in southern France. Here, according to the discoverer, Henry de Lumley (1969, p. 42), campsites were established by bands of hominids 300,000 years ago. An account of the Terra Amata finds in a Time-Life book (*The Emergence of Man*) gave a date of 400,000 years.

At the ancient seashore site, de Lumley found oval patterns of post holes and stone circles indicating that the hominids erected temporary shelters and built fires. Also found were bone tools. Among them was one apparently used as an awl, perhaps to sew skins. Impressions found in the old land surface at the site were said to demonstrate that the hominids slept or sat on hides. Stone implements were also found, including an object described as a projectile point, made from volcanic rock obtained from the Esterel region, 30 miles away.

Significantly, no hominid fossils were found at the Terra Amata site in France. De Lumley (1969, p. 45) did, however, report "the imprint of a right foot, 9.5 inches long, preserved in the sand of a dune." The print was described as "human" in the Time-Life book. It was also said that the foot of the Terra Amata hominid was "arched to support his whole weight" and "had lost all trace of the ability to grasp that ape feet possess" (Time-Life 1973, p. 12). In a welcome display of scientific reserve, de Lumley, in his 1969 article about the Terra Amata discoveries published in *Scientific American*, did not identify the type of hominid that occupied the temporary habitation site on the shores of the Mediterranean.

However, the authors of the Time-Life study were not so inhibited. They wrote: "What sort of man visited this cove on the coast of Europe 400,000 years ago? Who was he? Although he came each spring for many years, the fossils he left behind him included no human bones—only a single human footprint in the hard sand. ... He was the first man. He is known, in the scheme of evolution, as *Homo erectus*, or upright man. He was the direct descendant of *Australopithecus*, a creature considered the missing link between the apes and man" (Time-Life

1973, pp. 11-12). Judging from the available reports, the footprint is not different from that of a modern human being. The Time-Life book's assertion that *Homo erectus* was the inhabitant of Terra Amata is therefore unjustified.

At the Torralba, Spain, site, estimated to be about 300,000 years old, stone tools have been found in connection with fossil bones of elephants. Some scientists have interpreted Torralba as a *Homo erectus* kill-site. But as in the case of Terra Amata, no hominid fossils were found there. Only preconceived ideas about human evolution allowed scientists to attribute the Torralba tools and elephant bones to *Homo erectus*.

One skeptical researcher, Lewis Binford, even disagreed that Torralba was a kill-site. During the Middle Pleistocene, the area was a boggy marsh. Binford (1981, p. 16) pointed out that elephant fossils are generally found by water margins, because that is where they tend to die. Furthermore, the sediments at Torralba, according to F. Clark Howell, were deposited over many tens of thousands of years (Binford 1981, p. 16). Over this time, 115 elephants died. Assuming the sediments were deposited in just one 10,000-year period, Binford calculated that one elephant died every 87 years. Natural deaths, by disease, old age, or predators, he said, could very well account for the accumulation of elephant bodies at that rate. During the same 10,000 years, 611 stone tools accumulated at the site. This again, is not very many, considering the time involved

—about 6 tools per century. So the association of stone tools and elephant bones could be purely accidental.

Reacting to the standard view of what happened at Torralba, Binford (1981, pp. 17-18) stated: "Man killed the animals while executing game drives —possibly aided by fire—butchered them, and carried the meat away—truly extraordinary. . . This is a truly remarkable set of conclusions to draw from the Torralba data. . . . Given an aggregation of stone tools— evidence of hominid behavior—It is assumed that all other remains associated with the stone tools are also a by-product of human behavior. The researchers of Torralba have certainly made this assumption. Pleistocene archaeologists need to abandon such an approach."

Examples such as Torralba and Terra Amata could be multiplied, for at most paleoanthropological sites, no hominid bones are found. The artifacts at these sites are attributed to *Homo habilis*, *Homo erectus*, the Neanderthals, or *Homo sapiens* on the basis of their presumed age or their level of workmanship. But this practice, strictly speaking, is not Justifiable. Therefore, many Early and Middle Pleistocene sites currently identified with *Homo erectus*, for example, could Just as well be identified with anatomically modern *Homo sapiens*.

#### 6.1.5 A Human Skull from The Early Pleistocene at Buenos Aires

In 1896, workers excavating a dry dock in Buenos Aires found a human skull (Figure 6.1). They took it from the rudder pit at the bottom of the excavation, after breaking through a layer of a hard, limestonelike substance called toska. The level at which the skull was found was 11 meters (36 feet) below the bed of the river La Plata (Hrdlicka 1912, p. 318).

The workers who found the skull gave it to Mr. Junor, their supervisor, a senior member of the public works division of the Port of Buenos Aires (Hrdlicka 1912, p. 318). Information about the skull was furnished to the Argentine paleontologist Florentino Ameghino by Mr. Edward Marsh Simpson, an engineer for Charles H. Walker & Co. of London, the company contracted to excavate the port of Buenos Aires (Ameghino 1909, p. 108; Hrdlicka 1912, p. 319). In the opinion of Ameghino, the skull removed from the rudder pit belonged to a Pliocene precursor of *Homo sapiens*. He called this precursor *Diprothon. no platensis*.

Figure 6.1. Human skull taken from an Early Pleistocene formation in Buenos Aires, Argentina (Hrdlicka 1912, plate 49).

A. Hrdlicka (1912, p. 319) wrote: "Professor Ameghino [1909, p. 121] concludes from the information obtained from Mr. Simpson alone that the fragments of the skull came from the lower



portion of the rudder-pit In Dry Dock No. 1 and from beneath the tosca. He states further, however, that beneath the tosca was found a layer of quartz sand followed by a stratum of grey clay, and that it was in this layer of grey clay, 50 cm. [about 20 inches] below the floor of the dry dock, that the skull-cap of the *Diprodium* was discovered. " Hrdlicka (1912, p. 321)

said about Ameghino's opinion on the age of the deposits below the tosca: 'The gray clay he identifies as belonging to the upper-most portion of the Pre-Ensenadean stratum, which is the most inferior part of the Pampean formation, and belongs to the base of the Pliocene.' The base of the Pliocene is now dated at approximately 5 million years before the present. But modern authorities say that the Ensenadan began 1.5 million years ago (Anderson 1984, p. 41) or 1 million years ago (Marshall et al. 1982, p. 1352). The Pre-Ensenadan stratum in which the Buenos Aires skull was found would thus be at least 1.0 -1.5 million years old. Even at 1 million years, the presence of a fully modern human skull anywhere in the world —what to speak of South America—is highly anomalous.

In the course of his investigation, Hrdlicka (1912, p. 319) found Mr. Simpson and Mr. Junor, and through them located Mr. J. E. Clark of Bahia Blanca, the foreman of the laborers who found the skull. Simpson revealed that he had not been present at the dry dock at the time the discovery had been made. He had simply received a report, but noted that he had been told there was more than one skull (Hrdlicka 1912, p. 319).

Hrdlicka (1912, p. 320) then reported the substance of the discussion he had with Mr. Junor: "Mr. Junor states that he did not see the find, but was told of it the next day, or perhaps the second day after, by the foreman, Mr. Clark. . . . The foreman brought Mr. Junor two pieces of the skull, and the latter saved them because they were said to have come from beneath the tosca, giving them later to the Museo Nacional. ... As to the place from which the bones came, he remembers having been informed that the workmen had gotten through the floor of the dock into a sort of quicksand when the bones were encountered.

The bones must have been just beneath the tosca, for a small quantity of tosca was adhering to them. ... He did not examine the site from which the skull fragments given him were supposed to have come. No inquiries were made of the laborers."

Relating the testimony of Clark, Hrdlicka (1912, p. 320) stated: "Mr. Clark states in his letter that the skull 'was found at the commencement of the Rudder Pit at dock bottom'; he 'is quite sure the skull was found at the Rudder Pit and under tosca'; and 'it was the only one found in that locality, but there was another skull found in the sand at the entrance to Dock No. 4."

Bailey Willis, the geologist who accompanied Hrdlicka on his expedition to Argentina, related this account of the interviews they had made: "Mr. Junor was found at his home in Flores, a suburb of Buenos Aires, on the evening of May 7, 1910, and we were most courteously received. He appeared to be about 70 years of age, of sanguine temperament, still enthusiastic as in youth, and an ardent believer of the antiquity of man in Argentina. He recited freely his recollection of the finding of the skull, stating in substance: The piece of skull was brought to him by the foreman of a

gang of workmen who were digging out the rudder-pit. He (Mr. Junor) was very much occupied at the time by duties of supervision of construction and did not see the skull taken out, nor did he examine the place afterward to see where it came from; but he had no doubt that it came out of the well, 'probably' from between a layer of tosca and the underlying sand. The skull was said to have been found by a workman, who passed it to the foreman, who in turn gave it to Mr. Junor. The workman cannot now be identified. It does not appear that he was ever questioned as to how the bone was found. . . On one point Mr. Junor was positive: The fragment of skull was taken out of the well. And although this statement rests on the say-so of the foreman who was told so by a workman, it appears to be the one item in the early history of the find that is not open to serious doubt" (Hrdlicka 1912, pp. 343-344).

Some will be critical of the fact that the skull was not found in place by a scientist. One should note, however, that the Heidelberg Jaw, which is accepted by paleoanthropologists as genuine, was uncovered by a workman in a sand pit in Germany and turned over to a foreman, who in turn brought it to the attention of a local professor (Section 7.2). Many of the *Homo erectus* specimens from Java, reports of which have found their way into all authoritative textbooks, were collected by Javanese laborers while scientists were absent from the sites (Section 7.3). A more recent example is the Petralona skull, which Greek villagers found in a spot not clearly designated. Despite this, scientists, on stratigraphic grounds, assign the Petralona skull an age of 200,000 to 300,000 years (Gowlett 1984, p. 87), and use it as evidence for an evolutionary transition from *Homo erectus* to *Homo sapiens*.

Willis speculated that the Buenos Aires skull had somehow arrived quite recently in the position in which it was found. In discussing this possibility, Willis gave the known details of the construction of the dry dock. First an embankment was built to keep out the river, and to keep the excavation dry there was a pump operating from the sump or well in the lowest place. Then a concrete floor was laid, and concrete walls were built. Finally the rudder pit was dug. It was during the digging of the rudder pit that the skull was found. Willis suggested: "Any objects contained in the material excavated [in the course of building the dry dock] or in the standing earth exposed at the side might have found their way into the close vicinity of the rudder-pit, if not into the pit itself" (Hrdlicka 1912, p. 344). But by the time the pit was dug, the concrete walls and floor of the dry dock were already completed, which would mean there should not have been much dirt piled around on the floor or exposed on the walls. Plus there is abundant testimony that the skull was in fact found under a hard layer of tosca and was not lying loose at the top of the pit before digging took place.

Willis added: "We were told by Dr. Francesco Moreno that he, when a boy, used to go swimming where the dry dock now is, in deep pools" (Hrdlicka 1912, p. 345). Willis noted that a few kilometers away from the dry dock, there is a place where the river "has worked out deep irregular holes into which anything like the skull-cap called *Diprhomomys* would readily sink and where it would become buried lower than the surface of the Pampaeon, but beneath recent river mud" (Hrdlicka 1912, p. 345). There is, however, no basis for saying this occurred at the dry dock,

where the skull was found beneath a layer of tufa in an excavation fully 11 meters (36 feet) below the present bed of the river La Plata.

As previously mentioned, Ameghino thought his *Diprotodonto* represented an ancestral form of human. According to Hrdlicka (1912, p. 323), he believed the skull's capacity was only 1100 cc, compared to 1400 cc for an average *Homo sapiens*, and that it had a low vault. Hrdlicka (1912, p. 325) stated: 'The writer reached Buenos Aires with the foregoing data before him and in consequence thereof with very eager expectations. But when the specimen itself was placed before him by Professor Ameghino there followed a rapid disenchantment.'

Hrdlicka (1912, p. 326) noted: "In a detailed study of the specimen it soon became plain that almost the entire original description by Ameghino had miscarried by reason of the fragment having been placed and considered in a wrong position. . . The accidental and faulty position of the fragment . . . had caused the forehead to appear much lower than it is. ... these results of faulty orientation combined have helped to make the specimen look extraordinary and primitive, even unhuman." Hrdlicka's views on the positioning of the skull fragment were supported in an independent report by G. Schwalbe of Germany (Hrdlicka 1912, p. 343).

Describing the skull in its new orientation, Hrdlicka (1912, p. 332) wrote: "It was fairly but not very high; its capacity was surely not below 1,350, more probably between 1,400 and 1,500 cc." Hrdlicka (1912, p. 332) further stated: "Every feature shows it to be a portion of the skull of man himself; It bears no evidence of having belonged to an early or physically primitive man, but to a well-developed and physically modern-like human individual."

A firm believer in evolution and the recent origin of the human species, Hrdlicka (1912, p. 2) stated: "no conclusion can be more firmly founded than that man is the product of an extraordinary progressive differentiation from some anthropogenic stock, which developed somewhere in the later Tertiary, among the primates,"

Hrdlicka therefore believed that fossils of human ancestors from the Tertiary and Quaternary should be to a greater or lesser degree apelike, as confirmed by the discovery of *Pithecanthropus erectus* in Java in 1891. Any fossils of anatomically modern human appearance from the Tertiary and Quaternary had to be explained away as intrusive burials or hoaxes, Hrdlicka's prejudice is evident in the following statement (1912, p. 2): "to establish beyond doubt the geological antiquity of human remains, it should be shown conclusively that the specimen or specimens were found in geologically ancient deposits, whose age is confirmed by the presence of paleontologic remains; and the bones should present evidence of organic as well as inorganic alterations, and show also morphologic characteristics referable to an earlier type. In addition, it is necessary to prove in every case by unexceptional evidence that the human remains were not introduced, either purposely or accidentally, in later times into the formation in which discovered."

Hrdlicka (1912, p. 2) amplified this view in another statement: "On the basis of what is positively known to-day in regard to early man, and with the present scientific views regarding man's evolution, the anthropologist has a right to expect that human bones, particularly crania,

exceeding a few thousand years in age, and more especially those of geologic antiquity, shall present marked morphological differences, and that these differences shall point in the direction of more primitive forms."

Hrdlicka (1912, p. 3) further stated: "The antiquity, therefore, of any human skeletal remains which do not present marked differences from those of modern man may be regarded, on morphologic grounds, as only insignificant geologically, not reaching in time, in all probability, beyond the modern, still unfinished, geologic formations. Should other claims be made in any case, the burden of proof would rest heavily on those advancing them." Here we have a very clear formulation of the dubious principle of dating by morphology. We also see the application of a double standard in the treatment of evidence, with finds contrary to evolutionary expectations being subjected to much more rigorous scrutiny than finds conforming to evolutionary expectations.

Hrdlicka's views, which, in modified form, remain in force today among the vast majority of scientists concerned with human origins and antiquity, represent a perversion of the scientific method. Hrdlicka and those who shared his methodology were not prepared to impartially consider the facts and construct a theory upon the foundation of all the available evidence. Rather they allowed their theoretical biases to determine what evidence should be considered valid. Modern students of paleoanthropology may justifiably question whether their scientific predecessors have bequeathed to them a body of evidence that accurately reflects the truth, as far as it can be known by empirical methods, about human origins and antiquity. They should carefully consider the fact that the evidence that has come down to them has been selected from a larger body of evidence according to the criteria established by persons such as Hrdlicka. One purpose of this book is to acquaint modern students of paleoanthropology with that larger body of evidence and allow them to make their own decisions about the worth of the portion of it that was rejected.

It is abundantly clear that Hrdlicka harbored a strong prejudice that any reputedly ancient human remains must display primitive features. The *Diprotomo* skull from the dry dock excavation in the harbor of Buenos Aires did not display such features. Therefore, according to Hrdlicka, it could not possibly be as ancient as the Early Pleistocene stratum in which it was discovered. Willis, in his role as Hrdlicka's geological assistant, offered some purely speculative alternative explanations about how the skull may have found its way into the formation.

Of course, Ameghino had his own prejudices. Like Hrdlicka, he was committed to evolutionary ideas, but whereas Hrdlicka believed that *Homo sapiens* had evolved in the Old World and only recently emigrated to the Americas, Ameghino believed man had evolved in South America. Therefore, Ameghino had wanted his *Diprotomo* to be appropriately primitive for its Early Pliocene age (Early Pleistocene by modern reckoning). Hrdlicka, however, showed the skull was actually not different from that of *Homo sapiens sapiens*.

Putting aside prejudice and preconception, it seems that the bare facts, as far as we can ascertain them, support the view that human beings physiologically indistinguishable from *Homo sapiens*

sapiens were present in Argentina during the Early Pleistocene. This supposition, although in clear contradiction to presently accepted accounts of human evolution, fits in quite well with the overwhelming mass of evidence detailed in the preceding chapters.

Before moving on, let us consider another South American find with unsettling implications for current thinking about human evolution in general and the populating of the New World in particular.

#### 6.1.6 The Lagoa Santa Calotte

In 1970, Canadian archeologist Alan Lyle Bryan found a highly mineralized calotte (skullcap) with "very thick walls and exceptionally heavy brow ridges" in a paleontological collection from caves in the Lagoa Santa region of Brazil. This skullcap could not be given a date, since the cave excavations had not been stratigraphically controlled, but the fossil's morphology was reminiscent of *Homo erectus*. Bryan stated that he left the skullcap in a local museum, but unfortunately it was later lost. When Bryan (1978) showed photographs of the skullcap to several American physical anthropologists, they were unable to believe it could have come from the Americas, and proposed that it was either a fake, a cast, or possibly a skullcap from Europe that had somehow been introduced into the Brazilian collection examined by Bryan.

But Bryan countered that both he and his wife, who also saw the skullcap, had abundant experience with human fossil bones. And they were both quite sure that the skullcap could not have been a fake or a cast—it was a genuine, highly fossilized human skullcap.

That the Lagoa Santa calotte was not a European fossil, accidentally introduced into the Brazilian collection, was supported, said Bryan, by the fact that it differed in several important measurements from known European skulls.

Also, it was similar to other skulls found in the Sumidouro cave in the Lagoa Santa region during the 1930s. Bryan (1978) reported that pieces of similar skulls were found more recently in the same cave and were being studied by Marília Carvalho de Mello e Alvim at the National Museum in Rio de Janeiro.

What is the significance of the Lagoa Santa calotte? The presence of hominids with *Homo erectus* features in Brazil at any time in the past is highly anomalous. Paleoanthropologists holding standard views say that only anatomically modern humans ever came to the Americas. The methodology of science allows for views to change, but the kind of change inherent in accepting the presence of *Homo erectus* in the New World would be revolutionary.

Of course, there are now a few paleoanthropologists who propose that *Homo erectus* was responsible for the crude stone tools at sites such as Toca da Esperanga in Brazil (Section 3.8.4) and Calico in California (Section 3.8.3). If the view that *Homo erectus* was responsible for tools at certain very ancient sites in the Americas were to become more widely accepted, this might have the beneficial effect of encouraging the far more radical changes in view that would be

required to accommodate the evidence for the presence of anatomically modern humans in the early Quaternary and Tertiary.

Finally, we wonder how such an important fossil as the Lagoa Santa skullcap could have been lost in the museum where it was being kept. A similar thing happened to the postcranial portion of the skeleton discovered by H. Reck at Olduvai Gorge (Section 11.1.5). In the case of Bryan's and Reck's discoveries, we at least had a chance to hear about them before they disappeared. But we suspect that other fossils have escaped our attention because they were misplaced in museums or were perhaps intentionally discarded—without report.

## 6.2 Fossil Human Remains from Tertiary Formations

Having reviewed human skeletal remains from the Middle and Early Pleistocene, we shall now consider discoveries from the Tertiary. Of course, modern authorities, almost without exception, are convinced there were no humans in the Tertiary. In *Fossil Men*, Boule and Vallois (1957, p. 108), in predicting what sort of fossils might turn up to fill the gaps in the record of human ancestry, said: "Even in the Pliocene, what we shall meet will no longer be— or rather, will not yet be—true Hominids. They will be the ancestors of the Prehominians, the ancestors of the Australopithecines— or even these Australopithecines themselves—all of them forms so apelike that to call them human would be to give this term an extension that would deprive it of all logical meaning." Here we have yet another example of evolutionary preconceptions dictating what kind of evidence is safely discoverable.

### 6.2.1 The Foxhall Jaw (late Pliocene)

We have already discussed J. Reid Moir's reports about stone implements and hearths discovered at Foxhall, England, in the Late Pliocene Red Crag formation (Section 3.3.4). Earlier, in 1855, a human jaw was discovered at Foxhall by workers digging for coprolites (phosphate-rich nodules) in a quarry on Mr. Law's farm.

John Taylor, the town druggist, purchased the Foxhall Jaw (Figure 6.2) from a workman who wanted a glass of beer, and

Taylor called it to the attention of Robert H. Collyer, an American physician then residing in London. Collyer, having acquired the fossil, visited the quarry on Mr. Law's farm and noted that the coprolite bed, from which the jaw was said to have been taken, was 16 feet below the surface. The condition of the jaw, thoroughly infiltrated with iron oxide, was consistent with incorporation in the coprolite bed. Collyer said that the Foxhall jaw was "the oldest relic of the human animal in existence" (Osborn 1921, p. 567). The 16-foot level at Foxhall is the same from which Moir (1924, p. 647) later recovered stone tools and signs of fire. Anything found at this level, considered an old land surface, would be at least 2.5 million years old.

Aware that he was in the possession of a fossil of great significance, Collyer brought the jaw to the curator of the Royal College of Surgeons, who suggested that he show it to Richard Owen. Collyer

delivered the jaw to Owen. who kept it for two years without giving a report. In 1859. Collyer retrieved the jaw. and then took it, in turn. to Sir John Prestwich and Thomas Henry Huxley.

In April of 1863. Collyer displayed the fossil jaw at a meeting of the Ethnological Society of London. at which the prominent geologists Charles Lyell and Roderick Murchison were present. George Busk. a paleontologist. said at the meeting that the fossil bone from Foxhall was "the jaw of some old woman. perhaps from some Roman burial ground." but he later withdrew this skeptical statement (Osborn 1921. p. 567).

Huxley. who was also present at the meeting. visited Collyer the next morning to further examine the jaw. At that time. Huxley said that it was "most extraordinary." but in May of 1863 he wrote that the morphology of the bone did not indicate it belonged "to an extinct or aberrant race of mankind." adding that "the condition of the bone is not such as I should expect a crag fossil to be" (Osborn 1921. p. 568).

Figure 6.2. Human jaw discovered in 1855 in the Late Pliocene Red Crag formation at Foxhall. England (Osborn 1921, p. 568).

The jaw then passed into the hands of Hugh Falconer and eventually wound up in the possession of George Busk. who showed it to de Quatrefages and other French scientists. In July of 1863. Busk stated the jaw was of "very great antiquity" (Osborn 1921. p. 568) but not necessarily from the coprolite bed at Foxhall.

American paleontologist Henry Fairfield Osborn. writing in the 1920s about Moir's finds of flint tools in the same area where the Foxhall jaw was uncovered. wondered why the above-mentioned scientists did not take the trouble to visit the site. They disbelieved. said Osborn (1921. p. 568). "probably because the shape of the jaw was not primitive and the degree of mineralization was not such as positively to prove it a fossil. He [Collyer] had a chemical analysis made that showed that the jaw was largely mineralized, but retained 8 per cent of animal matter. " But Moir reported that chemical analysis of bones from the Red Crag demonstrated that many of them had up to 6.5 percent animal matter (Osborn 1921. p. 568).

After some time. the jaw mysteriously disappeared. as did Collyer himself. All that is now known of Collyer is that he was a graduate of the Berkshire School of Medicine. once located at Pittsfield. Massachusetts. and that he was a friend of Dr. Morton. who was a craniologist and member of the Academy of Natural Sciences of Philadelphia. All that remains of the jaw is a detailed drawing

made in 1867 by Collyer and the scant published record of the controversy surrounding it (Osborn 1921, p. 569).

The fossil jaw from the Red Crag at Foxhall is almost never mentioned by modern authorities, and those who do mention it are invariably scornful. For example, we find in *Fossil Men*, by Boule and Vallols (1957, p. 107), this statement: "It requires a total lack of critical sense to pay any heed to such a piece of evidence as this."

But, as we have often pointed out, many conventionally accepted bones and artifacts have been found by uneducated workers or in other dubious ways. For example, most of the *Homo erectus* finds from Java were made by unsupervised, paid native collectors (Section 7.3). And the Heidelberg *Homo erectus* jaw was found by German workmen, whose foreman later turned it over to scientists (Section 7.2).

If scientists can seriously consider these discoveries, then why can they not seriously consider the Foxhall jaw as well? One might object that the Java *Homo erectus* fossils and the Heidelberg *Homo erectus* jaw are still available for inspection, while the Foxhall Jaw has vanished. But the original Peking *Homo erectus* fossils disappeared from China during World War II (Section 9.1.12): yet they are still accepted as evidence for human evolution.

#### 6.2.2 Human Skeletons from Castenedolo, Italy (Middle Pliocene)

One of the more significant Tertiary finds turned up in Italy. Millions of years ago, during the Pliocene period, a warm sea washed the southern slopes of the Alps, depositing layers of coral and molluscs. Late in the summer of 1860, Professor Giuseppe Ragazzoni, a geologist and teacher at the Technical Institute of Brescia, traveled to the nearby locale of Castenedolo, about 10 kilometers (roughly 6 miles) southeast of Brescia, to gather fossil shells in the Pliocene strata exposed in a pit at the base of a low hill, the Colle de Vento (Figure 6.3).

Figure 6.3. Tin's section of the Colle de Vento, near Castenedolo, Italy (after Sergi 1884, p. 313), shows the general stratigraphic position of human skeletal remains found there. (1) The human fossils found by geologist G. Ragazzoni in 1860 lay on the bank of coral and shells, at a place where it was surmounted by Middle Pliocene blue clay, which was itself covered by red clay (ferreflo) washed from the top of the hill. (2) On January 2 and January 25, 1880, more human fossils,



representing three individuals (a man and two children), were found about 15 meters (49 feet) from the 1860 site. The bones lay on the bank of coral, and were covered by about 2 meters (7 feet) of Pliocene blue clay, surmounted by a red layer of ferretillo. (3) On February 16, 1880, the bones of a woman were found at a depth of 1 meter (3 feet) in the blue clay, which was overlain by a layer of yellow sand and a layer of bright red ferretillo. In all three cases, Ragazzoni looked for signs of burial and found none.

Ragazzoni (1880, p. 120) reported: "Searching along a bank of coral for shells, there came into my hand the top portion of a cranium, completely filled with pieces of coral cemented with the blue-green clay characteristic of that formation. Astonished. I continued the search, and in addition to the top portion of the cranium I found other bones of the thorax and limbs, which quite apparently belonged to an individual of the human species."

Ragazzoni took the bones to the geologists A. Stoppani and G. Curioni. According to Ragazzoni (1880, p. 121), their reaction was negative: "Not giving much credence to the circumstances of discovery, they expressed the opinion that the bones, instead of being those of a very ancient individual, were from a very recent burial in that terrain."

"I then threw the bones away," stated Ragazzoni (1880, p. 121), "not without regret, because I found them lying among the coral and marine shells, appearing, despite the views of the two able scientists, as if transported by the ocean waves and

covered with coral, shells, and clay.

But that was not the end of the story. Ragazzoni could not get out of his mind the idea that the bones he had found belonged to a human being who lived during the Pliocene. "Therefore," he wrote, "I returned a little later to the same site, and was able to find some more fragments of bone in the same condition as those first discovered" (Ragazzoni 1880, p. 121).

In 1875, Carlo Germani, on the advice of Ragazzoni, purchased land at Castenedolo for the purpose of selling the phosphate-rich shelly clay to local farmers for use as fertilizer. Ragazzoni stated (1880, p. 121): "I explained to Germani about the bones I had found, and strongly advised him to be vigilant while making his excavations and to show me any new human remains."

A few years later, Germani noticed some bones. Ragazzoni recalled (1880, p. 121): "In December of 1879, Germani made an excavation, about 15 meters [49 feet] from the first place, to the northwest, and on January 2, 1880 announced to me the discovery of human bones between the bank of coral and the overlying shelly clay. The next day, I went there with my assistant Vincenzo Fracassi, in order to remove the bones with my own hands. These were: pieces of the left parietal, fragments of the occipital, the left temporal, the front part of the lower jaw with a canine, two loose molars, a cervical vertebra, fragments of vertebrae and ribs, part of the ilium, pieces of

humerus, ulna, radius, femur, tibia, fibula; and a tarsal and two phalanges." More discoveries were to follow: "On the 25th of the same month. Carlo Germani brought me two fragments of lower jaw. and some teeth of smaller size and different shape than the first, found at a distance of 2 meters [7 feet] from them, but at the same depth. Uncertain whether they belonged to a young individual or to an anthropomorphic ape, I returned once more to Castenedolo with Signor Germani, and was able to collect: a great quantity of upper cranial fragments (which I suspected belonged to two individuals), the left orbital of the frontal, two parietals. a fragment of the upper jaw with two molars. other free teeth, and fragments of ribs and limbs. All of them were completely covered with and penetrated by the clay and small fragments of coral and shells, which removed any suspicion that the bones were those of persons buried in graves, and on the contrary confirmed the fact of their transport by the waves of the sea" (Ragazzoni 1880, p. 122).

Figure 6.4. This anatomically modern human skull (Sergi 1884, plate 1) was found in 1880, at Castenedolo, Italy. The stratum from which it was taken is assigned to the Astian stage of the Pliocene (Oakley 1980, p. 46). According to modern authorities (Harland et al. 1982, p. 110), the Astian belongs to the Middle Pliocene, which would give the skull an age of 3- 4 million years.

On February 16, Germani advised Ragazzoni that a complete skeleton was discovered. Ragazzoni (1880, p.

122) journeyed to the site and supervised the excavation, instructing the workmen to "use the greatest diligence so as to be able to ascertain as clearly and exactly as possible the reality of the facts." According to Ragazzoni (1880, p. 122), they "removed the strata successively from higher to lower, with the intent of exposing the entire skeleton." This was accomplished. About the remains, Ragazzoni (1880, pp. 122 -123) wrote: "The skeleton, slightly inclined to the southeast, appeared to have been subjected to a kind of pressure in an oblique direction from south to north by movement of the strata in which it was found: consequently. it was from the region of the pelvis that we recovered the majority of the ribs, which appeared to have been crushed from above. The cranium was bent somewhat to the right. The lower jaw was detached and the separated facial bones were encased in a mass of blue-green clay penetrating the cavity of the cranium. which presented a variety of fractures." The cranium. as restored by G. Sergi ( Figure 6.4), was indistinguishable from that of a modern woman

Ragazzoni (1880. p. 121) then stated: "I desired to make photographs. but the perverse winter weather prevented it. In spite of the bad weather. the next morning I returned to the site with my son Pietro. and resolved to remove the entire skeleton despite the icy rain. which did. however. by penetrating the clay. make it easier to take out the bones" Ragazzoni (1880. p.

123)wrote: 'Differing from the set of remains found in 1860. and the other two found earlier this year [1880 ]. the complete skeleton was found in the middle of the layer of blue clay. . over which passes a layer of medium yellow sand." The other skeletons were found lower in the blue clay. at the point where it meets the underlying bank of coral and shells (Figure 6.3). Ragazzoni (1880. p. 123) added: "The stratum of blue clay. which is over 1 meter [3 feet] thick. has preserved its uniform stratification. and does not show any sign of disturbance. In accordance with the Judgement of the excavator himself. who is not preoccupied with any preconceived ideas. the skeleton was very likely deposited in a kind of marine mud and not buried at a later time. for in this case one would have been able to detect traces of the overlying yellow sand and the iron-red clay called ferretto, which forms the top part of the hill. and which by successive flooding has washed down and covered the lower formations of conglomerate and sand that cover the shelly Subappennine blue clays. "

From the above statements by Ragazzoni. it appears that the finds of 1860 and 1880 (except the complete female skeleton) were made in places on the slope of the hill where the layer of yellow sand covering the blue clay had been stripped away by erosion (Sergi 1884. p. 314). The hill at Castenedolo (Figure 6.3) was approximately 25 meters (82 feet) high. The top layer was recent soil. Below that. on the summit of the hill. was the red ferretto. Next in sequence came layers of glacial deposits and conglomerate. Below these came several layers of sand and clay. Then came the above-mentioned yellow sand. followed by the blue Pliocene clay in which the skeletal remains were discovered. Ragazzoni (1880. p. 126) indicated that even in the places where the blue clay had been exposed. the rain had washed down a surface layer of red ferretto deposits. Thus. for all the fossil discoveries. a layer of bright red clay was apparently lying above the blue clay. Any burial would have certainly produced a noticeable mixing of different colored materials in the otherwise undisturbed blue clay layer. and Ragazzoni. a geologist. testified that there was no sign of such mixing.

Of course. one could always propose that the skeletons (other than the adult female skeleton) were buried in the blue clay at a time when the red hillwash was not present. But this is unlikely. The red ferretto at the top of the hill would. it seems. have been continually carried down the hillside by rain or melting snow. Only under some unusual circumstance would it not have been present over the blue clay. Also. the blue clay had its own stratification. any disturbance of which would have been noticed.

Ragazzoni (1880. p. 126) then dealt with another possible objection to his conclusion that the human bones from Castenedolo were as old as the Pliocene layer in which they were found. Perhaps streams had stripped away the layers covering the blue clay and penetrated part way into the blue clay itself. The human bones could then have been washed into hollows. and new material could have been deposited over them. This could explain why there were no signs of burial. But Ragazzoni (1880. p. 126) said that it was highly unlikely that the human fossils had been washed recently into the positions in which they were found: "The fossil remains discovered on January 2 and January 25 lay at a depth of approximately 2 meters. The bones were situated at the boundary between the bank of shells and coral and the overlying blue clay. They were dispersed,

as if scattered by the waves of the sea among the shells. The way they were situated allows one to entirely exclude any later mixing or disturbance of the strata." Ragazzoni (1880, p. 126) further stated: "The skeleton found on the 16th of February occurred at a depth of over 1 meter in the blue clay, which appeared to have covered it in a state of slow deposition." Slow deposition of the clay, which Ragazzoni (1880, p. 123) said was stratified, ruled out the hypothesis that the skeleton had recently been washed into the blue clay by a torrential stream. Ragazzoni (1880, p. 126) added that the blue clay "was also in such a condition as to exclude any rearrangement by human agency." At the place where this complete human skeleton was discovered, the blue clay was still covered by a layer of yellow sand and a layer of red /erreffo. The absence of any mixture of yellow and red materials in the blue clay eliminated the idea of recent intrusive burial.

Ragazzoni (1880, p. 126) concluded: "These facts demonstrate the existence of man in Lombardy during the Early Pliocene." He stated elsewhere in his report: 'To render it perfectly clear to anyone that the terrain in which the bones and skeleton were found belongs to the Early Pliocene. I thought it convenient to offer a sample of the fossils that exist there in abundance' (Ragazzoni 1880, p. 124). He then referred his readers to an accompanying illustration of fossil Pliocene shells. Geologists who examined the blue clay layer of the Colle de Vento at Castenedolo, including Professor G. B. Cacclamali, agreed that it belonged to the Astian stage of the Pliocene (Oakley 1980, p. 46). Modern opinion places the Astian in the Middle Pliocene (Harland et al. 1982, p. 110), which would give the discoveries from Castenedolo an age of about 3-4 million years.

In 1883, Professor Giuseppe Sergi, an anatomist from the University of Rome, visited Ragazzoni and personally examined the human remains at the Technical Institute of Brescia. After studying the bones, he determined they represented four individuals—an adult male, an adult female, and two children.

Sergi also visited the site at Castenedolo. He wrote (1884, p. 315): "I went there accompanied by Ragazzoni, on the 14th of April. The trench that had been excavated in 1880 was still there, and the strata were clearly visible in their geological succession. In order to see still better, we cut a fresh vertical section down to the bank of coral. . . . The terrain was undisturbed, and Professor Ragazzoni said to me that I was seeing the undisturbed clay just as he had found it when he extracted the skeletons. And what was true of the clay, was also true of the underlying strata, which were also found intact, with no sign of resorting." In his report, Sergi (1884, p. 315) also wrote: "What is, one might demand, the guarantee of the authenticity of a discovery of this type? I believe that any doubt can be removed if the person who made the discovery adopted the necessary methods and noted all the circumstances with due care and conscientiousness. Professor Ragazzoni is a geologist and was quite familiar with the stratigraphic conditions of that region, and all of Lombardy, and would have been able to immediately recognize any movement of the terrain or signs that the blue clay had been mixed with materials from the overlying strata.'

Sergi (1884, pp. 315-316) added: "It is especially noteworthy that the color and structure of the strata in question are quite different. If a hole had been excavated for a burial, then it would not have been refilled exactly as before. The clay from the upper surface layers, recognizable by its

intense red color, would have been mixed in. Such discoloration and disturbance of the strata would not have escaped the notice of even an ordinary person what to speak of a trained geologist. One may also note that we are not talking about just one small area from which the bones of a single individual were taken, but also of a larger area, many square meters in size, from which the remains of three other individuals, lying close together, were taken. If these latter three were burials why was there not observed any artificial displacement of the overlying strata? Were such signs of burial overlooked? That might have happened in the case of a single unexpected discovery, made too suddenly to properly observe the circumstances; but as we have seen, the excavations were planned in advance and carried out with all due caution at different times and in various conditions, allowing sufficient time for observation and examination. Signs of mixing of the strata may have been missed the first time, but certainly not the second, third, and fourth times."

Gabriel de Mortillet (1883, pp. 71-72) did not believe that the Castenedolo skeletons were truly of Pliocene antiquity. Responding to de Mortillet's negative opinion, Sergi (1884, p. 316) wrote: "De Mortillet, in connection with this discovery, did not attempt to dispute the fact that there was no sign of disturbance in the strata. He said, however, that this observation was not sufficient to rule out burial, because 'the action of the sea would have dispersed the bones of the skeletons.' We note, however, the presence of four individuals—two adults (a male and female) and two children, indicating a family shipwrecked on a Pliocene coastline. The bones of all of the skeletons—except for one—were in fact dispersed, which accounts for the fact that they were discovered at successive times, with the fragments found in diverse parts of an extensive area. These bones were found as if disseminated across a single flat surface. Not all of the bones were found for each individual, but some only, which doubtlessly means that the others were dispersed elsewhere. By some circumstance, the last skeleton happened to come to rest at one spot, where, as could be seen when it was excavated, it was covered by layers of sedimentary deposits. Professor Ragazzoni told me that he found the bones in a uniform, compact mass of clay, like a fly that happened to fall into soft soap, where it remained caught. "

Sergi (1884, pp. 314-315) noted: "the almost entirely preserved female skeleton was not found in a posture indicating ordinary burial, but overturned; I saw the front part of the skull with the face inside the posterior cavity, and all the head entangled in the greenish clay, from which I patiently separated it. The other parts of the skeleton were also like this, and I still have the vertebral column with the ribs in a mass of clay, and the bones of one hand in the same clay."

Sergi (1884, p. 316) concluded: "From all that I heard and saw, I came to the conclusion that the skeletons of Castenedolo are from the geological era to which the strata of blue clay and the marine shell bed may be referred, and they are an irrefutable document for the existence of man in the Tertiary epoch, man of a character fully human, and not a precursor." To Sergi, the Castenedolo skeletons suggested that the creatures responsible for the Tertiary flints and cut bones found by other researchers (Chapters 2-5) might have been fully human rather than apelike ancestors, as scientists such as de Mortillet had proposed.

Sergi pointed out that the scientific community had recognized the existence of human beings in the Pleistocene only after great controversy. "But no sooner than that fact was accepted," wrote Sergi (1884, p. 303), "human artifacts from the Tertiary began to appear. This development was, however, confronted with obstacles and opposition arising not only from the prejudice expected of common men, but also from prejudice within the scientific community. Science has no problem finding existing species of shells in strata millions of years old, and some living mammals are also represented in the Tertiary, but man himself, it is thought, must be quite recent."

Sergi (1884, pp. 303-304) stated: "There were presented at various academies and congresses the artifacts of Tertiary man, consisting of impressions, incisions, cuts, and scraping on bone and stone, including flints chipped by the hand of man, and there followed only negativity. And when there was no other reason to negate, it was simply said: 'I don't believe it.' The reporting of actual human remains—crania and other bones—was received with irony and rejected with dogmatic incredulity."

Sergi (1884, p. 304) then recounted how the artifacts of Tertiary man gradually won a degree of positive recognition: "One set of facts was not able to be rejected, although it took much time to be accepted, and that was the chipped flints discovered in Tertiary deposits at Thenay (Loire-et-Cher) by Bourgeois. At the congress at Paris in 1867, Bourgeois was not believed; but Worsae soon declared his support, and shortly thereafter de Mortillet and others did the same. At the congress in

Brussels in 1872, the question was discussed, and the adherents increased, This prepared the way for Rames, who discovered worked flint and quartzite in the conglomerates of Cantal at Aurillac."

Continuing his review, Sergi (1884, p. 304) said: "The Tertiary flints of Portugal also encountered great resistance, But C. Ribeiro did research of unequalled value and effect. Yet only at the congress of 1880 at Lisbon did his discoveries achieve complete recognition, especially after a commission of scientists, in the course of a visit to Monte Redondo at Ota, found a flint implement in place, still embedded in the conglomerate, Professor Bellucci had the fortune to make this discovery and report it." It is quite remarkable that most modern students of paleoanthropology are unaware of the sequence of discoveries discussed by Sergi.

Sergi (1884, p. 304) then stated: "Taking all this into consideration, it is possible to conclude and affirm without hesitation that man appeared not only in the Quaternary epoch, but that the signs of his existence certainly extend back into the Tertiary epoch,"

After pointing out that some scientists wanted to attribute the Tertiary flint implements and other artifacts to a hypothetical apelike human precursor, Sergi (1884, p. 305) wrote: "Therefore it became important to consider human skeletal remains, but no acceptable ones had been found, This is the reason for the negative opinions of de Mortillet and Hovelacque, But neither was there much fossil evidence to back up the proposed precursor of man," Java man, the first scientifically accepted ape-man, was not uncovered until 1891, seven years after Sergi presented the report we have been reviewing,

De Mortillet, it may be recalled, believed the fossil record showed that mammals displayed extensive and progressive evolutionary development from primitive forms in the Tertiary up to more advanced forms in the present. Accepting this sequence as a paleontological law, de Mortillet anticipated that any fossils of Tertiary human ancestors would be very primitive and apelike. It could not be otherwise,

But Sergi pointed out that some Tertiary mammals (such as the mastodon) had survived without much change well into the Quaternary (Pleistocene) in Italy and Spain. Also, Sergi (1884, p. 306) reported that in the United States geologists had discovered in Late Miocene formations some fossil wolf jaws that were indistinguishable from those of living wolves,

Sergi (1884, p. 309) therefore stated: "the tendency to reject, by reason of theoretical preconceptions, any discoveries that can demonstrate a human presence in the Tertiary is, I believe, a kind of scientific prejudice. Natural science should be stripped of this prejudice." This prejudice was, however, not overcome, and it persists today. Sergi (1884, p. 310) wrote: "By means of a despotic scientific prejudice, call it what you will, every discovery of human remains in the Pliocene has been discredited,"

But Sergi was not alone in his acceptance of Ragazzoni's discoveries at Castenedolo. De Quatrefages, familiar to us from our review of stone implements, also accepted them. Concerning the female skeleton uncovered at Castenedolo, he said in his book *Races Humaines*: "The deposit was removed in successive horizontal layers, and not the least trace was found of the beds having been mixed or disturbed" (Laing 1894, p. 371). De Quatrefages further stated: "there exists no serious reason for doubting the discovery of M. Ragazzoni, and ... if made in a Quaternary deposit no one would have thought of contesting its accuracy. Nothing, therefore, can be opposed to it but theoretical a priori objections, similar to those which so long repelled the existence of Quaternary man" (Laing 1893, p. 119),

In 1889, an additional human skeleton was discovered at Castenedolo. This find introduced an element of confusion about the discoveries of 1880,

Ragazzoni invited G. Sergi and A. Issel to examine the new skeleton, which had been found in an ancient oyster bed. Sergi (1912) reported that both he and Issel believed this new 1889 skeleton to be a recent intrusion into the Pliocene layers "because the almost intact skeleton lay on its back in a fissure of the oyster bed and showed signs of having been buried" (Cousins 1971, p. 53),

Issel (1889) therefore reported that this new skeleton was not of Pliocene age, but was much younger. Issel (1889, p. 109) concluded that the 1880 discoveries were also recent burials. Concerning the dispersal of the bones of some of the skeletons found in 1880, he suggested this might have been caused by agricultural work (Issel 1889, p. 109). In a footnote, Issel (1889, p. 109) claimed that Sergi agreed with him that none of the skeletons found at Castenedolo were of Pliocene age. For the scientific community, this apparently resolved the ongoing controversy.

But Sergi (1912) later wrote that Issel was mistaken. Despite his views on the 1889 skeleton, Sergi said he had never given up his conviction that the 1880 bones were Pliocene. "Today I declare that the one thing does not invalidate the other" (Cousins 1971, p. 54). Sergi (1912) then added: "In any case this new pseudo-discovery [of 1889] gave a decisive blow to the first, and from this a deeper silence, like that of a grave, fell on the Castenedolo man: I had neither heart nor reason to exhume him. . . Since then no one has spoken any more of the Castenedolo man [except to cast doubt upon him]" (Cousins 1971, p. 54).

A good example of the unfair treatment given to the Castenedolo finds may be found in Professor R. A. S. Macalister's Textbook of European Archaeology, written in 1921. Macalister (1921, p. 183) admitted that the Castenedolo finds "whatever we may think of them, have to be treated seriously." He noted that they were 'unearthed by a competent geologist, Ragazzoni . . . and examined by a competent anatomist, Sergi.' Still he could not accept their Pliocene age. Faced with the uncomfortable facts, Macalister (1921, p. 183) claimed 'there must be something wrong somewhere.' First of all the bones were anatomically modern. 'Now, if they really belonged to the stratum in which they were found.' wrote Macalister (1921, p. 184), 'this would imply an extraordinarily long standstill for evolution. It is much more likely that there is something amiss with the observations.' Macalister (1921, p. 185) also said: 'the acceptance of a Pliocene date for the Castenedolo skeletons would create so many Insoluble problems that we can hardly hesitate in choosing between the alternatives of adopting or rejecting their authenticity.' Here once more we find a scientist's preconceived ideas about evolution Influencing him to reject skeletal evidence that would otherwise be considered of good quality.

Equally unfriendly to Tertiary stone tools. Macalister (1921. p. 185) protested: 'On the one hand, we are asked to believe in eoliths; on the other hand we are introduced to highly advanced and intellectual people like those of Castenedolo. The two are Incompatible. The quest for Tertiary Man is a game at which the player must be fair; he cannot win both ways. Let him become an Eolithist if he see fit, but let him then give up all expectation of finding a Tertiary man with a fully-developed mental equipment. Or let him seek a Tertiary Man. but he must then throw his eoliths and all the rest of his ballast overboard.'

There is, however. no fundamental incompatibility between advanced intellectual capabilities and the manufacture of crude stone tools—even today tribal people in various parts of the world, with the same brain capacity as modern city dwellers, make such implements. Also, there is no reason why anatomically modern humans could not have coexisted with more apelike creatures in the Tertiary Just as humans today coexist with gorillas, chimpanzees, and gibbons.

Macalister cited Issel (1889) in support of his attempt to discredit the Castenedolo finds, apparently not aware that according to Sergi (1912) Issel's 1889 report discredited only the 1889 skeleton. For example, Macalister (1921, p. 184), referring to all of the Castenedolo finds. wrote: 'examination of the bones and their setting, by Issel of Geneva. revealed the fact that the strata were full of marine deposits, and that everything solid within them, except the human bones. shewed marine incrustations.' While it is true that Issel (1889, p. 108) reported that the bones of



the skeleton uncovered In 1889 were smooth and free of Incrustations. the same cannot be said of the earlier discoveries, which both Ragazzoni (1880, pp. 120. 122) and Sergi (1884, pp. 311, 312) said were Incrusted with blue Pliocene clay and pieces of shells and coral.

Another example of the unfair treatment given the Castenedolo discoveries is found in *Fossil Men*. In this book, Boule and Vallois (1957, p. 107) stated that "it seems certain that at Castenedolo. as at Savona [Section 6.2.3]. we are dealing with more or less recent burials." But In *Fossil Men*, Boule and Vallois devoted only one paragraph to Castenedolo, and did not mention the undisturbed layers lying over the skeletons or the scattered and incomplete state of some of the skeletons— Information that tends to rule out intrusive burial.

Boule and Vallois (1957. p. 107) noted: 'In 1889, the discovery of a new skeleton was the subject of an official report by Professor Issel, who then observed that the various fossils from this deposit were all impregnated with salt. with the sole exception of the human bones." Here Boule and Vallois implied that what was true of the bones found in 1889 was also true of the bones found previously. But In his 1889 report. Issel described in this connection only the bones found in 1889. In fact. Issel did not even mention the word salt. referring instead to marine incrustations—which were. as above mentioned, present on the bones found in 1860 and 1880.

Scientists have employed chemical and radiometric tests to deny a Pliocene age to the Castenedolo bones. K. P. Oakley (1980, p. 40) found the Castenedolo bones had a nitrogen content similar to that of bones from Late Pleistocene and Holocene Italian sites and thus concluded the Castenedolo bones were recent. But as previously mentioned. in connection with Galley Hill. the degree of nitrogen preservation in bone can vary widely from site to site, making such comparisons unreliable as age Indicators. The Castenedolo bones were found in clay, a substance known to preserve nitrogen-containing bone proteins.

The Castenedolo bones had a fluorine content that Oakley (1980, p. 42) considered relatively high for bones he thought were recent. Oakley explained this discrepancy by positing higher past levels of fluorine in the Castenedolo groundwater. But this was simply guesswork. The Castenedolo bones also had an unexpected high concentration of uranium, consistent with great age.

A carbon 14 test yielded an age of 958 years for some of the Castenedolo bones. But, as in the case of Galley Hill, the methods employed are now considered unreliable and the bones themselves were veiy likely contaminated with recent carbon, causing the test to yield a falsely young age. For a more detailed discussion of the chemical and radiometric testing of the Castenedolo bones, see Appendix 1.

The case of Castenedolo demonstrates the shortcomings of the methodology employed by paleoanthropologists. The initial attribution of a Pliocene age to the discoveries of 1860 and 1880 appears to have been amply Justified. The finds were made by a trained geologist. G. Ragazzoni, who carefully observed the stratigraphy at the site. He especially searched for signs of intrusive burial. and observed none. Ragazzoni duly reported his findings to his fellow scientists in scientific

Journals. But because the remains were modern in morphology they came under intense negative scrutiny. As Macalister put it, there had to be something wrong.

The account of human origins now dominant in the scientific community is the product of attitudes such as Macalister's. For the last century, the idea of progressive evolution of the human type from more apelike ancestors has guided the acceptance and rejection of evidence. Evidence that contradicts the idea of human evolution is carefully screened out. Therefore, when one reads textbooks about human evolution, one may think, "Well, the idea of human evolution must be true because all the evidence supports it." But such textbook presentations are misleading, for it is the unquestioned belief that humans did in fact evolve from apelike ancestors that has determined what evidence should be included and how it should be arranged and interpreted.

### 6.2.3 A Skeleton from Savona, Italy (Middle Pliocene)

We now turn our attention to another Pliocene find, made at Savona, a town on the Italian Riviera, about 30 miles west of Genoa. In the 1850s, a church was being built on a hill bearing the same name as the one at Castenedolo (Colle de Vento). During the construction, workmen discovered a human skeleton at the bottom of a trench 3 meters (10 feet) deep. De Mortillet (1883, p. 70) reported: "Its bones were found in their natural connection, encased in a very compact and characteristic Pliocene marl, which also contained many other fossils typical of the Astian stage of the Pliocene." This would make the Savona skeleton the same age as the Castenedolo skeletons—Middle Pliocene.

Arthur Issel communicated details of the Savona find to the members of the International Congress of Prehistoric Anthropology and Archeology at Paris in 1867. In favor of the authenticity of the discovery, Issel (1868) declared that "the man of Colle de Vento was contemporary with the strata in which he was found" (de Mortillet 1883, p. 70). Issel said it was unfortunate that a trained geologist was not present to confirm that the strata were undisturbed and that the human bones had been buried at the same time as the animals bones found at the same level.

De Mortillet (1883, pp. 70 -71), after mentioning that most of the skeleton was later lost, stated: "The bones that remain show that the individual was of small stature, much smaller than the present Ligurians. At first glance, the lower Jaw appears to have a special primitive character, but the more one examines it the more one sees that the effects are those of breaking or wear rather than actual features. One sees that its features are analogous to those of modern Jaws. The Pliocene strata are characterized by many marine shells, but they also contain terrestrial fossils including bones of rhinoceros as well as remains of plants. But the mammalian bones are scattered and separated, while the human bones preserve their natural connections. Does this not prove that instead of the remains of a human cadaver tossed in the waves of a Pliocene sea, we are simply in the presence of a later burial of undetermined date? Much desiring to clarify this question, Issel in 1874 began an excavation 1 meter [3 feet] distant from the foundation of the church, almost at the spot where the human bones were uncovered. He observed that at this point the Tertiary beds were completely intact, without a trace of disturbance. Unfortunately,

those members of the religious order owning the land stopped him from continuing the excavation. which had then reached the 2-meter [7-foot] level. "

In his report, Issel catalogued the remaining bones of the Savona skeleton: a fragment of the right parietal, some pieces of the Jaw, a fragment of humerus, a clavicle, the head of a femur, and some finger bones. Issel (1868, p. 77) pointed out that "the material embedded in the fractures of the bones is the same as that in the Pliocene strata." To Issel (1868, p. 78) the bones seemed "slightly different and smaller than those of modern man."

At the International Congress of Prehistoric Anthropology and Archeology at Bologna in 1871, Father Deo Gratias (D. Perrando), a priest who had been present at the time of the discovery of the human skeleton at Savona, gave a report indicating that it was not an intrusive burial. Deo Gratias, a student of paleontology, explained that in 1851 the sisters of the Misericordia of Savona had wanted to build a church next to their convent. G. B. Mogliolo undertook the work, under the direction of Giuseppe Cortese. Antonio Brilla, a sculptor and artist, assisted in the excavations. So, in addition to the workers, these three educated gentlemen regularly monitored the excavations. Brilla, in particular, was specifically looking for fossils.

At a depth of 3 meters (10 feet), the excavators discovered an object that Brilla thought might be a piece of ancient pottery, but it turned out to be part of a skull. Despite their astonishment, the gentlemen present did not report the find to professional scientists and allowed the workers to continue. They later uncovered a skeleton. The fragmentary remains were taken to Brilla's studio, Deo Gratias wanted them, but Brilla kept them to use as models for some of his works. Brilla did, however, eventually give Deo Gratias some pieces of bone. These would appear to be the bones listed by Issel. The rest of the bones were then lost.

Deo Gratias (1873, pp. 419-420) stated: "It is unfortunate an experienced naturalist was not there, but on the basis of testimony by Brilla and the workers who excavated the skeleton here is what is known. The body was discovered in an outstretched position, with the arms extending forward, the head slightly bent forward and down, the body very much elevated relative to the legs, like a man in the water. Can we suppose a body was buried in such a position? Is it not, on the contrary, the position of a body abandoned to the mercy of the water? The fact that the skeleton was found on the side of a rock in the bed of clay makes it probable that it was washed against this obstacle,"

Deo Gratias (1873, p. 419) further stated: "Had it been a burial we would expect to find the upper layers mixed with the lower. The upper layers contain white quartzite sands, The result of mixing would have been the definite lightening of a closely circumscribed region of the Pliocene clay sufficient to cause some doubts in the spectators that it was genuinely ancient, as they affirmed. The biggest and smallest cavities of the human bones are filled with compacted Pliocene clay. This could only have happened when the clay was in a muddy consistency, during Pliocene times," Deo Gratias pointed out that the layers of Pliocene clay, now hard and dry, were situated on a hill, which meant they would be well drained,

De Mortillet, and later Boule and Vallols (1957, p, 106), argued that since the mammal bones in the stratum were scattered, whereas the human bones were found in natural connection, this indicated that the latter must be a recent intrusive burial. But the following points all argue strongly against the intrusive burial hypothesis: (1) the lack of material from the higher stratum mixed in with that of the lower stratum; (2) the depth of 3 meters (10 feet)—rather deep for a burial, at least from the present land surface; (3) the position of the skeleton, face down when discovered,

How then do we explain the scattered mammal bones? The site was once covered by the shallow shoreline waters of a Pliocene sea, as shown by the presence of characteristic shells. Animals could have died on the land, and their isolated bones could have been washed into the sea and incorporated into the formation. The human bones, found in natural connection, could have come to rest in the same marine formation as a result of someone drowning there during the Pliocene. This combination of events accounts for the presence of a relatively complete human skeleton amid scattered animal bones, without recourse to the hypothesis of recent intrusive burial. Keep in mind that the posture of the skeleton, face down and with limbs outstretched, was like that of a drowned corpse rather than one deliberately buried,

The very infrequent references to the Savona skeleton in current textbooks are predictably negative, and just as predictably flawed in their presentation of the facts. For example, Boule and Vallols (1957, p, 106) claimed: "No stratigraphic study of the formation was made," This statement of theirs is, however, inaccurate, as can be seen from the above-mentioned reports, which established the Pliocene age and undisturbed condition of the layer containing the skeleton,

#### 6.2.4 A Human Vertebra from Monte Hermoso (Early Pliocene)

Having discussed the discovery of flint tools and signs of intentional use of fire at Monte Hermoso in Argentina (Section 5.1.1), we will now consider a human bone found there. Dr. F. Ameghino (1908, pp, 106 -107) reported: "The precursor of man who burned the pampas grass, who made fire in hearths, chipped flint implements, and burned and split the bones of animals he hunted, has also left some of his own fossil bones," He was speaking of a human atlas (the first, or topmost, vertebra of the spinal column) collected by Santiago Pozzi, an employee of the Museo de La Plata (F. Ameghino 1908, p, 174). According to Ameghino (1908, p, 107), the atlas was from the Pliocene Montehermosan formation at Monte Hermoso,

A. Hrdlicka wrote (1912, p, 346): "some time in the [eighteen] eighties (the exact date is not known), an employee of the Museo de La Plata made for that institution at Monte Hermoso a collection of fossils. Among these bones was found at the museum a humanlike atlas of subaverage size."

"When this atlas was seen by Senor Moreno, at that time the director of the La Plata Museum," wrote Hrdlicka (1912, p. 346), "it was still partially enveloped in yellowish or yellowish-brown earth." The Montehermosan is a yellow-brown loess. There are no other beds of that color at Monte Hermoso, according to a detailed description of the site stratigraphy compiled by geologist

Bailey Willis (Hrdlicka 1912, p. 362). As previously mentioned (Section 5.1.1), the Montehermosan dates back about 3-5 million years before the present, and belongs to the Early Pliocene (Marshall et al. 1982).

In a footnote, Hrdlicka (1912, p. 346) added: "Ameghino (*Tetraprothomo*, etc., p. 174) says that the specimen was 'still in a portion of the rock' but Senor Moreno expressly stated to the writer that it was in 'earth' which held together but was not solidified. Whether or not this earth was sandy can not now be definitely determined. The fact that later the bone was cleanly disengaged from the mass shows further that it could not have been in 'rock.' Roth speaks of the bone as having been enveloped in 'loess' (in Lehmann-Nitsche, *Nouvelles recherches*, etc., p. 386)." The Montehermosan is the only loess formation at Monte Hermoso (Hrdlicka 1912, p. 362).

Hrdlicka (1912, pp. 346-347) then traced the further history of the atlas: "Soon after its discovery the specimen was forgotten and lay unnoticed in the collections of the museum for many years, until finally it was observed accidentally by Santiago Roth, who freed it from the 'loess,' and seeing that the specimen appeared to be a human atlas of small size transferred it to the anthropologic collections of the institution. There again it lay for several years longer without receiving any special consideration, until a new discovery at Monte Hermoso attracted to it the attention of Ameghino. Through Lehmann-Nitsche Ameghino borrowed the specimen, studied it in detail, and published a description of it in his memoir on the *Tetraprothomo*, identifying the bone with that particular hypothetical genus of man's precursors. At the same time a study of the atlas was undertaken and published by Lehmann-Nitsche, who in turn attributed it to 'a Tertiary primate of Monte Hermoso, the *Homo neogaeus*.'" It may be noted that the Gibraltar skull lay for many years in the garrison museum before it was recognized as a Neanderthal specimen. Also, several *Homo erectus* femurs from Java were shipped to Holland in boxes of bones. They went unrecognized and uncataloged for several decades after they were unearthed, but are now listed in textbooks with other accepted finds. The number of similar cases could be expanded, the point being that scientists have become aware of many fully accepted fossil finds in the same way as the Monte Hermoso atlas.

At a later date, another bone turned up. Hrdlicka (1912, p. 347) wrote: "Sometime during the early years of the present century Carlos Ameghino discovered in the same barranca of Monte Hermoso a peculiar bone, which eventually was referred to a supposed ancient parental form of man. It was a portion of the fossil femur of a being which F. Ameghino identified as a very ancient forerunner of man, the *Tetraprothomo argentinus*.

Hrdlicka felt the femur belonged to something other than a human being. Hrdlicka (1912, p. 376) wrote: "The femur of the *Tetraprothomo* bears only a slight resemblance to that of man or the anthropoid apes, and but little greater to that of the lower monkeys. It presents no feature which would make obligatory or even possible its inclusion in the Primate class, but on the other hand it shows many features which approximate it to a distant family of mammals. The class of mammals with which the characteristics of the femur connect it most closely are the carnivores, and among these especially the cats."

As far as the atlas was concerned, Ameghino and others thought it displayed some primitive features, but extensive analysis by Hrdlicka (1912, p. 364) led him to conclude: 'The bone is submedium in size and rather massive, but is in every respect human. An extensive comparison with human and other mammalian atlases settles its human provenience beyond question. It is more or less distant morphologically from the atlases of all the anthropoid apes and still more so from those of the monkeys, while the atlases of the Camlvora and other mammals present such differences that a comparison becomes entirely superfluous." It is fairly obvious what Hrdlicka was trying to do. Ameghino had pointed to primitive features in the atlas, with a view toward attributing it to a precursor of the modern human race, a species that lived in Argentina during the Early Miocene (the Early Pliocene according to modern estimates). For Hrdlicka, it was sufficient to show that the bone was completely modern in character. Hrdlicka was an evolutionist and believed the laws of biological development required that the human form should, as we proceed back in time, become more and more primitive. If the bone was of the fully modern human type, then no matter what layer it was found in, it had to be of recent origin. There was no doubt about it. Such a bone's presence in an ancient stratum always could be, indeed had to be, explained as some kind of intrusion.

Along these lines, Hrdlicka (1912, p. 384) wrote about the atlas: "Its extraction is problematical, but even if found in quite intimate relation with the real Monte Hermosean loess, it is not necessarily old. It may well have been derived from the dune above the Monte Hermoso barrancas, which, as shown before, contain numerous traces of the modern native of the coast, and which fall from the crumbling edge above the ledges into pockets of the lower ancient formation." But there is another possible explanation: human beings of the modern physiological type were living over 3 million years ago in Argentina. This is supported by the fact that the atlas showed signs of having been thoroughly embedded in sediments from the Montehermosan formation.

All in all, Hrdlicka (1912, p. 384) felt that the Monte Hermoso atlas was worthy of being "dropped of necessity into obscurity." That is exactly what happened. The atlas was dropped into obscurity. It had to be done. Otherwise, Hrdlicka's claim that humans only recently entered the Americas would have been placed on very shaky ground. Certainly there are many who will insist that the Monte Hennoso atlas remain in the obscurity into which it was of necessity dropped. Evidence for a fully human presence 3 million or more years ago, in Argentina of all places, is still not welcome in mainstream paleoanthropology.

#### 6.2.5 A Jaw Fragment from Miramar, Argentina (late Pliocene)

Early in the twentieth century, fossil human skeletal remains were found in the Late Pliocene Chapadmalalan formation at Miramar, Argentina. Previously, stone tools and a mammalian bone with an arrow head embedded in it had been discovered at this site (Section 5.2). Hugo Obermaier (1924, p. 306) wrote: 'in 1921 M. A. Vignati discovered further human remains at Miramar, not far from Buenos Aires, consisting of a fragment of lower jaw with two molars still in it. According to Vignati it came from the geologic formation of Chapalmalal." We have not been able to locate

VignaU's original report on this find, potentially an important paleoanthropological discovery. But we have found a report about the Jaw fragment by another South American scientist, E. Boman.

Boman (1921, pp. 341-342) stated: 'From the publication of my article in the Journal de la Societe des Amercanistes de Paris up to the time of my visit to Miramar last year, some other objects have been discovered there. Those that have attracted the most attention are two human lower molars (2nd and 3rd right), which were adhering to a small fragment of mandible. Parodi found them, according to the report, embedded in the barranca, at great depth in the Chapadmalalan strata, at about the level of the sea. Parodi extracted the object from its position and took it to town, where he showed it to Dr. F. Kuhn, who at the time he saw It concluded it was of some importance. Kuhn advised him to inform Carlos Ameghln, who came to Miramar to take possession of the teeth."

The discovery appears quite significant—human fossil remains found in the Chapadmalalan—a formation which Anderson (1984, p. 41) gave an age of 2.5-3.0 million years and which Marshall et al. (1982, p. 1352) gave an age of 2.0-3.0 million years.

Boman, however, treated this evidence in a manner typical of those sharing his views. He stated: 'The newspapers published bombastic articles about 'the most ancient human remains in the world.' But all who examined the molars found them to be identical to the corresponding molars of modern human beings. Human beings existing at that time would have been contemporary with their 'precursor,' the mysterious Tea-a^vthomo" (Boman 1921, pp. 341-342). In the opinion of Florentino Ameghln, Telaprophomo was a primitive apelike ancestor of anatomically modern humans, which he thought evolved in South America.

Boman took it for granted that the fully human nature of the Miramar Jaw fragment unequivocally insured its recent date. But nothing Boman said excludes the possibility that the Miramar fossil demonstrates a fully human presence in the Pliocene of Argentina.

Boman mockingly suggested that the Miramar Jaw fragment, if one could imagine it was genuinely old, would contradict Florentino Ameghln's theory that human beings evolved from apelike ancestors in Argentina, a theory Boman regarded as fanciful. But Boman neglected the possibility that the discovery of a fully human Jaw in the Chapadmalalan formation might contradict his own views, and those of others, who believed that Homo sapiens evolved quite recently. The presence of Homo sapiens 2-3 million years ago in Argentina would have invalidated the entire story of human evolution then, and now, accepted as fact.

#### 6.2.6 Human skeletal remains from the California Gold country (Pliocene to Eocene)

In the preceding chapter (Section 5.5), we discussed the numerous stone implements discovered in the auriferous gravels of the Sierra Nevada Mountains of California. Some of these implements were found beneath the latite cap of Table Mountain in Tuolumne County. We noted that this latite cap has yielded radiometric dates of 9 million years, while the prevolcanic auriferous gravels lying Just above the bedrock have yielded dates of 33-55 million years. Now we will describe human skeletal remains that have been discovered beneath the latite cap of Tuolumne Table

Mountain, and elsewhere in California. We will begin our review with the Calaveras skull, the history of which is colorful but inconclusive. The accounts of the remaining discoveries, although less entertaining, provide better evidence for a human presence in the Tertiary.

#### 6.2.6.1 The Calaveras Skull

The most notorious fossil discovered in the Gold Rush mines of California was the Calaveras skull. The State Geologist of California, J. D. Whitney (1880, pp. 267-273), described the circumstances surrounding this find.

In February 1866, Mr. Mattison, the principal owner of the mine on Bald Hill, near Angels Creek, removed this skull from a layer of gravel 130 feet below the surface. The gravel was near the bedrock, underneath several distinct layers of volcanic material. Volcanic eruptions began in this region during the Oligocene, continued through the Miocene, and ended in the Pliocene (Clark 1979, p. 147).

Since the skull occurred near the bottom of the sequence of interspersed gravel and lava layers at Bald Hill, it would seem likely that the gravel in which the skull was found was older than the Pliocene, perhaps much older.

After finding the skull, Mattison later carried it to Mr. Scribner, an agent of Wells, Fargo and Co.'s Express at Angels. Mr. Scribner's clerk, Mr. Matthews, cleaned off part of the incrustations covering most of the fossil. Upon recognizing that it was part of a human skull, he sent it to Dr. Jones, who lived in the nearby village of Murphy's and was an enthusiastic collector of such items. Then Dr. Jones wrote to the office of the Geological Survey in San Francisco, and after receiving a reply, he forwarded the skull to this office, where it was examined by Whitney. Whitney at once made the journey to Murphy's and Angels, where he personally questioned Mr. Mattison, who confirmed the report that was given by Dr. Jones. Both Scribner and Jones were personally known to Whitney and were regarded by him as trustworthy.

On July 16, 1866, Whitney presented to the California Academy of Sciences a report on the Calaveras skull, affirming that it was found in Pliocene strata. The skull caused a great sensation in America.

According to Whitney (1880, p. 270), "the religious press in this country took the matter up . . . and were quite unanimous in declaring the Calaveras skull to be a 'hoax,'" One paper reported: "We believe the whole story worthy of no scientific credence, and are also more fully established in this belief by the declaration of an able Congregationalist minister, who has preached some time in the region, and who told us that the miners freely told him that they purposely got up the whole affair as a joke on Professor Whitney." Another religious paper (the Congregationalist, Sept. 27, 1867) reported that the skull 'had been placed [in the mine] by some mischievous miners as a hoax upon one of their own number, who was of an anti-Scriptural and geologic turn of mind. He swallowed the hoax and carried the news to Professor Whitney, who thereupon secured the skull for the State Museum" (Whitney 1880, p. 270).



The image of the rough and ready humorists of the rip roaring Gold Rush mining camps having a goodjoke at the expense of a stuffy geologist is reflected in the following verses excerpted from Bret Harte's poem 'The Pliocene Skull' (Harte 1912, pp. 280-281):

"Speak, Oman, less recent! FragmentalY fossil! Primal pioneer ofpliocene formation,

Hid in lowest drifts below the earliest stratum Of volcanic tufa!

"Older than the beasts, the oldest Paleothenum: Older than the trees, the oldest Cryptogami:  
Older than the hills, those infantile eruptions

Of earth's epidermis!

"Eo—Mio—Plio—whatsoe 'er the 'cene' was That those vacant sockets filled with awe and  
wonder,—

Whether shores Devonian or Silurian beaches,—

Tell us thy strange stoDIY!

"Speak, thoi awful vestige of the earth's creation, Solitary fragment of remains organic!

Tell the wondrous secret ofthy past existence,—

Speak! thou oldest primate!"

Even as!ggazed, a thrill of the maxilla,

And a iaterai movement of the condyloid process, With post-pliocene sounds of healthy  
mastication. Ground the teeth together.

And from that imperfect dental exhibition. Stained with exp^ss juices of the weed nicotine,

Came these hollow accents, blent with softer murmurs Of expectora tion:

" Which my name is Bowers. and my crust was busted

Falling down a shaft in Calaveras County; Biit I'd take it kindly ifyou'd send the pieces Home to old  
Missoun! "

Whitney noted that the hoax stories did not arise until after his discovery was publicized widely in newspapers. Some of the hoax stories were propagated not by Western poets and preachers but by scientists such as William H. Holmes.

Holmes. an anthropologist. worked for the Smithsonian Institution, founded in 1846 with a half-million dollar bequest from James Smithson, an English scientist and inventor. As late as the 1890s, the Calaveras skull was still a matter of great interest and debate within the scientific community. Holmes, who tended to doubt the skull's Tertiary age, wanted to put the matter to rest, once and

for all. During a visit to Calaveras County, he gathered testimony from some people who were acquainted with Mr. Scribner and Dr. Jones, and this testimony raised the possibility that the skull examined by Whitney was not a genuine Tertiary fossil (Holmes 1899, pp. 459- 464).

Mr. J. L. Sperry, the keeper of the lone hotel in Murphy's, recalled that one day he had seen Dr. Jones. whose office faced the hotel, come out shouting and hurl a broken skull into the street. Sperry asked Jones what the fuss was about. Jones explained that he felt he had been the victim of a practical Joke by Scribner, who had sent him a supposedly ancient skull that now appeared to be a fake. But then Jones reconsidered the matter, picked up the skull, and later sent it to Whitney (Holmes 1899, p. 459).

Furthermore, in 1908, William J. Sinclair, a California archeologist, reported receiving an article by Rev. W. H. Dyer from the Tuolumne Independent of September 14, 1901. In this article (Sinclair 1908, p. 128), Dyer stated that he had been present when Mr. Scribner and two friends retold "the story of the skull, which they had planted deep in the bottom of the shaft where it astonished the miner, the curious public and the wondering scientists." Dyer later told Sinclair he had learned from Scribner's sister that his relatives "have long known as aJoke of his. the planting of a skull in a mine" (Sinclair 1908, p. 129).

But there are many different sides to the story. Holmes reported the efforts of Dr. A. S. Hudson to solve the Calaveras mystery. In 1883, Dr. Hudson received a letter from Dr. John Walker of Sonora. In this letter, Walker related how he had tried to convince J. D. Whitney that the Calaveras skull had originally been found in an Indian grave at Salt Spring Valley and not in Mattison's mine on Bald Mountain. Walker subscribed to the view that the whole incident was "a fabrication and aJoke" (Holmes 1899, p. 460). Hudson visited Walker, but found he had little evidence to back up his claims.

Hudson then went to Angels to talk to Scribner, the alleged prankster, who, according to Holmes, "assured him that Dr. Walker was wrong, and that no deception whatever had been practiced" (Holmes 1899, p. 460). Dr. Hudson then interviewed Mattison and his wife, and they confirmed that he had brought the incrustated skull home from his mine, where he had found it at a depth of 128 feet. It had remained in the Mattison household for a year. When shown a picture of the skull from Whitney's book, Mrs. Mattison recognized the skull as the same one she had kept for a year (Holmes 1899, p. 461). Feeling "perplexed and discouraged" by the seemingly "incomplete and incoherent" stories, Dr. Hudson returned to his office (Holmes 1899, p. 461).

Two weeks later, Scribner appeared and gave more information. Hudson wrote: "It seems, as time went on. Mrs. Mattison, an orderly housekeeper, began to take a dislike to the untidy thing—an unwashed dead head in her house—and made a complaint. It was more in the way than of use or ornament, and she decided to get rid of it. Thereupon her husband, like a proper acquiescing partner in life, carried it to Mr. Scribner's store" (Holmes 1899, p. 461).

Scribner related to Hudson that his partner, Mr. Henry Matthews, was angry at Dr. Jones for giving him some unpleasant medicine. Therefore, as a kind of a joke, Matthews sent the skull in a sack with some lumps of rock and petrified wood to the office of Dr. Jones, who was known to be a collector of geological curiosities. Dr. Jones, apparently thinking the skull to be recent and of little value, is then said to have tossed the skull out into his back yard, where it remained for several months. Then, while visiting Dr. Jones, Mr. Mattison saw the object, and upon recognizing it stated it was the same skull he had removed from his mine. Appreciating the relic in a new light, Dr. Jones then forwarded it to Whitney. So according to

Dr. Hudson, there was some joking involved, but the motive was 'not to play upon the spirit of scientific inquiry' but rather an attempt by Mr. Matthews to get even with Dr. Jones (Holmes 1899, p. 463).

Additional stories open up the possibility that the skull was exchanged with another one while it was at Mr. Scribner's store. Holmes spoke with George Stickle, the postmaster at Angels Camp, who told him that the Calaveras skull had actually been brought to him by Mr. J. L. Boone, from an Indian burial place in Salt Spring Valley, 12 miles from Angels Camp. The likelihood of such a thing happening cannot be easily dismissed. As Holmes (1899, p. 463) noted: 'There were ancient skulls in plenty in this region in early times, and the valley and the county received their name Calaveras—which in Spanish signifies skulls—from this circumstance. The Indians of the high sierra do not bury their dead, but cast them into pits, caverns, holes in the rock, and deep gorges. . . . Skulls were plentiful at Angels in those days. '

After remaining in his store for a few weeks, said Stickle, the skull fell into the hands of Scribner and his fun-loving friends, who were always pulling practical jokes on each other (Holmes 1899, p. 463). Stickle also testified that the skull taken from Mattison's mine was whole and white in color, and did not at all resemble the skull sent by Dr. Jones to Whitney. Yet Dr. Hudson reported that when Mrs. Mattison was shown a photograph of the Calaveras skull she identified it as the same one she had kept in her home. These stories are rather sketchy and incomplete, but at any rate there appears to be some doubt about the real age of the skull examined by Whitney.

After visiting Calaveras county, Holmes (1899, p. 469) examined the actual Calaveras skull at the Peabody Museum in Cambridge, Massachusetts, and concluded that "the skull was never carried and broken in a Tertiary torrent, that it never came from the old gravels in the Mattison mine, and that it does not in any way represent a Tertiary race of men." Some testimony supporting this conclusion comes from persons who examined the matrix of pebbles and earth in which the Calaveras skull had been discovered. Dr. F. W. Putnam of Harvard University's Peabody Museum of Natural History testified: "Had it been taken from the shaft there probably would have been some trace of gravel, such as is found in the beds through which the shaft was sunk, mixed with the materials taken from the skull by Professors Whitney and Wyman, but no such gravel has been found in the several examinations which have been made of the matrix" (Sinclair 1908, p. 129). Professor William J. Sinclair of the University of California also personally examined the matrix and concluded that it "is not strictly a gravel" and that "the material is dissimilar in every respect to

either of the gravels exposed on Bald Hill. In every respect it is comparable to a cave breccia" (1908, p. 126). A breccia is a deposit of various kinds of stone fragments mixed in a matrix of sand or clay. Sinclair believed that tiny fragments of bone, belonging to humans and small mammals, found adhering to the skull, along with a decorative bead found inside it, all reported by Whitney, were evidence of a recent cave origin.

On the other hand, Holmes (1899, p. 467) reported: "Dr. D. H. Dall states that while in San Francisco in 1866, he compared the material attached to the skull with portions of the gravel from the mine and that they were alike in all essentials." And W. O. Ayres (1882, p. 853), writing in the *American Naturalist*, stated: "I saw it and examined it carefully at the time when it first reached Professor Whitney's hands. It was not only incrustated with sand and gravel, but its cavities were crowded with the same material; and that material was of a peculiar sort, a sort which I had occasion to know thoroughly. It was the common

'cement' or 'dirt' of the miners; that known in books as the auriferous gravel." Ayres, a competent observer, intimately familiar with the region, should have been able to distinguish a recent cave breccia from Pliocene or Eocene auriferous gravels.

But even if it were true that some auriferous gravel was adhering to the skull, that would not have satisfied Holmes (1899, p. 467), who stated that "the peculiar agglomeration of earth, pebbles, and bones is readily explained by referring to the conditions existing in the limestone caverns and crevices of the region where the calcareous accretions bind together bones, gravel (very generally present), cave earth, and whatever happens to be properly associated, in just such a manner as that illustrated in the specimen under discussion." Yet if we prefer to listen to Ayres (1882, p. 853), we learn: "It has been said that it is a modern skull which has been incrustated after a few years of interment. This assertion, however, is never made by anyone knowing the region. The gravel has not the slightest tendency toward an action of that sort. ... the hollows of the skull were crowded with the solidified and cemented sand, in such a way as they could have been only by its being driven into them in a semi-fluid mass, a condition the gravels have never had since they were first laid down. "

Whitney (1880, p. 271), in his original description of the fossil, observed that the Calaveras skull was highly fossilized. This is certainly consistent with great age, however, as Holmes pointed out, it is also true that bones can become fossilized over the course of a few hundred or thousand years. Yet geologist George Becker (1891, p. 195) reported: "I find that many good Judges are fully persuaded of the authenticity of the Calaveras skull, and Messrs. Clarence King, O. C. Marsh, F. W. Putnam, and W.H. Dall have each assured me that this bone was found in place in the gravel beneath the lava." Becker added that this statement was made with the permission of the authorities named. Clarence King, as mentioned previously, was a famous geologist attached to the U.S. Geological Survey. O. C. Marsh, a paleontologist, was one of the pioneer dinosaur fossil hunters, and served as president of the National Academy of Sciences from 1883 to 1895. But F. W. Putnam of Harvard's Peabody Museum, as we have seen, later changed his mind, saying that the matrix of the skull appeared to be a cave deposit.

Can it really be said with certainty that the Calaveras skull was either genuine or a hoax? The evidence is so contradictory and confusing that although the skull could have come from an Indian burial cave we might regard with suspicion anyone who comes forward with any kind of definite conclusion. The reader may pause to contemplate what steps one would take to make one's own determination of the true age of the Calaveras skull.

It should, however, be kept in mind that the Calaveras skull was not an isolated discovery. Great numbers of stone implements were found in nearby deposits of similar age. And, as we shall see in the next sections of this chapter, additional human skeletal remains were also uncovered in the same region. The reports of these discoveries, although brief, are more satisfactory than the reports concerning the Calaveras skull. The reports are simpler, providing no basis for charges of fraud —unless one wants to argue that California gold miners suffered from a massive paleoanthropological hoax obsession.

Similar discoveries, although not quite as old as those from California, were made elsewhere in the world, as at Castenedolo. In light of this, the Calaveras skull cannot be dismissed without the most careful consideration. As Sir Arthur Keith (1928, p. 471) put it: 'The story of the Calaveras skull. . . cannot be passed over. It is the bogey' which haunts the student of early man . . . taxing the powers of belief of every expert almost to the breaking point."

Furthermore, it seems the evolutionary preconceptions of Holmes, Hrdlicka, and others were partly responsible for the scientific community's rejection of the Calaveras skull, as well as other anomalously old human fossils. We have documented the opinions of Holmes and Hrdlicka in our discussion of the stone implements discovered in the California auriferous gravels (Section 5.5.13) and in our discussion of the Buenos Aires skull (6.1.5). Concerning the Calaveras skull, James Southall (1882, p. 199) said, in a paper delivered at the Victoria Institute in London. England: 'If the human skull was exactly the same at the beginning of the Pliocene, or the close of the Miocene, that it is now: on the theory of evolution, how shall we explain the absence of all progress or change? and what margin of time is there for man's development from the generalised lemurs of the Eocene? There is no doubt whatever that the confirmation of Professor Whitney's opinion as to the age of this skull would be fatal to the evolution theory."

In this regard, Laing (1894, p. 389) wrote: 'if we accept ... the skulls of Castenedolo [sic] and Calaveras, which are supported by such extremely strong evidence, it would seem that as we recede in time, instead of getting nearer to the 'missing link,' we get further from it. This, and this alone, throws doubt on evidence which would otherwise seem to be irresistible." In other words, the fact that the discoveries violated evolutionary expectations was sufficient to overrule all other testimony.

It is indeed true that J D. Whitney's reports of skeletal remains and artifacts, which imply that anatomically modern human beings existed in California over 9 million years ago, do call into question the theory of human evolution, as presently understood. How can humans not have changed over that vast period of time? Whitney was certainly aware of the implications of his findings. Writing 11 years before the discovery of the Java ape-man, *Pithecanthropus erectus*, he

stated: "All the investigations of geologists and ethnologists thus far have failed to obtain satisfactory evidence of the existence at a previous epoch of any type of being connecting man with the inferior animals, or decidedly lower in grade than races now inhabiting portions of the earth, or anything that we fail to recognize instantly as man" (Whitney 1880, p. 286). More explicitly, Whitney concluded: "Man, thus far, is nothing but man, whether found in Pliocene, Post-pliocene, or recent formations" (1880, p. 288). He did admit the chance that some precursor of modern humanity might someday be found in strata older than Pliocene, but his tone in presenting this possibility suggested a challenge to his opponents rather than a fervent and soon-to-be-fulfilled hope of his own.

In the decades following Whitney's statements, fossils displaying varying degrees of apelike and humanlike features did in fact come to light in Pliocene and post-Pliocene formations. But their discovery does not, however, automatically eliminate the many remains of anatomically modern humans discovered in the same, and earlier, formations. Nevertheless, the anomalously old human discoveries were eliminated by advocates of the recent evolution of the modern human type. If this elimination had not occurred, it would not have been possible to speak of the newly discovered ape-man-like creatures as precursors of *Homo sapiens sapiens*, human beings of the modern type.

#### 6.2.6.2 Captain Akey's Report

On January 1, 1873, the president of the Boston Society of Natural History read extracts from a letter by Dr. C. F. Winslow about a discovery of human bones at Table Mountain in Tuolumne County. The find was made in 1855 or 1856, and the details of it were communicated to Winslow by Capt. Akey, who had witnessed it. The discovery took place about 10 years before J. D. Whitney first reported on the famous Calaveras skull. Regarding the finds Whitney described, Winslow (1873, p. 257) wrote: 'some distrust as to their identity has been entertained in certain scientific circles. The verification of such discoveries is all important to the interests of science, and I take great pleasure in communicating another fact to the Society of the same character; and in order that the record may in this instance be placed beyond dispute,

I have requested my informant to substantiate his statement made to me in due legal form before a notary public."

Winslow (1873, pp. 257-258) then went on to relate: "During my visit to this mining camp I have become acquainted with Capt. David B. Akey, formerly commanding officer of a California volunteer company, and well known to many persons of note in that State, and in the course of my conversation with him I learned that in 1855 and 1856 he was engaged with other miners in running drifts into Table Mountain in Tuolumne County at the depth of about two hundred feet from its brow, in search of placer gold. He states that in a tunnel run into the mountain at the distance of about fifty feet from that upon which he was employed, and at the same level, a complete human skeleton was found and taken out by miners personally known to him, but whose names he does not now recollect. He did not see the bones in place, but he saw them after they were brought down from the tunnel to a neighboring cabin. All the bones of the skeleton

apparently were brought down in the arms of miners and placed in a box, and it was the opinion of those present that the skeleton must have been perfect as it laid in the drift. He does not know what became of the bones, but can affirm to the truth of this discovery, and that the bones were those of a human skeleton, in an excellent state of preservation. The skull was broken in on the right temple, where there was a small hole, as if a part of the skull was gone, but he cannot tell whether this fracture occurred before the excavation or was made by the miners. ... He thinks that the depth from the surface at which this skeleton was found was two hundred feet. and from one hundred and eighty to two hundred feet from the opening cut or face of the tunnel. The bones were in a moist condition. found among the gravel and very near the bed rock, and water was running out of the tunnel. There was a petrified pine tree, from sixty to eighty feet in length and between two and three feet in diameter at the butt, lying near this skeleton. Mr. Akey went into the tunnel with the miners, and they pointed out to him the place where the skeleton was found. He saw the tree in place and broke specimens from it. He cannot remember the name of this tunnel. but it was about a quarter of a mile east of the Rough and Ready tunnel and opposite Turner's Flat. another well known point. He cannot tell the sex of the skeleton, but it was of medium size. The bones were altogether, and not separated. when found."

Winslow (1873, p. 258) added: "On the same level at which this skeleton was found, but from other tunnels. Mr. Akey saw many bones of animals taken, but no other human remains. Among those remains were mastodon's teeth and bones of animals smaller than mastodons, the names of which he does not know. . . . Overlying these placer deposits and organic remains was volcanic matter consisting of lava or of 'honey-combed' material." Akey swore to the truth of these statements before a notary at Bear Gulch (Winslow 1873, p. 259).

The gravel Just above the bedrock at Tuolumne Table Mountain, where the skeleton was found, is said to be between 33 and 55 million years old (Slemmons 1966, p. 200). This must be the age of the skeleton unless it was introduced into the gravels at a later time, and we are not aware of any evidence indicating such an intrusion. The reported presence of mastodon teeth "on the same level . . . but from other tunnels" is interesting. Mastodons are generally thought to have appeared in North America during the Miocene, but if mastodon teeth were in fact found near the bedrock at Tuolumne Table Mountain, they would be considerably older — Early Oligocene or Eocene.

#### 6.2.6.3 The Hubbs Skull Fragment

In 1868, J. D. Whitney reported on the Calaveras skull to the American Association for the Advancement of Science. Soon thereafter, Dr. J. Wyman informed him that in the collection of the Museum of the Natural History Society of Boston there was a skull fragment that Wyman, one of America's leading craniologists (Holmes 1899, p. 456), said was human. The fossil was labeled as follows: "From a shaft in Table Mountain, 180 feet below the surface, in gold drift, among rolled stones and near mastodon debris. Overlying strata of basaltic compactness and hardness. Found July, 1857. Given to Rev. C. F. Winslow by Hon. Paul K. Hubbs, August. 1857." Another fragment. from the same skull, and similarly labeled, was to be found at the Museum of the Philadelphia Academy of Natural Sciences.

The proceedings of the Boston Natural History Society (Volume VI, p. 278, October 7, 1857) contain a message that Winslow sent to Boston along with the first skull fragment. Winslow stated: "I sent by a friend, who was going to Boston this morning, a precious relic of the human race of earlier times, found recently in California. 180 feet below the surface of Table Mountain. . . . My friend Colonel Hubbs, whose gold claims in the mountains seem to have given him much knowledge of this singular locality, writes that the fragment was brought up in the paydirt (the miner's name for the placer gold-drift) of the Columbia Claim. and that the various strata passed through in sinking the shaft consisted of volcanic formations exclusively" (Whitney 1880, p. 264).

Whitney, in California, then began his own investigation. He learned that Hubbs was "a well-known citizen of Vallejo, California, and a former State Superintendent of Education" (Whitney 1880, p. 264). Whitney got from Hubbs a detailed written account of the discovery, which occurred in the Valentine Shaft, south of Shaw's Flat. Whitney (1880, p. 265) stated: "The essential facts are, that the Valentine Shaft was vertical. that it was boarded up to the top, so that nothing could have fallen in from the surface during the working under ground, which was carried on in the gravel channel exclusively, after the shaft had been sunk. There can be no doubt that the specimen came from the drift in the channel under Table Mountain. as affirmed by Mr. Hubbs." The skull fragment was found in a horizontal mine shaft (or drift) leading from the main vertical shaft, at a depth of 180 feet from the surface (Whitney 1880, p. 265). Hubbs stated that he "saw the portion of skull immediately after its being taken out of the sluice into which it had been shoveled" (Whitney 1880, p. 265). Adhering to the bone was the characteristic gold-bearing gravel. Whitney (1880, p. 265) commented: "It is clear from Mr. Hubbs's statements that the fragment was raised from the stratum of pay gravel, and that it was noticed when the contents of the bucket were dumped into the head of the sluice, and either picked up by Mr. Hubbs, or by some one else, who happened to be standing by, and who handed it to him on the spot. "

Independent corroborating evidence came from Mr. Albert G. Walton, one of the owners of the Valentine claim, at which the skull fragment was discovered. Mr. Walton reported that a stone mortar 15 inches in diameter was found in the Valentine mine, in gold-bearing gravels 180 feet below the surface and also below the latite cap of Tuolumne Table Mountain (Whitney 1880, p. 265).

When Sinclair (1908, p. 115) visited Table Mountain in 1902, he found that many of the drift mines south of Shaw's Flat were connected. Thus, according to Sinclair, Whitney's statement that the Valentine shaft was securely boarded up to the top so that nothing could fall in from the surface did not rule out the possibility that objects could have found their way into the Valentine underground tunnel from some other tunnels.

But Sinclair did not prove that there were in fact such interconnections between the tunnels at the time the discoveries were made in 1857. Perhaps the interconnections between the tunnels he observed in 1902 were made after the discoveries. Furthermore, Sinclair (1908, p. 115) admitted that during his 1902 visit he was not even able to find the old Valentine shaft. This means he had no direct evidence that the Valentine mine shafts were connected to any others. Finally, even if



there were tunnels that connected with the drift tunnel running from the main Valentine shaft, this does not invalidate Hubbs's report. Whitney (1880, p. 265) observed that all the mines near the Valentine mine were "working through vertical shafts." One would have to imagine that somehow or other a fragment of skull was dropped into one of these vertical shafts and that it was transported some distance along a horizontal tunnel. It is hard to see how this could happen, because material from the horizontal tunnel, as it was being excavated, would have been taken back toward the vertical shaft.

So Sinclair was not able to confirm, by direct inspection or testimony, his claim that the horizontal drift tunnels running from the Valentine vertical shaft were in fact connected to other tunnels. His objection thus appears to be simply a weak and highly speculative attempt to discredit a discovery he opposed on theoretical grounds. The gravels in which the skull fragment was embedded lay 180 feet below the surface and beneath the latite cap of Table Mountain. The skull fragment could thus be from 9 million to 55 million years old.

Whitney (1880, p. 265), in his discussion of this find, noted: "It is clear that, had it not been for the accidental presence of Mr. Hubbs on the spot, at the time the piece of skull was found, we should never have heard anything of it. And if Mr. Hubbs had not given it to an enthusiastic observer, like Dr. Winslow, it would probably never have come to the notice of scientific men. One should bear in mind how few of the discoveries of human relics or remains which are made are likely ever to be heard of beyond a very limited area, even under the most favorable circumstances, as is well illustrated by the facts in this case."

#### 6.2.6.4 A Human Jaw from Below Table

##### Mountain

J. D. Whitney (1880, p. 264) personally examined a collection belonging to Dr. Snell, consisting of stone spoons, handles, spearheads, and a human jaw—all found in the auriferous gravels beneath the latite cap of Tuolumne Table Mountain. The jaw measured 5.5 inches across from condyle to condyle, which is within the normal human range. Whitney (1880, p. 288) remarked that all the human fossils uncovered in the gold-mining region, including this one, were of the anatomically modern type. The gravels from which the jaw came could be anywhere from 9 to 55 million years old.

#### 6.2.6.5 Human Bones from the Missouri

##### Tunnel

Whitney reported several discoveries from Placer County. In particular, he gave this account of human bones that were found in the Missouri tunnel: "The Missouri Tunnel runs from the Devil's Canon southerly into the ridge between it and the Middle Fork of the American River, a little above Yankee Jim's. This region has been described in the preceding pages as deeply covered with volcanic materials. In this tunnel, under the lava, two bones had been found . . . which were pronounced by Dr. Fagan to be human. One was said to be a leg bone; of the character of the

other nothing was remembered. The above information was obtained by Mr. Goodyear from Mr. Samuel Bowman, of whose intelligence and truthfulness the writer has received good accounts from a personal friend well acquainted with him. Dr. Fagan was at that time one of the best known physicians of the region" (Whitney 1880, p. 277).

In October 1989, our researcher (Stephen Bernath) contacted the California Division of Mines and Geology regarding the age of the deposits at the place where the Missouri tunnel was located. George Saucedo informed him that the andesitic deposits in that vicinity are probably part of a larger formation that has yielded a potassium-argon date of 8.7 million years ago. Thus the human skeletal remains found under the andesitic deposits in that location would have an age of over 8.7 million years.

#### 6.2.6.6 Dr. Boyce's Discovery

Professor Whitney (1880, p. 276) reported a discovery made in 1853 by a physician named Dr. H. H. Boyce at Clay Hill in El Dorado County, California. In 1870, Dr. Boyce wrote to Whitney (1880, p. 276), who had requested information: "While engaged in the business of mining in the spring of 1853. I purchased an interest in a claim on this hill, on condition that it prospected sufficiently well to warrant working it. The owner and myself accordingly proceeded to sink a shaft for the purpose of working it. It was while doing so that we discovered the bones to which you refer. Clay Hill is one of a series of elevations which constitute the water-shed between Placerville Creek and Big Canon, and is capped with a stratum of basaltic lava, some eight feet thick. Beneath this there are some thirty feet of sand, gravel, and clay. The country-rock is slightly capped on this, as on most of the elevations, the slope being towards the centre of the hill. Resting on the rock and extending about two feet above it, was a dense stratum of clay. It was in this clay that we came across the bones. While emptying the tub, I saw some pieces of material which on examination I discovered were pieces of bones: and, on further search, I found the scapula, clavicle, and parts of the first, second, and third ribs of the right side of a human skeleton. They were quite firmly cemented together: but on exposure to the air began to crumble. We made no further discoveries." According to Whitney (1880, p. 276), Boyce "stated there could be no mistake about the character of the bones, and that he had made a special study of human anatomy."

Sinclair (1908, p. 123) reported that he examined Clay Hill in 1902 and found "no basalt capping appeared either on the hill or anywhere in the vicinity." He did, however, note the presence of "a small area of andesitic breccia on the top of the hill" (Sinclair 1908, p. 123). Both andesite and basalt are dark greyish volcanic rocks; thus it is possible that Boyce, not a trained geologist, may have mistaken the andesite for basalt. Whitney (1880, p. 276) said that Boyce's "description of the geology of Clay Hill agrees, in the main, with that given by Mr. Goodyear, who states that the deposit on the bed-rock was from twentyfive to thirty feet thick, all but the lower five feet consisting of 'mountain gravel', a local name for the volcanic material capping the hills in that vicinity."

According to the United States Geological Survey Map made by W. Lindgren and H. Tumer in July 1893, the andesitic deposits on the top of Clay Hill are Pliocene or Miocene in age—therefore the stratum in which the human bones were found must be at least as old.

But Sinclair persistently attempted to cast whatever doubt he could on the discovery. He said he could not locate the clay stratum said to have contained the bones "owing to the heavy talus slopes" (Sinclair 1908, p. 123). He further stated: "The impression conveyed . . . is that the skeleton found by Dr. Boyce was at a depth of thirty-eight feet. in undisturbed strata under eight feet of so-called basalt. There is nothing, however, in the letter to show that this was the section passed through in sinking the Boyce shaft" (Sinclair 1908, p. 123). Because of the ambiguity about the exact location of the shaft, Sinclair thus concluded (1908, p. 123): "The skeleton may have been found in such a place and at such a depth in the clay that the possibility of recent interment would have to be considered. As the evidence is presented, we are not justified in regarding the skeleton from Clay Hill as of great antiquity."

The points raised by Sinclair are valid. and we agree that there are reasons to doubt the antiquity of the skeletal remains found at Clay Hill. Yet the presence of heavy talus slopes. with so much rock that Sinclair was not able to gain access to the stratum of clay at the base of the hill, seems to argue against, rather than for, the possibility of a recent burial into the clay from the slope of the hill. Also. if there were a recent burial, it is peculiar that so few bones were recovered.

This brings us to the end of our review of fossil human skeletal remains from the auriferous gravels of California. Despite the imperfections of the evidence, one thing is certain—human bones were found in the Tertiary gravels, dating as far back as the Eocene. How the bones got there is open to question. The reports of the discoveries are sometimes vague and inconclusive. yet they are suggestive of something other than pranks by miners or recent intrusive burials by Indians. The presence of numerous stone tools, incontestably of human manufacture. in the same formations. lends additional credibility to the finds.

In an address to the American Association for the Advancement of Science, delivered in August, 1879, O. C. Marsh, president of the Association and one of America's foremost paleontologists, said about Tertiary man: 'The proof offered on this point by Professor J. D. Whitney in his recent work (Atirif. Gravels of Sierra Nevada) is so strong. and his careful, conscientious method of investigation so well known. that his conclusions seem irresistible. . . . At present. the known facts indicate that the American beds containing human remains and works of man. are as old as the Pliocene of Europe. The existence of man in the Tertiary period seems now fairly established" (Southall 1882, p. 196).

#### 6.2.7 More European discoveries (Miocene and eocene)

More evidence for human beings in the early and middle Tertiary comes from Europe. According to de Mortillet. M. Quiquerez reported the discovery of a skeleton at Delemont in Switzerland in ferruginous clays said to be Late Eocene. About this find. de Mortillet (1883, p. 72) simply said one should be suspicious of human skeletons found with the bones in natural connection. De Mortillet

(1883. p. 72) further stated that one should be cautious about a similarly complete skeleton found by Garrigou in Miocene strata at Midi de France.

It is possible, however, that these skeletons were from individuals buried during the Eocene or Miocene periods. A burial does not necessarily have to be recent. The truly frustrating thing about finds such as these is that we are not able to get more information about them. We find only a brief mention by an author bent on discrediting them. Because such finds seemed doubtful to scientists like de Mortillet, they went undocumented and uninvestigated, and were quickly forgotten. How many such finds have been made? We may never know. In contrast, finds which conform to accepted theories are thoroughly investigated, safely enshrined in museums, and widely taught to millions around the world.

We are now nearing the end of our survey of evidence for Tertiary man uncovered by scientists in the nineteenth and early twentieth centuries. Much of this evidence is comparable to (or better than) the evidence used by paleoanthropologists in support of the standard scenario of human evolution. For example, in the case of Castenedolo, human skeletal remains were taken from undisturbed Pliocene formations by a professional geologist. By way of contrast, most of the Java man discoveries reported by von Koenigswald and others (Sections 7.3, 7.4) were made at poorly specified locations by paid native collectors, with no scientist present. Yet the Castenedolo find is rarely mentioned in standard textbooks, while the Java *Homo erectus* finds are routinely reported.

Over time, the scientific community eliminated Castenedolo and other discoveries discussed in this chapter from the realm of serious consideration. In 1924, in one of the final published discussions of this material, Hugo Obermaier offered a decidedly negative opinion about human beings in the Tertiary. "A fact of such transcendent importance would be demonstrated beyond question by the discovery of human skeletons of Tertiary age, but up to the present time none of the supposed discoveries of this nature is sufficiently well proved to withstand any serious scientific investigation. Neither the 'Eocene' skeleton of Delemont in Switzerland, nor the 'Pliocene' remains of Colle del Vento near Savona, Liguria, nor those of Matera, all in Italy, have supplied any data for the solving of this interesting problem—being therefore relegated to oblivion, even as the Indian skull of Calaveras, California. Neither has it been possible to prove that the discoveries of F. Ameghino in South America during the last fifteen years . . . are of Tertiary age as claimed" (Obermaier 1924, p. 2).

It is questionable whether the evidence mentioned by Obermaier, and additional evidence presented in this chapter (such as the Castenedolo finds), should have been "relegated to oblivion." Is it really the case that there were no valid scientific grounds for considering this evidence? It would appear that it was simply the great age of the discoveries, an age that conflicted with accepted ideas about human evolution, that was the real problem. In any case, science has quite effectively buried this disconcerting evidence. For example, we have so far been unable to find any other data on the Matera skeleton referred to by Obermaier.

### 6.3 Pre-tertiary Discoveries

We shall now consider rare cases of anatomical evidence for the presence of human beings in pre-Tertiary geological contexts. As we have seen in earlier chapters, some scientists believed apemen existed as far back as the Miocene and Eocene. A few bold thinkers even proposed that fully human beings were alive during those periods. But now we are going to proceed into times still more remote. Since most scientists had trouble with Tertiary humans, we can just imagine how difficult it would have been for them to give any serious consideration to the cases we are about to discuss. One is tempted not to mention such finds as these because they seem unbelievable. But the result of such a policy would be that we discuss evidence only for things we already believe. And unless our current beliefs represent reality in total, this would not be a wise thing to do.

#### 6.3.1 Macoupin, Illinois (Carboniferous)

In December of 1862, the following brief but intriguing report appeared in a journal called *The Geologist*. "In Macoupin county, Illinois, the bones of a man were recently found on a coal-bed capped with two feet of slate rock, ninety feet below the surface of the earth. . . . The bones, when found, were covered with a crust or coating of hard glossy matter, as black as coal itself, but when scraped away left the bones white and natural."

We wrote to the State Geological Survey Division of the Illinois Department of Energy and Natural Resources for information about the age of the coal in which the bones were found. We received the following response from C. Brian Trask of the Geological Survey, who wrote in a letter dated July 9, 1985: 'In response to your inquiry concerning age of coal, the youngest bituminous coal beds in Illinois are found in the upper Pennsylvanian system. . . . The coal mined in the 1860's in Macoupin County is probably the Herrin (No. 6) Coal, although the Colchester (No. 2) Coal occurs at this depth locally in the western part of the county. The Herrin Coal is late Desmoinesian (middle to late Westphalian D) in age." In North America, the Pennsylvanian makes up the latter half of the Carboniferous, which extends from 286 million to 360 million years ago. From the information provided by Trask, it would thus appear that the coal in which the Macoupin County skeleton was found is at least 286 million years old and might be as much as 320 million years old.

#### 6.3.2 Human Footprints from the carboniferous

Our final examples of anomalous pre-Tertiary evidence are not in the category of fossil human bones, but rather in the category of fossil humanlike footprints. Professor W. G. Burroughs, head of the department of geology at Berea College in Berea, Kentucky, reported (1938, p. 46): 'during the beginning of the Upper Carboniferous (Coal Age) Period, creatures that walked on their two hind legs and had human-like feet, left tracks on a sand beach in Rockcastle County, Kentucky. This was the period known as the Age of Amphibians when animals moved about on four legs or more rarely hopped, and their feet did not have a human appearance. But in Rockcastle, Jackson and several other counties in Kentucky, as well as in places from Pennsylvania to Missouri inclusive, creatures that had feet strangely human in appearance and that walked on two hind legs did exist. The writer has proved the existence of these creatures in Kentucky. With the cooperation of Dr. C.

W. Gilmore, Curator of Vertebrate Paleontology, Smithsonian Institution. it has been shown that similar creatures lived in Pennsylvania and Missouri."

The Upper Carboniferous (the Pennsylvanian) began about 320 million years ago (Harland et al 1982, p. 94). It is thought that the first animals capable of walking erect. the pseudosuchian thecodonts. appeared around 210 million years ago (Desmond 1976, p. 86). These lizardlike creatures, capable of running on their hind legs, would not have left any tail marks since they carried their tails aloft. But their feet did not look at all like those of human beings; rather they resembled those of birds. Scientists say the first appearance of apelike beings was not until around 37 million years ago, and it was not until around 4 million years ago that most scientists would expect to find footprints anything like those reported by Burroughs from the Carboniferous of Kentucky.

Burroughs (1938, p. 46) stated: "The footprints are sunk into the horizontal surface of an outcrop of hard, massive grey sandstone on the O. Finnell farm. There are three pairs of tracks showing left and right footprints. . . . Each footprint has five toes and a distinct arch. The toes are spread apart like those of a human being who has never worn shoes. " Kent Prevlette (1953) wrote: 'Scientists and travelers who have seen the tracks which he [Burroughs] proved to be genuine, or studied photographs of them, state that they resemble those of the most primitive people of the Andes, the aboriginal Chinese, and the South Sea islanders—all being people who have never worn shoes. "

Giving more details about the prints, Burroughs (1938, p. 46) stated: "The length of the foot from the heel to the end of the longest toe is nine and one-half inches though this length varies slightly in different tracks. The width across the ball of the foot is 4.1 inches while the width including the spread of the toes is about six inches. The foot curves back like a human foot to a human appearing heel." These humanlike tracks are thus quite distinct, unlike the more famous but indistinct Paluxy "man tracks" reported in Biblical creationist literature.

David L. Bushnell. an ethnologist with the Smithsonian Institution suggested the prints were carved by Indians (Science News Letter 1938a, p. 372). In ruling out this hypothesis, Dr. Burroughs (1938, pp. 46^7) used a microscope to study the prints and noted: 'The sand grains within the tracks are closer together than the sand grains of the rock Just outside the tracks due to the pressure of the creatures' feet' Even the sand grains in the arch of one of the best preserved tracks are not as close together as in the heel of the same track, though closer together than the sand outside the track. This is because there was more pressure upon the heel than beneath the arch of the foot. In comparing the texture of sandstone only the same kind of grains and combinations of grains within and outside of the tracks are considered. The sandstone adjacent to many of the tracks is uprolled due to the damp. loose sand having been pushed up around the foot as the foot sank into the sand. The forward part of one track is covered by solid Pottsville sandstone only a few days or weeks younger than the sandstone in which is the track. Another track nearby is also partially covered by solid Pottsville sandstone of the Coal Age." These facts led Burroughs to

conclude that the humanlike footprints were formed by compression in the soft, wet sand before it consolidated into rock some 300 million years ago.

Two doctors from the town of Berea, Alson Baker and A. F. Cornelius, also counted the sand grains per unit area under magnification and arrived at the same result as had Dr. Burroughs. They reported: "We examined the arrangement of the sand grains in the deepest portions of the prints, with special attention to the heels. The sand grains in the bottoms of the prints were much more closely packed than those in the slopes, and those in the slopes were more closely packed than those in the rock an inch from the margins of the prints, or at any other point. Each member of the party certified and checked these findings and we all agree that the imprints were made by pressure when the sand was soft and wet. The fact that the sand grains in the bottoms and slopes of the imprints are of exactly the same kind as those in all other parts of the rock surface examined, seems to prove conclusively that the closer arrangement observed was not due to any possible drifting in of extraneous material" (ScienceNews Letter 1938a, p. 372).

Burroughs also consulted a sculptor. Kent Previette (1953) wrote: "The sculptor said that carving in that kind of sandstone could not have been done without leaving artificial marks. Enlarged photomicrographs and enlarged infrared photographs failed to reveal any 'indications of carving or cutting of any kind. '"

If the prints were not carvings, were they left by a nonhuman Carboniferous species? The most advanced land animals then existing were amphibians that resembled crocodiles and moved about on four legs. But Burroughs (1938, p. 47) wrote: "There are no indications of front feet although the rock is large enough to have recorded front feet if front feet had been used to move about. In the pair of footprints that show the left and right feet about parallel to each other, the distance between the feet is about the same as that of a normal human being. Nowhere on this rock nor on another rock outcrop that also has numerous similar tracks upon its surface, is there any sign that these creatures had tails." Nor were there any belly marks (Previette 1953).

Burroughs (1938, p. 47) added: "The creatures that made the tracks have not as yet been identified, but a name for these creatures has been chosen by the writer with the co-operation of Dr. Frank Thone, Editor in Biology, Science Service, Washington, D.C., Dr. C. W. Gilmore, Curator of Vertebrate Paleontology, Smithsonian Institution, and Miss Charlotte Ludlum, Professor of Latin, Berea College. The name chosen is *Phenanthropus mirabilis*." The word *phenanthropus* means "looks human," and *mirabilis* means "remarkable."

Burroughs himself stopped short of claiming that the prints were made by humans, but his presentation leaves one with the strong impression that they were human. When asked about them, Burroughs said, "They look human. That is what makes them especially interesting, as man according to some textbooks has been here only a million and a half years" (Previette 1953). But mainstream science reacted predictably to any suggestion, that the prints were made by humans. Science News Letter (1938b) published an article titled "Human-Like Tracks in Stone Are Riddle to Scientists." A subtitle stated: "They Can't Be Human Because They Are Much Too Old — But What Strange Biped Amphibian Can Have Made Them?" Despite the doubts of scientists, the Burroughs

footprints continued to attract public attention, which might explain why geologist Albert G. Ingalls felt compelled to set matters straight in Scientific American.

Ingalls (1940, p. 14) stated that a scientist, confronted with the suggestion that the tracks were human, would have little choice but to reply: "What? You want man in the Carboniferous? Entirely and absolutely— totally and completely— impossible. We admit we don't know exactly what made the prints, but we do know one agency that didn't, and that is man in the Carboniferous."

But what about scientific detachment—the willingness to give up established ideas or tentative hypotheses when confronted with contrary evidence? Ingalls (1940, p. 14) wrote: "Science is like the streets of New York: it is never finished, and is always being torn up, often in a major way.. .. Nevertheless, asking the scientist for man in the Carboniferous is like asking the historian for Diesel engines in ancient Sumeria. The comparison is no exaggeration but an understatement. If man, or even his ape ancestor, or even that ape ancestor's early mammalian ancestor, existed as far back as in the Carboniferous Period in any shape, then the whole science of geology is so completely wrong that all the geologists will resign their jobs and take up truck driving. Hence, for the present at least, science rejects the attractive explanation that man made these mysterious prints in the mud of the Carboniferous with his feet. "

Ingalls thought the prints were made by some as yet unknown kind of amphibian. "Science has no proof that these tracks were not made by one or more of these animals — one with which it is not yet familiar—for it does not know everything. Professor W. G. Burroughs, Berea College, Kentucky, geologist, champions this theory, supported by the paleontologist Charles W. Gilmore at the United States Museum" (Ingalls 1940, p. 14). Here Ingalls appears to have put his own interpretation on Burroughs's ambiguous testimony, bringing the wayward researcher firmly back within the bounds of scientific sanity.

We should note that scientists do not really take the amphibian theory seriously. Human-sized Carboniferous bipedal amphibians do not fit into the accepted scheme of evolution much better than Carboniferous human beings— they wreak havoc with our Ideas of early amphibians, requiring a host of evolutionary developments we now know nothing about.

Ingalls (1940, p. 14) wrote: ' What science does know is that, anyway, unless 2 and 2 are 7, and unless the Sumerians had airplanes and radios and listened to Amos and Andy, these prints were not made by any Carboniferous Period man. "

### 6.3.3 A Central Asian Footprint (Jurassic)

The Moscow News (1983, no. 24, p. 10) gave a briefbut intriguing report on

what appeared to be a human footprint In 150-million-year-old Jurassic rock next to a giant three-toed dinosaur footprint. The discovery occurred in the Turkmen Republic in what was then the southeastern USSR. Professor Amanniyazov, corresponding member of the Turkmen SSR Academy of Sciences, said that although the print resembled a human footprint, there was no conclusive proof that it was made by a human being. This discovery has not received much attention, but



then, given the current mindset of the scientific community, such neglect is to be expected. We only know of a few cases of such extremely anomalous discoveries, but considering that many such discoveries probably go unreported we wonder how many there actually might be.

#### 6.4 Conclusion

The evidence reviewed in Chapters 2-6 suggests the existence of anatomically modern humans as far back as the early Tertiary. None of this evidence tends to be reported in modern textbooks on anthropology. Should it be reported? We leave it to the reader to decide. If taken seriously, this evidence would certainly challenge the currently dominant understanding of human origins and antiquity, but perhaps this topic is not as thoroughly understood as some believe. The cultural evidence we have considered, in the form of stone tools and incised bones, suggests a relatively primitive level of advancement. There is, however, evidence that suggests a higher level of cultural achievement. But unlike the evidence considered in Chapters 2-6, much of this evidence was never reported by scientists. For a review of this controversial evidence see Appendix 2.

## Part II

### Accepted evidence

#### Java Man

In the preceding chapters, we have reviewed three categories of anomalous evidence relating to human origins and antiquity—human skeletal remains, incised bones, and stone implements of various kinds. At the end of the nineteenth century, on the basis of such evidence, a consensus was building within an influential portion of the scientific community that human beings of the modern type had existed as far back as the Pliocene and Miocene periods—and perhaps even earlier.

Anthropologist Frank Spencer (1984, pp. 13-14) stated: "From accumulating skeletal evidence it appeared as if the modern human skeleton extended far back in time, an apparent fact which led many workers to either abandon or modify their views on human evolution. One such apostate was Alfred Russell Wallace (1823-1913). In 1887, Wallace examined the evidence for early man in the new World, and . . . found not only considerable evidence of antiquity for the available

specimens, but also a continuity of type through time. In an effort to explain this, Wallace suggested that . . . man, through culture, had been essentially partitioned from the vagaries of natural selection and was, thereby, a unique creation of the biotic realm."

to Darwin, this was heresy of the worst sort. But Spencer (1984, p. 14) noted that Wallace's challenge to evolutionary doctrine "lost some of its potency as well as a few of its supporters when news began circulating of the discovery of a remarkable hominid fossil in Java." considering the striking way in which the Java man fossils were employed in discrediting and suppressing evidence for the great antiquity of the modern human form, we shall now review their history.

We will discuss the initial discoveries made by Eugene Dubois in the 1890s, the discoveries made by G. H. R. von Koenigswald in the 1930s and 1940s, and the discoveries made by other researchers since 1950. We will then discuss the chemical and radiometric dating of these discoveries, and conclude with a critique of standard scientific presentations of the Java *Homo erectus* evidence. In this chapter, we shall also discuss the Heidelberg Jaw, discovered not long after the original Java man finds and also classified as *Homo erectus*.

In succeeding chapters, we will examine other paleoanthropological evidence currently employed by scientists to support their hypothesis that the modern human form evolved within the past 100,000 years from more primitive hominid ancestors. We will focus on discoveries made in China (chapter 9) and Africa (chapter 11). In addition to this conventionally accepted evidence, we will also examine the controversial Piltdown case (chapter 8) and evidence for living ape-men (chapter 10).

## 7.1 Dubois and *Pithecanthropus Erectus*

The city of Bandung lies in the high cool uplands of western Java. From there a road leads eastward, down to the steaming plain of Leles, continuing on to the district town of Madiun. The green, forested peaks of the volcanoes Mt. Lawu and Mt. Willis rise against the brilliantly blue tropical sky. Pushing onward one arrives at the kampong, or village, of Trilni, surrounded by fields of rice and sugar cane, as well as groves of coconut trees. Past the village, the road ends on a high bank overlooking the Solo River. Here one encounters a small stone monument, marked with an arrow pointing toward a sand pit on the opposite bank. The monument also carries a cryptic German inscription, "p.e. 175 m OnO 1891/93," indicating that *Pithecanthropus erectus* was found 175 meters east northeast from this spot, during the years 1891-1893.

The discoverer of *Pithecanthropus erectus* was Eugene Dubois, born in Eindhoven, Holland, in 1858, the year before Darwin published *The Origin of Species*. As a boy, Dubois explored the nearby limestone quarries, filling his pockets with fossils. Although the son of devout Dutch Catholics, the idea of evolution, especially as it applied to the question of human origins, fascinated him. His imagination was quickened by this passage in A. R. Wallace's *Malay Archipelago* (1869): "With what interest must every naturalist look forward to the time when the caves and tertiary deposits of the tropics may be thoroughly examined and the past history and earliest appearance of the great man-like apes be at length made known."

After studying medicine and natural history at the University of Amsterdam, Dubois became a lecturer in anatomy at the Royal nonnal School in 1886. But his real love remained evolution. Dubois knew that Danvin's opponents were constantly pointing out the almost complete lack of fossil evidence for human evolution. He carefully studied the principal evidence then available—the bones of Neanderthal specimens. these were regarded by most authorities (among them Thomas Huxley) as too close to the modern human type to be considered truly intermediate between fossil apes and modern humans. the German scientist Ernst Haeckel had, however, predicted that the bones of a real missing link would eventually be found. Haeckel even commissioned a painting of the creature, whom he called Pithecanthropus (In Greek, pitheko means "ape," and anthropms means "man" ). influenced by Haeckel's vision of Pithecanthropus, Dubois resolved to someday find the ape- man's bones,

Mindful of Darwin's suggestion that humanity's forbearers lived In “some warm, forest-clad land," Dubois became convinced Pithecanthropus would be found in Afiitca or the east indies, Because he could more easily reach the east indies, then under Dutch rule, he decided to Journey there and begin his quest. He applied first to private philanthropists and the government, requesting financing for a scientific expedition, but was turned down, He then accepted an appointment as an army surgeon In Sumatra, With his friends doubting his sanity, he gave up his comfortable post as a college lecturer and with his young wife set sail for the east Indies In December 1887 on the S S Princess Amalie.

#### 7.1.1 Initial Discoveries

In 1888, Dubois found himself stationed at a small military hospital In the interior of Sumatra. His exact movements during this period remain somewhat unclear, but from a variety of accounts the following general sequence emerges. The year he arrived in Sumatra, Dubois published a scientific paper titled “On the need for an investigation of an ice Age Fauna in the Dutch east Indies, and especially in Sumatra." He was, of course, primarily interested in finding the remains of human ancestors. Dubois wrote: “Since all apes—and notably the anthropoid apes—are inhabitants of the tropics, and since man's forerunners, as they have gradually lost their coat of hair, must certainly have continued to live in warm regions, we are inescapably led toward the tropics as the area in which we may expect to find the fossilized precursors of man" (von Koenigswald

1956, p. 28).

Dubois's writings attracted the attention of officials of the colonial Mining Authority. In its first quarterly report for the year 1889, the Mining Authority informed Its readers that Dubois had been authorized to undertake paleontological research in Sumatra. in his spare time, and using his own funds, Dubois investigated Sumatran caves, finding fossils of rhino and elephant, and the teeth of an orangutan, but no hominid remains.

In 1890, after suffering an attack of malaria, Dubois was placed on inactive duty and transferred from Sumatra to Java, where the climate was somewhat drier and healthier. He and his wife set up housekeeping in tulungagung, on eastern Java's southern coast. the Mining Authority gave him

permission to carry out his paleontological explorations in Java, supplying him with two sergeants from the corps of military engineers and a crew of fifty convict laborers. At the nearby marble quarry at Wadjak, Dubois turned up two fossil human skulls, both modern in type (related to the Australian aborigines) and therefore not worthy of consideration as ancestral ape-men. Interestingly enough, Dubois did not report these skulls to the scientific world until 1922.

In November 1890, at Kedungbrubus, Dubois made another find—a fossil jaw with part of a tooth root embedded in it. In a preliminary report, he judged it to be human (von Koenigswald 1956, p. 31). This specimen was not fully described until 1924, at which time Dubois designated it *Pithecanthropus*.

#### 7.1.2 The Discoveries at Trinil

During the dry season of 1891, Dubois conducted excavations on the bank of the Solo River in central Java, near the village of Trinil. His laborers took out many fossil animal bones. In September, they turned up a particularly interesting item—a primate tooth, apparently a third upper right molar, or wisdom tooth. Dubois, believing he had come upon the remains of an extinct giant chimpanzee, ordered his laborers to concentrate their work around the place where the tooth had turned up. In October, they found what appeared to be a turtle shell. But when Dubois inspected it, he saw it was actually the top part of a cranium (Figure 7.1), heavily fossilized and having the same color as the volcanic soil. The fragment's most distinctive feature was the large, protruding ridge over the eye sockets, leading Dubois to suspect the cranium had belonged to an ape. The onset of the rainy season then brought an end to the year's digging. In a report published in the government mining bulletin, Dubois said about the tooth and skullcap, "that both specimens come from a great manlike ape was at once clear" (Time-Life 1973, p. 40). There was no suggestion that the fossils belonged to a creature transitional to humans. The term "manlike" (or "anthropoid") is widely used with reference to modern apes such as chimpanzees, gorillas, and orangutans, although these are not considered ancestral to human beings.

Figure 7.1. *Pithecanthropus* skullcap discovered by Eugene Dubois in 1891 in Java (Wendt 1972, p. 167).

In August 1892, Dubois returned to Trinil and found there—among bones of deer, rhinoceroses, hyenas, crocodiles, pigs, tigers, and extinct elephants—a fossilized humanlike femur (thighbone). This femur (Figure 7.2) was found about 45 feet from where the skullcap and molar were dug up. Later another molar was found about 10 feet from the skullcap. Dubois believed the molars, skull,

and femur all came from the same animal, which he still considered to be an extinct giant chimpanzee (von Koenigswald 1956, p. 31).

Figure 7.2. thighbone found by Eugene Dubois at Trinil, Java (Boule 1923, p. 100). Dubois attributed it to

*Pithecanthropus erectus*.

The British researcher Richard Carrington (1963, p. 84) stated in his book *A Million Years of Man*: "Dubois was at first inclined to regard his skull cap and teeth as belonging to a chimpanzee, in spite of the fact that there is no known evidence that this ape or any of its ancestors ever lived in Asia. But on reflection, and after corresponding with the great Ernst Haeckel, professor of Zoology at the University of Jena, he declared them to belong to a creature which seemed admirably suited to the role of the 'missing link.'" We have not found any correspondence Dubois may have exchanged with Haeckel, but if further research were to turn it up, it would add considerably to our knowledge of the circumstances surrounding the birth of *Pithecanthropus erectus*. Obviously, both men had a substantial emotional and intellectual stake in finding an ape-man specimen. Haeckel, on hearing from Dubois of his discovery, telegraphed this message: "From the inventor of *Pithecanthropus* to his happy discoverer!" (Wendt 1972, p. 167).

It was only in 1894 that Dubois finally published a complete report of his discovery, titled "*Pithecanthropus erectus*, a Man-like Species of transitional Anthropoid from Java." therein he wrote: "*Pithecanthropus* is the transitional form which, in accordance with the doctrine of evolution, must have existed between man and the anthropoids" (von Koenigswald 1956, p. 31). *Pithecanthropus erectus*, we should carefully note, had itself undergone an evolutionary transition within the mind of Dubois, from fossil chimpanzee to transitional anthropoid.

What factors, other than Haeckel's influence, led Dubois to consider his specimen transitional between fossil apes and modern humans? Dubois found that the volume of the *Pithecanthropus* skull was in the range of 800-1000 cubic centimeters. Modern apes average 500 cubic centimeters, while modern human skulls average 1400 cubic centimeters, thus placing the Trinil skull midway between them. To Dubois, this indicated an evolutionary relationship. But logically speaking, one could have creatures with different sizes of brains without having to posit an evolutionary progression from small to large. Furthermore, in the Pleistocene many mammalian species were represented by forms much larger than today's. Thus the *Pithecanthropus* skull might belong not to a transitional anthropoid but to an exceptionally large Middle Pleistocene gibbon, with a skull bigger than that of modern gibbons.

today, anthropologists still routinely describe an evolutionary progression of hominid skulls, increasing in size with the passage of time—from Early Pleistocene *Australopithecus* (first discovered in 1924), to Middle Pleistocene Java man (now known as *Homo erectus*), to Late Pleistocene *Homo sapiens sapiens*. But the sequence is preserved only at the cost of eliminating skulls that disrupt it. For example, the Castenedolo skull, discussed in chapter 6, is older than that of Java man but is larger in cranial capacity. In fact, it is fully human in size and morphology. Even one such exception is sufficient to invalidate the whole proposed evolutionary sequence.

Dubois observed that although the Trinil skull was very apelike in some of its features, such as the prominent brow ridges, the thighbone was almost human. This indicated that *Pithecanthropus* had walked upright, hence the species designation *erectus*. It is important, however, to keep in mind that the femur of *Pithecanthropus erectus* was found fully 45 feet from the place where the skull was unearthed, in a stratum containing hundreds of other animal bones. This circumstance makes doubtful the claim that both the thighbone and the skull actually belonged to the same creature or even the same species.

### 7.1.3 Reports Reach Europe

When Dubois's reports began reaching Europe, they received much attention. In *Meeting Prehistoric Man*, von Koenigswald (1956, p. 26) commented on Java man's significance: "Dubois's find came at just the right moment: at a time when the conflict around Darwinism was at its height. For the scientific world it constituted the first concrete proof that man is subject not only to biological but also to paleontological laws." The discoverer of Lucy, Donald C. Johanson, in describing the expectant mood of scientists in the late nineteenth century, wrote: "if the theory of evolution had any validity whatsoever, then human fossils would have to reveal an increasing retreat toward primitiveness as one tracked them deeper into time" (Johanson and Edey 1981, p. 30). *Pithecanthropus erectus* appeared to amply satisfy this requirement, and even today, it is advertised (under the name *Homo erectus*) as a critical piece of evidence confirming the theory of evolution.

Haeckel, of course, was among those celebrating *Pithecanthropus* as the strongest proof to date of human evolution. "Now the state of affairs in this great battle for truth has been radically altered by Eugene Dubois's discovery of the fossil *Pithecanthropus erectus*," proclaimed the triumphant Haeckel. "He has actually provided us with the bones of the ape-man I had postulated. This find is more important to anthropology than the much-lauded discovery of the X-ray was to physics" (Wendt 1972, p. 167). Haeckel would also state that Java man "was truly a Pliocene remainder of that famous group of the higher catarrhines [Old World apes], which were the pithecoïd ancestors of man. He is indeed the long-sought-for Missing Link" (Bowden 1977, p. 128). There is an almost religious tone of prophecy and fulfillment in Haeckel's remarks. But Haeckel had a history of overstating physiological evidence to support the doctrine of evolution; an academic court at the University of Jena once found him guilty of falsifying drawings of embryos of various animals in order to demonstrate his particular view of the origin of species (Section 1.3).

### 7.1.4 Dubois Journeys to Europe with Java Man

In 1895, Dubois decided to return to Europe to display his Pithecanthropus to what he was certain would be an admiring and supportive audience of scientists. Taking 215 cases of other fossils, he boarded a ship along with his family. During a storm at sea, Dubois was especially concerned about his prized Pithecanthropus erectus specimens. Standing with the box containing Pithecanthropus, Dubois said to his wife, "if something happens, you're to take care of the children. I've got to look after this" (Time-Life 1973, p. 44).

Soon after arriving in Europe, Dubois exhibited his specimens and presented reports at the third international congress of Zoology at Leyden, Holland. Although some of the scientists present at the congress were, like Haeckel, anxious to support the discovery as a fossil ape-man, others thought it merely an ape, while still others challenged the idea that the bones belonged to the same individual.

Dubois exhibited his treasured bones at Paris, London, and Berlin. In December of 1895, experts from around the world gathered at the Berlin Society for Anthropology, ethnology, and prehistory to pass judgement on Dubois's Pithecanthropus specimens. The president of the Society, Dr. Virchow, refused to chair the meeting. In the controversy-ridden discussion that followed, the Swiss anatomist Kollman said the creature was an ape. Virchow himself said that the femur was human, and further stated: "the skull has a deep suture between the low vault and the upper edge of the orbits. Such a suture is found only in apes, not in man. Thus the skull must belong to an ape. In my opinion this creature was an animal, a giant gibbon, in fact. The thigh-bone has not the slightest connection with the skull" (Wendt 1972, pp. 167-168). This opinion contrasted strikingly with that of Haeckel and others, who remained convinced that Dubois's Java man was a genuine human ancestor.

As Dubois traveled from city to city, carrying his Pithecanthropus fossils with him, controversy continued. Some were suspicious because the discoveries had been made in Java without any opportunity for confirmation by other scientists on the scene. Nevertheless, Dubois repeatedly defended his ape-man interpretation of the fossils. For example, when Sir Arthur Keith of Britain maintained that Pithecanthropus erectus was actually just a somewhat primitive human, Dubois personally brought his fossils for Keith to examine, but even after seeing them Keith maintained his dissent (Goodman 1982, p. 60).

Dubois carried his bones around with him on his paleontological pilgrimage in a battered suitcase. Once, in Paris, he went to show the bones to Leone Pierre Manouvrier, a noted French anthropologist. In Manouvrier's laboratory, the two talked till midnight and then went to a restaurant. Upon leaving, Dubois realized that he had forgotten his suitcase. Rushing back to the restaurant, Dubois asked desperately, "Where is Pithecanthropus?" It turned out that a waiter had the suitcase. Dubois hurriedly opened it, assuring himself that the fossils were still inside. Manouvrier suggested Dubois sleep that night with the bones under his pillow (Time-Life 1973, p. 45).

dubois and Manouvrier attempted a reconstruction of the whole Java man skull, including the facial region, for which no bones were actually discovered. It is apparent that the entire jaw and facial structure were simply imagined (Boule

1923, p. 105). Some authorities thought dubois and Manouvrier had reconstructed the skull improperly, making the cranial curve appear too low. A reconstruction that featured a higher skull profile made *Pithecanthropus* appear much more human (von Koenigswald 1956). Another imaginative reconstruction of the Java man skull was attempted by J. H. W. McGregor (Osborn 1916, p. 79). About a full-scale statue of Java man, Boule (1923, p. 105) stated: "dubois ventured still further in the realm of imagination when he exhibited at the International exhibition of 1900, in the Dutch Indies pavilion, a painted model of *Pithecanthropus* as he appeared in life."

In light of the incompleteness of the Java man skeletal remains and the doubtful circumstances of their discovery, it is amazing numerous scientists accepted the hypothetical *Pithecanthropus erectus* and wrote so many books about him.

#### 7.1.5 The Selenka Expedition

To resolve some of the questions surrounding the *Pithecanthropus* fossils and their discovery, Emil Selenka, professor of zoology at Munich University in Germany, prepared a full-fledged expedition to Java, but he died before it departed. His wife, professor Lenore Selenka, took over the effort and conducted excavations at Trinil in the years 1907-1908, employing 75 laborers to hunt for more *Pithecanthropus erectus* fossils. Altogether, Selenka's team of geologists and paleontologists sent back to Europe 43 boxes of fossils, but they included not a single new fragment of *Pithecanthropus*. Sir Arthur Keith (1911) reviewed the results of the Selenka expedition in the *Journal of Nature* and reported that the geological stratification at Trinil was unclear. Three of the geologists with the expedition thought the deposits were Pleistocene, perhaps recent, but two other experts agreed with dubois that they might be Pliocene. As far as the bones themselves were concerned, their age was uncertain. Some of the geologists believed volcanic activity could have caused their fossilization, which therefore was not a sure sign of great age. The bones might have been recent, and might have been mixed in with older fossils by floods. The report mentioned a flood that occurred in Java in 1909, sending mud slides down the volcanic mountainsides, killing 500 persons and sweeping away entire villages. Perhaps even more troubling was the discovery in the Trinil strata of signs of a human presence—splintered animal bones, charcoal, and foundations of hearths. Signs like this led Lenore Selenka to conclude that humans and *Pithecanthropus erectus* were contemporary (Bowden 1977, pp. 134-135). The implications of all this for an evolutionary interpretation of dubois's *Pithecanthropus* specimens were, and still are, unsettling.

Furthermore George Grant Maccurdy, a Yale professor of anthropology, wrote in his book *Human Origins* (1924a, p. 316): "the Selenka expedition of 1907-1908 . . . secured a tooth which is said by Walkoff to be definitely human. It is a third molar from a neighboring stream bed and from deposits older (Pliocene) than those in which *Pithecanthropus erectus* was found. Should this tooth prove to be human, *Pithecanthropus* could no longer be regarded as a precursor of man.



Instead it would simply give us the cross section of a different limb of the primate tree whose branches now represent the various types of Hominidae." the beds referred to by Maccurdy as being older than the *Pithecanthropus erectus* deposits might be the dJetis Beds of the putjangan formation, now placed in the early pleistocene or in the early Middle pleistocene (Section 7.5.1).

In the aftermath of the Selenka expedition, tourists began coming to Java to look at the place where Java man had been discovered. they found the site littered with hundreds of beer bottles left by the thirsty scientists. As might be expected, many of the pilgrims were hoping they might stumble upon a *Pithecanthropus* bone. The local residents, who would find all kinds of bones washed out of the ground after floods, obliged them by selling them assorted pieces of skeletons. On december 27, 1926, a newspaper in Batavia announced that dr. c. e. J. Heberlein had found at trinil a new skull of *Pithecanthropus*. But it turned out to be a large ball-like Joint from the leg bone of a fossil elephant.

#### 7.1.6 Dubois Withdraws from the Battle

Meanwhile, the status of dubois's ape-man remained somewhat controversial. Surveying the range of opinion about *Pithecanthropus*, Berlin zoologist Wilhelm dames gathered statements from 25 scientists: three said *Pithecanthropus* was an ape, five said it was human, six said it was an ape-man, six said it was a missing link, and two said it was a link between the missing link and man. virchow had said: "All i can do is warn against drawing decisive conclusions from these few pieces of bone about the greatest question facing us in the study of our creation. *Pithecanthropus* ^11 remain doubtful as a transitional form until someone can demonstrate how this transition, which to me is conceivable only in my dreams, actually came true" (Wendt 1972, p. 169).

But although virchow and others maintained their doubts, many scientists followed Haeckel in hailing Java man as stunning proof of darwin's theory. Some used Java man to discredit evidence for a fully human presence in the tertiary. As we learned in Section 5.5.13, W. H. Holmes (1899, p. 470) dismissed discoveries of stone tools in the tertiary auriferous gravels of california because they "implied a human race older by at least one-half than *Pithecanthropus erectus* of dubois, which may be regarded as an incipient form of human creature only."

At a certain point, dubois became completely disappointed with the mixed reception the scientific community gave to his *Pithecanthropus*. He stopped showing his specimens. Some say that he kept them for some time beneath the floorboards in his home. in any case, they remained hidden from view for some 25 years, until 1932.

During and after the period of withdrawal, the controversies concerning *Pithecanthropus* continued. Marcellin Boule, director of the institute of Human paleontology in paris, reported (1923, p. 96), as had other scientists, that the layer in which the *Pithecanthropus* skullcap and femur were said to have been found contained numerous fossil bones of fish, reptiles, and mammals. Why, therefore, should anyone believe the skullcap and femur came from the same individual or even the same species? Boule, like virchow (Section 7.1.4), stated that the femur was identical to that of a modern human whereas the skullcap resembled that of an ape, possibly a

large gibbon. dr. F. Weidenreich, honorary director of the Cenozoic Research Laboratory at Peiping Union Medical College, also stated (1941, p. 70) that there was no Justification for attributing the femur and the skullcap to the same individual. the femur, Weidenreich said, was very similar to that of a modern human, and its original position in the strata was not securely established. Modern researchers have employed chemical dating techniques in order to detennine whether or not the original Pithecanthropus skull and femur were both contemporary with the Middle pleistocene trinil fauna, but the results were inconclusive (Section 7.5.2).

#### 7.1.7 More Femurs

The belated revelation that more femurs had been discovered in Java further complicated the issue. in 1932, dr. Bernsen and eugene dubois recovered three femurs from a box of fossil mammalian bones in the Lelden Museum in the netherlands. the box contained specimens said to have been excavated in 1900 by dubois's assistant, Mr. Kriele, from the same trinil deposits on the left bank of the Solo river that had yielded Dubois' s first Java man finds. Dr. Bernsen died very shortly thereafter, without providing further information about the details of this museum discovery.

dubois (1932, p. 719) stated that he was not present when the femurs were taken out by Kriele. therefore the exact location of the femurs in the excavation, which was 75 meters (246 feet) long by 6-14 meters (20-46 feet) wide, was unknown to him. According to standard paleontological procedures, this uncertainty greatly reduces the value of the bones as evidence of any sort. nevertheless, as we shall see, authorities later assigned these femurs to a particular stratum without mentioning the dubious circumstances of their discovery in boxes of fossils over 30 years after they were originally excavated (Section 7.6). Moreover, G. H. R. von Koenigswald (1956, p. 36) reported that Dubois's collection "comprised finds from various sites and various ages, which are very inadequately distinguished, because some of the labels got lost."

eugene dubois (1934, p. 139) reported that in december of 1932 he discovered a fragment of a fourth new femur in the same collection in which the others had been found. Once again, dubois pointed out that the original place of excavation was unknown. in August 1935, a museum employee named van der Steen handed dubois yet another femur fragment from the collection, but dubois said that this bone was "certainly not from trinil but from another part of the Kendeng region." dubois (1935, p. 850) speculated that it may have been found at Kedungbrubus, but he admitted that he was not really sure.

the existence of these additional femurs has important implications for the original Pithecanthropus skull and femur found by dubois in the 1890s. As we have seen, the fact that the apelike skull and humanlike femur were found at a great distance from each other is sufficient to suggest that they belonged not to one ape-man creature but to two different creatures, one apelike and the other fully human. in response, one might argue, as dubois's supporters did, that the odds of an apelike creature leaving a skull and no leg bones and a man leaving a leg bone and no skull so close to each other were remote. if it were not possible to prove there were two creatures, it would be best to assign the bones to one creature. dubois suggested that the bones

were found separated because the Pithecanthropus had been dismembered by a crocodile (Bowden 1977, p. 127). But if you throw in more humanlike femurs, that argument loses a great deal of its force. Where were the other skulls?

Were they apelike skulls, like the one found? And what about the skull that was found? does it really go with the femur that was found 45 feet away? Or does it belong with one of the other femurs that later turned up? Or maybe with a femur of an entirely different sort?

#### 7.1.8 Are the Trinil Femurs Human?

M. H. day and t. i. Molleson (1973, p. 151) concluded that "the gross anatomy, radiological [X-ray] anatomy, and microscopical anatomy of the trinil femora does not distinguish them significantly from modern human femora." They also said that Homo erectus femurs from china (Zhoukoudlan) and Africa (Olduvai Hominid 28) "are anatomically similar, and distinct from those of trinil" (day and Molleson 1973, p. 152).

In 1984, Richard Leakey and three American scientists discovered an almost complete skeleton of Homo erectus in Kenya. Examining the leg bones, these scientists found that the femurs differed substantially from those of modern human beings: "the biomechanical neck length of 85 mm is well over 3 standard deviations from the mean of a sample of H. sapiens. As well as having a long femoral neck, the neck-shaft angle is very small at 110 degrees, being 5 standard deviations from the mean of the same H. sapiens population" (Brown et al. 1985, p. 791). About the Java discoveries, the authors stated: "From trinil, indonesia, there are several fragmentary and one complete (but pathological) femora. despite the fact that it was these specimens that led to the species name [Pithecanthropus erectus], there are doubts as to whether they are H. erectus with the most recent consensus being that they probably are not" (Brown et al. 1985, p. 789).

in summary, Brown et al. (1985) and day and Molleson (1973) agreed that the trinil femurs were not like those of Homo erectus, while day and Molleson (1973) said the trinil femurs were like those of modern Homo sapiens.

What is to be made of these revelations? the Java thighbones have traditionally been taken as evidence of an ape-man (Pithecanthropus erectus, now called Homo erectus) existing around 800,000 years ago in the Middle pleistocene. Should we now accept them as evidence for anatomically modern humans existing 800,000 years ago? perhaps wisely, Brown and his associates offered in their report no suggestions about the real age of the human femurs found at trinil. there is safety in silence when confronting disconcerting paleontological anomalies.

Some have said that the femurs were mixed in from higher levels. Of course, if one insists that the humanlike trinil femurs were mixed in from higher levels, then why not the Pithecanthropus skull as well? that would eliminate entirely the original Java man find, long advertised as solid proof of human evolution.

#### 7.1.9 Dubois Backs Away from His Original Claims

Late in his life, Dubois concluded that the skullcap of his beloved Pithecanthropus belonged to a large gibbon, an ape not thought by evolutionists to be closely related to humans (Gowlett 1984, p. 17). But the heretofore skeptical scientific community was not about to say good-bye to Java man, for by this time Pithecanthropus was firmly entrenched in the ancestry of modern Homo sapiens. Dubois' s denials were dismissed as the whims of a cantankerous old man. If anything, the scientific community wanted to remove any remaining doubts about the nature and authenticity of Java man. this, it was hoped, would fortify the whole concept of darwinian evolution, of which human evolution was the most highly publicized and controversial aspect.

Despite the doubts about the Trinil find expressed by Dubois himself in his later years, and by other scientists from the 1890s to the present, public presentations remain unchanged. visitors to museums around the world still find models of the Trinil skullcap and femur portrayed as belonging to the same Middle pleistocene Homo erectus Individual. In 1984, the much-advertised Ancestors exhibit, at the Museum of natural History in New York brought together from around the world the major fossil evidence for human evolution, including prominently displayed casts of the trinil skullcap and femur.

## 7.2 The Heidelberg Jaw

In addition to dubois's Java man discoveries, further evidence relating to human evolution turned up in the form of the Heidelberg Jaw. On October 21, 1907, daniel Hartmann, a workman at a sand pit at Mauer, near Heidelberg, Germany, discovered a large Jawbone at the bottom of the excavation, at a depth of 82 feet. the workmen were on the lookout for bones, and many other nonhuman fossils had already been found there and turned over to the geology department at the nearby University of Heidelberg. the workman then brought the Jaw (Figure 7.3) over to J. Rosch, the owner of the pit, who sent a message to dr. Otto Schoetensack: "For twenty long years you have sought some trace of early man in my pit ... yesterday we found it. A lower Jaw belonging to early man has been found on the floor of the pit, in a very good state of preservation" (Wendt 1972, p. 161).

Figure 7.3. The Heidelberg mandible, discovered in 1907 at Mauer, near Heidelberg, Germany (Osborn 1916, p. 98).

Professor Schoetensack designated the creature Homo heidelbergensis. dating it using the accompanying fossils to the Günz-Mindel Interglacial period. david pilbeam (1972, p. 169) said: "it appears to date from the Mindel glaciation, and its age is somewhere between 250,000 and 450,000 years."

The German anthropologist Johannes Ranke, an opponent of evolution, wrote in the 1920s that the Heidelberg Jaw belonged to a representative of *Homo sapiens* rather than an apelike predecessor (Wendt 1972, p. 162).

Even today, the Heidelberg Jaw remains somewhat of a morphological mystery. The thickness of the mandible and the apparent lack of a chin are features common in *Homo erectus*. But mandibles of some modern Australian aboriginals are also massive compared to jaws of modern Europeans and have chins that are less well developed (Le Gros Clark and Campbell 1978, p. 96, figure 11).

According to Frank E. Poirier (1977, p. 213), the teeth in the Heidelberg Jaw are closer in size to those of modern *Homo sapiens* than those of Asian *Homo erectus* (Java man and Peking man). T. W. Phenice of Michigan State University wrote (1972, p. 64): "the teeth are remarkably like those of modern man in almost every respect, including size and cusp patterns."

Modern opinion thus confirms Ranke, who wrote in 1922: "The teeth are typically human: the canines do not project above the level of the other teeth, and the third molar, which in primitive races of men—for instance often in the aboriginal Australians—is similar in size to or even larger than the second, is smaller in the Heidelberg Jaw, just as in our more advanced races today" (Wendt 1972, p. 162).

Many *Homo erectus* jaws are characterized by projecting canines and a diastema, a gap in the teeth that accommodates the tip of a projecting canine. The fact that these features were absent in the Heidelberg Jaw, and other considerations, led Poirier (1977, p. 213) to question: "is Heidelberg a representative of *Homo erectus* or a primitive member of the species *H. sapiens*?"

The Heidelberg Jaw is one of the few European fossils generally attributed to *Homo erectus*. Another is the Vertesszöllos occipital fragment, from a Middle Pleistocene site in Hungary.

The morphology of the Vertesszöllos occipital is even more puzzling than that of the Heidelberg Jaw. David Pilbeam (1972, p. 169) wrote: "the occipital bone does not resemble that of *H. erectus*, or even archaic man, but instead that of earliest modern man. Such forms are dated elsewhere as no older than 100,000 years." Pilbeam believed the Vertesszöllos occipital to be approximately the same age as the Heidelberg jaw, between 250,000 and 450,000 years old. If the Vertesszöllos occipital is modern in form, it helps confirm the genuineness of anatomically modern human skeletal remains of similar age found in England at Ipswich (Section 6.1.3) and Galley Hill (6.1.2.1).

Returning to the Heidelberg Jaw, we note that the circumstances of discovery were less than perfect. If an anatomically modern human jaw had been found by a workman in the same sand pit, it would have been subjected to merciless criticism and judged recent. After all, no scientists were present at the moment of discovery. But the Heidelberg Jaw, because it fits, however imperfectly, within the bounds of evolutionary expectations, has been granted a dispensation.

### 7.3 Further Java Man Discoveries by Von Koenigswald

In 1929, another ancient human ancestor was discovered, this time in China. Eventually, scientists would group Java man, Heidelberg man, and Peking man together as examples of *Homo erectus*, the direct ancestor of *Homo sapiens*. But initially, the common features and evolutionary status of the Indonesian, Chinese, and German fossils were not obvious, and paleoanthropologists felt it particularly necessary to clarify the status of Java man.

In 1930, Gustav Heinrich Ralph von Koenigswald of the Geological Survey of the Netherlands East Indies was dispatched to Java. In his book *Meeting Prehistoric Man*, von Koenigswald (1956, p. 55) wrote: "despite the discovery of Peking man, it remained necessary to find a further *Pithecanthropus* sufficiently complete to prove the human character of this disputed fossil."

Upon personally examining Dubois's *Pithecanthropus* skull, von Koenigswald (1956, p. 33) had noted: "it is no more than a calvarium from which the most important parts are missing—the temporal region, which is essential to an accurate assessment of its nature." Dubois had attributed three teeth (two large molars and one premolar) to his Java man specimen, but von Koenigswald believed that only one of them belonged to *Pithecanthropus*. The others were apparently from the jaw of an orangutan. von Koenigswald (1956, p. 34) concluded: "it therefore becomes manifest on what shaky ground Dubois erected his hypothetical building, and we can only wonder at the boldness and tenacity with which he defended his *Pithecanthropus*."

Von Koenigswald, like Dubois, was fascinated by fossils as a youth, having also gathered a collection of ancient bones, teeth, and shells. He managed to put himself through a university education in Germany during the troubled years following the First World War, and upon graduation obtained a position as a museum assistant in Munich. In 1930, he took an opportunity to join the Geological Survey in the Dutch East Indies, where his finds would eventually gain him a reputation as one of the twentieth century's greatest fossil hunters.

#### 7.3.1 The Ngandong Fossils

Von Koenigswald arrived in Java in January 1931. In August of that same year, one of von Koenigswald's colleagues, the Dutch archaeologist ter Haar, was surveying the Kendeng Hills region near Trinil. He set up camp at the kampong of Ngandong on the River Solo. One evening at sundown, while going to the river for a bath, he happened upon a terrace of old river gravels, from which he pulled out a buffalo skull and some other bones. A trained native collector, or mantri, named Samsi, who was employed by the Geological Survey, was given the job of excavating the site. Samsi dutifully sent boxes of fossils back to the city of Bandung, where they were examined by Dr. W. F. F. Oppenoorth, the head of the Geological Survey. On September 15, 1931, Oppenoorth examined a specimen labeled by Samsi as a tiger skull and determined it was actually the major portion of a humanlike braincase. More fragments turned up in the boxes of bones arriving in Bandung, and others were turned over to Oppenoorth at the Ngandong site. Von Koenigswald (1956, pp. 65-77) classified the Solo specimens discovered in the fall of 1931 as a Javanese variety of Neanderthal, appearing later in time than *Pithecanthropus erectus*.

#### 7.3.2 First Find at Sangiran

Gradually, the history of human ancestors in Java seemed to be clearing up, but more work was needed. In 1934, von Koenigswald journeyed to Sangiran, a site west of Trinil on the Solo River. He took with him several Javanese workers, including his trained collector, Atma, who also served as von Koenigswald's cook and laundryman in the field.

Von Koenigswald (1956, p. 88) wrote: "there was great rejoicing in the kampong over our arrival. The men gathered all the

Jaws and teeth they could lay hands on and offered to sell them to us. Even the women and girls, who are generally so retiring, took part." When one considers that most of the finds attributed to von Koenigswald were actually made by local villagers or native collectors, who were paid by the piece. In most cases, the scene described cannot but cause some degree of uneasiness.

At the end of 1935, because of funding cutbacks in the midst of the worldwide economic depression, von Koenigswald's position with Java's Geological Survey was terminated. Undeterred, von Koenigswald kept his servant Atma and others working at Sangiran, financing their activities with contributions from his wife and colleagues in Java.

Uncovered during this period was what appeared to be the fossilized right half of the upper jaw of an adult *Pithecanthropus erectus*. This fossil jaw from Sangiran is designated SLA in table 7.2 on p. 498. An examination of many reports by von Koenigswald has failed to turn up any description by him of exactly how this specimen was found. But the British researcher K. P. Oakley and his associates stated (Oakley et al. 1975, p. 108) that the fossil was found in 1936 on the surface of exposed lake deposits east of Kali Joso in central Java by collectors employed by von Koenigswald.

Considering that SLA was a surface find, it is surprising that modern authorities (Oakley et al. 1975, p. 109) have concluded that this fossil is of the same Middle to early Pleistocene age as the exposed Djétis beds where it was found (see Section

7.5.1 for more on the age of the Djétis beds). The fact that the SLA upper jaw fragment is fossilized is not a guarantee of any great age, because there is evidence that bone can be fossilized in periods as short as a few hundred years. It might be argued that in the Sangiran region there are no strata younger

than those of the Middle Pleistocene Kabuh formation, which lie over the Djétis beds of the Putjangan formation. And therefore the jaw should be at least Middle Pleistocene in age. But the SLA jaw was said to have been found by paid collectors, who may have imported it from almost anywhere.

As we have seen in our discussions of anomalous discoveries made in Argentina and elsewhere, professional scientists sometimes question the credibility and honesty of paid collectors. Boman (Section 5.2.5), for example, said such persons are always suspect. If so, that judgment should also automatically apply to the collectors who found the SLA jaw and other Java *Homo erectus* fossils, all of which are completely accepted by the scientific community.

A more reasonable approach would be to separately evaluate the qualification of the collectors involved in particular discoveries. Lorenzo Parodi, the collector who worked for C. Ameghino at Miramar, Argentina, it may be recalled, left his discoveries in place for scientists to photograph in situ and excavate, and was not known to have engaged in any kind of deception over the course of a long career.

As we shall see in connection with the subsequent hominid finds reported by von Koenigswald, his Javanese collectors were often implicated in questionable behavior. Therefore, we do not really know the exact place of discovery of the Slajaw reportedly found at KaliJoso in 1936.

In addition, we must keep in mind that scientists themselves are not always honest. We shall explore this subject in some detail in chapter 8, which deals with the infamous Piltdown Incident.

At this point, an anthropologist might observe that the Slajaw fragment exhibits the features of *Homo erectus*, as *Pithecanthropus erectus* is now known. Hence it must have been deposited at least several hundred thousand years ago, despite the fact that it was found on the surface. But this is reasoning from theory to fact, not from fact to theory. What if there were existing in geologically recent times, or even today, a rare species of hominid having physical features similar to those of *Homo erectus*? In that case one could not automatically assign a date to a given bone based on the physical features of that bone. In chapter 10 can be found evidence suggesting that a creature like *Homo erectus* has lived in recent times and in fact may be alive today.

As far as chemical dating of finds such as the Slajaw is concerned, we discuss this in Sections 7.5.2 and 7.5.3.

We do not insist that the Slajaw is recent. It might very well be several hundred thousand years old, and we would have no problem with that. What we do object to is the uneven application of standards for evaluating paleoanthropological evidence. In affirming the contemporaneity of the Slajaw with the Djétis beds, scientists have applied such standards more leniently than they have in denying great antiquity to the anomalous finds discussed in previous chapters.

### 7.3.3 The Role of the Carnegie Institution

During the difficult year of 1936, in the course of which the fossil Jaw discussed above was uncovered, the unemployed von Koenigswald received a remarkable visitor—Pierre Teilhard de Chardin, whom von Koenigswald himself

had invited to come and inspect his discoveries in Java. Teilhard de Chardin, a world-famous archaeologist and Jesuit priest, had been working in Peking (now Beijing), where he had participated in the Peking man excavations at Choukoutien (now Zhoukoudian). One reason for his coming to Java was that he desired to establish a link between Peking man and Java man.

During his visit to Java, Teilhard de Chardin advised von Koenigswald to write to John C. Merriam, the president of the Carnegie Institution (Cuenot 1958). von Koenigswald did so, informing



Merriam that he was on the verge of making Important new Pithecanthropus finds. Teilhard de Chardin, who was personally acquainted with most of the leading paleoanthropologists of the day, also wrote in support. Teilhard de Chardin's biographer Cuenot (1958, p. 163) stated: "One has the impression of a vast web, of which Teilhard held in parts the threads, where he served as a liaison agent, or better still, as chief of staff, able, like a magician, to make American money flow, or at least to channel it for the greatest good of paleontology."

Merriam responded positively to von Koenigswald's letter, inviting him to come to Philadelphia in March 1937 to attend the Symposium on early Man, sponsored by the Carnegie Institution. There von Koenigswald joined many of the world's leading scientists working in the field of human prehistory.

One of the central purposes of the meeting was to form an executive committee for the Carnegie Institution's financing of paleoanthropological research. Suddenly, the impoverished von Koenigswald found himself appointed a research associate of the Carnegie Institution and in possession of a large budget.

Considering the critical role played by private foundations in the financing of research in human evolution, it might be valuable at this point to further consider the motives of the foundations and their executives. The Carnegie Institution and John C. Merriam provide an excellent case study. In chapter 9, we will examine the Rockefeller Foundation's role in financing the excavation of Peking man.

The Carnegie Institution was founded in January 1902 in Washington, D.C., and a revised charter approved by Congress became effective in 1904. The Institution was governed by a board of 24 trustees, with an executive committee meeting throughout the year, and was organized into 12 departments of scientific investigation, including experimental evolution. The Institution also funded the Mt. Wilson Observatory, where the first systematic research leading to the idea that we live in an expanding universe was conducted. Thus the Carnegie Institution was actively involved in two areas, namely evolution and the big bang universe, that lie at the heart of the scientific cosmological vision that has replaced earlier religiously inspired cosmologies.

It is significant that for Andrew Carnegie and others like him, the impulse to charity, traditionally directed toward social welfare, religion, hospitals, and general education, was now being channeled into scientific research, laboratories, and observatories. This reflected the dominant position that science and its world view, including evolution, were coming to occupy in society, particularly within the minds of its wealthiest and most influential members, many of whom saw science as the best hope for human progress.

John C. Merriam, president of the Carnegie Institution, believed that science had "contributed very largely to the building of basic philosophies and beliefs" (1938, p. 2531), and his support for von Koenigswald's fossil-hunting expeditions in Java should be seen in this context. A foundation like the Carnegie Institution had the means to use science to influence philosophy and belief by selectively funding certain areas of research and publicizing the results. "The number of matters

which might be investigated is infinite," wrote Merriam (1938, p. 2507). "But it is expedient in each period to consider what questions may have largest use in furtherance of knowledge for the benefit to mankind at that particular time. "

The question of human evolution satisfied this requirement. "Having spent a considerable part of my life in advancing studies on the history of life," said Merriam (1938, p. 2529), "i have been thoroughly saturated with the idea that evolution, or the principle of continuing growth and development, constitutes one of the most important truths obtained from all knowledge.'

By training a paleontologist, Merriam was also by faith a christian. But his Christianity definitely took a backseat to his science. "My first contact with science," Merriam (1938, pp. 2041-2042) recalled in a 1931 speech, "was when i came home from grammar school to report to my mother that the teacher had talked to us for fifteen minutes about the idea that the days of creation described in Genesis were long periods of creation and not the days of twenty-four hours. My mother and i held a consultation—she being a Scotch presbyterian— and agreed that this was rank heresy. But a seed had been sown. i have been backing away from that position through subsequent decades. I realize now that the elements of science, so far as creation is concerned, represent the uncontaminated and unmodified record of what the Creator did."

Having dispensed with scriptural accounts of creation, Merriam managed to turn darwinian evolution into a kind of religion. At a convocation address at the George Washington University in 1924, Merriam (1938, p. 1956) said of evolution, "there is nothing contributing to the support of our lives in a spiritual sense that seems so clearly indispensable as that which makes us look forward to continuing growth or improvement."

He held that science would give man the opportunity to take on a godlike role in guiding that future development. "Research is the means by which man will assist in his own further evolution," said Merriam (1938, pp. 2541-2542) in a 1925 address to the Carnegie Institution's Board of trustees. He went on to say: "I believe that if he [man] had open to him a choice between further evolution directed by some Being distant from us, which would merely carry him along with the current; or as an alternative could choose a situation in which that outside power would fix the laws and permit him to use them, man would say, 'I prefer to assume some responsibility in this scheme.' "

"According to the ancient story," Merriam continued, "man was driven from the Garden of Eden lest he might learn too much; he was banished so that he might become master of himself. A flaming sword was placed at the east gate, and he was ordered to work, to till the ground, until he could come to know the value of his strength. He is now learning to plough the fields about him, shaping his life in accordance with the laws of nature. In some distant age a book may be written in which it will be stated that man came at last to a stage where he returned to the Garden, and at the east gate seized the flaming sword, the sword that symbolized control, to carry it as a torch guiding him to the tree of life." Seizing the flaming sword and marching to take control of the tree of life? One wonders if there would be enough room in Eden for both God and a hardcharging scientific superachiever like Merriam.

#### 7.3.4 Back to Java

Armed with Carnegie grant money, von Koenigswald returned to Java in June of 1937. Immediately upon his arrival, he hired hundreds of natives and sent them out in force to find more fossils.

Meanwhile, in the course of looking through baskets of fossils gathered at Sangiran during his absence, von Koenigswald came upon a large, fossilized, lower right jaw fragment (Sib in table 7.2, p. 498). von Koenigswald stated (1940a, p. 142) that the fossil had been lying on the surface at the time it was discovered. von Koenigswald then asked his native collectors to specify the exact location at which it was discovered, and they informed him that it was found at a place where the Djatis beds of the Putjangan formation are exposed. von Koenigswald searched this area, but he stated that he was unable to locate the exact spot at which this fossil was said to have been found.

Adhering to the SibJaw fragment was a fine-grained conglomerate, the presence of which caused von Koenigswald (1937, p. 884) to conclude that the fossil had originally been embedded in the Kabuh formation, which lies above the Putjangan formation. It is in the early Middle Pleistocene Trinil beds of the Kabuh formation that Dubois reported he found the original *Pithecanthropus* specimens. Two years later, in 1939, after the conglomerate surrounding the SibJaw fragment had been removed in the Cenozoic Research Laboratory in Peking, it was observed that the fossil had fine cracks in it. Such cracks are typically reported on specimens that have been embedded in a clayey layer, such as one would find in the Putjangan formation. Also, this fossil was more heavily fossilized than most bones found in the Kabuh formation. In light of these new facts, von Koenigswald (1940a, p. 142) reversed himself and declared that the SibJaw must have come from the Putjangan formation, considered early Middle Pleistocene or late early Pleistocene (Section 7.5.1).

A radically different opinion was expressed by Dubois, who thought that the Jaw belonged to *Homo soloensis* (the Javanese neanderthal) and therefore was only about 100,000 years old (von Koenigswald 1956, p. 93). von Koenigswald countered that such recent layers were not present at Sangiran.

But the fact remains that the Jaw was said to have been discovered by native collectors on the surface at a location the collectors themselves could not clearly remember, and thus we do not know for sure where the Jaw was originally situated. It is apparent from the above discussion that the actual age of the Jaw is unknown.

At Mojokerto, in 1936, one of von Koenigswald's native collectors, Andojo, discovered the skullcap of a young hominid and labeled it an orangutan. On unpacking the specimen, von Koenigswald reported that the skull exhibited features that are typically human, and not those of an ape; yet its brain capacity was smaller than that of an anatomically modern human of corresponding age. H. de Terra (1943, p. 443) stated that "since the facial part and the base of the skull are missing, its true phylogenetic rank is unknown." But today most paleoanthropologists believe several features of the skull indicate it belonged to a *Homo erectus* child.

von Koenigswald, relying on AndoJo's statement that the skull was dug up from a depth of 3 feet at ModJokerto, concluded that the skull was found embedded in the putjangan layers. the real location of this skull was, however, known only by AndoJo, because von Koenigswald was not present at the time of the discovery. During the course of most of the Sangiran finds, von Koenigswald remained at Bandung, about 200 miles away, although he would sometimes travel to the fossil beds after being notified of a discovery. AndoJo's credibility is suspect. because as we shall see in the description of the next discovery, the Javanese collectors employed by von Koenigswald were sometimes driven, by a desire for extra profit. to engage in deception and trickery.

In the fall of 1937, one of von Koenlgswald's collectors, Atma, mailed him a temporal bone that apparently belonged to a thick, fossilized, hominid cranium. this specimen (S2 in table 7.2, p. 498) was said to have been discovered near the bank of a river named the Kali tjemoro, at the point where it breaks through the sandstone of the Kabuh formation at Sanglran.

von Koenigswald took the night train to central Java and arrived at the site the next morning. "We mobilized the maximum number of collectors," stated von Koenigswald (1956, pp. 95-96). "i had brought the fragment back with me, showed it round, and promised 10 cents for every additional piece belonging to the skull. that was a lot of money, for an ordinary tooth brought in only 1 cent or 1 cent. We had to keep the price so low because we were compelled to pay cash for every find; for when a Javanese has found three teeth he Just won't collect any more until these three teeth have been sold. consequently we were forced to buy an enonnous mass of broken and worthless dental remains and throw them away in Bandung—If we had left them at Sangiran they would have been offered to us for sale again and again."

the highly motivated crew quickly turned up the desired skull fragments. von Koenlgswald (1947, p. 15) would later recall: "there, on the banks of a small river, nearly dry at that season, lay the fragments of a skull, washed out of the sandstones and conglomerates that contained the trlnil fauna. With a whole bunch of excited natives, we crept up the hillside, collecting every bone fragment we could discover. i had promised the sum of ten cents for every fragment belonging to that human skull. But i had underestimated the 'big-business' ability of my brown collectors. the result was terrible! Behind my back they broke the larger fragments into pieces in order to increase the number of sales! .. We collected about 40 fragments, of which 30 belonged to the skull. . . . they formed a fine, nearly complete Pithecanthropus skullcap. Now, at last, we had him!"

How did von Koenlgswald know that the fragments found on the surface of a hill really belonged, as he claimed, to the Middle pleistocene Kabuh formation? perhaps the native collectors found a skull elsewhere and broke it apart, sending one piece to von Koenigswald and scattering the rest by the banks of the Kali tjemoro.

von Koenlgswald constructed a skull from the 30 fragments he had collected, calling It Pithecanthropus II, and sent a preliminary report to dubois. the skull (S2 In table 7.2, p. 498) was much more complete than the original skullcap found by dubois at trlnil. von Koenigswald (1956, pp. 97-99) had always thought that dubois had reconstructed his Pithecanthropus skull with too

low a profile, and believed the Pithecanthropus skull fragments he had just found allowed a more humanlike interpretation. Dubois, who by this time had concluded his original Pithecanthropus was merely a fossil ape (von Koenigswald 1956, p. 55), disagreed with von Koenigswald's reconstruction and published an accusation that he had indulged in fakery. He later retracted this indictment and said that the mistakes he saw in von Koenigswald's reconstruction were probably not deliberate.

But von Koenigswald's position was gaining support. Franz Weidenreich, supervisor of the Peking man excavations at Choukoutien, stated (1938, p. 378) in the prestigious *Journal Nature* that von Koenigswald's new finds had definitely established Pithecanthropus as a human precursor and not a gibbon as claimed by Dubois.

Weidenreich journeyed to Java and participated in another discovery, known as Pithecanthropus III (S3 in Table 7.2, p. 498). This find consisted of many skull fragments, adding up to the right parietal bone, part of the left parietal bone, and a small piece of the occipital bone of a juvenile individual. von Koenigswald stated (1940a, p. 102) that most of these fragments were found by his collectors in July 1938 in the southern sector of the Sangiran dome. A few were discovered by himself and Weidenreich in the course of their visit to the site in September 1938. von Koenigswald (1940a) went on to describe the condition of these skull fragments. They were found on the surface, and they were extensively corroded. In fact, von Koenigswald wrote that these pieces of bone must have been lying around on the surface for a long time, because roots of grasses were penetrating a piece of one of the fragments. Despite this, von Koenigswald (1940a, p. 103; 1956, p. 101) and Le Gros Clark and Campbell (1978, p. 94) stated that Pithecanthropus III is from the Kabuh formation. But considering that this specimen was discovered on the surface, there is room to doubt this. In his 1956 book *Meeting Prehistoric Man* von Koenigswald failed to mention that the Pithecanthropus II skull was found on the surface, thus misleading the reader into accepting it as strong evidence for a missing link in the Middle Pleistocene.

#### 7.3.5 A Meeting in Peking

In January 1939, von Koenigswald and Weidenreich met at the Cenozoic Research Laboratory in Peking to directly compare fossils of Pithecanthropus and Sinanthropus, as Peking man was known in scientific circles. Peking man was represented by some fairly complete skulls with features thought to be markedly human. The humanlike nature of Peking man was further supported by the presence of crude stone implements and evidence of fire in the excavation at Choukoutien. All of this indicated Sinanthropus was much more than an ape. At their meeting, von Koenigswald and Weidenreich agreed that Pithecanthropus and Sinanthropus were anatomically very closely related. So if Peking man was a distant ancestor of the human type, then so was Java man. von Koenigswald (1956, pp. 47-48) wrote: "the cranial curve of Peking man was exactly similar to that of the disputed Javanese Pithecanthropus. Since there could be no doubt that the Peking man, despite all his primitive characteristics, was genuinely human, practically all Dubois's opponents were convinced by this new find that Pithecanthropus, too, must have been human."

Von Koenigswald's characterization of Pithecanthropus and Sinanthropus as "genuinely human" refers only to certain departures from apelike morphology in the direction of humanlike morphology. But such departures do not require one to conclude that modern humans descended from Pithecanthropus or Sinanthropus. Furthermore, one cannot rule out the possibility that humans of modern type existed contemporaneously with or previous to Pithecanthropus and Sinanthropus. As we have seen, there is much evidence demonstrating this latter possibility.

concerning the alleged identity of Pithecanthropus and Sinanthropus, dubois himself was not convinced. "the discovery of

peking man might have been expected to represent a great triumph for dubois, who had up till then been exerting every ounce of his authority to convince the world that the disputed [Java man] fossil was human," stated von Koenigswald (1956, p. 55). "curiously enough, this was not how dubois saw it. till the end of his life [in 1940] he refused to recognize any affinity between Sinanthropus and his Pithecanthropus. He described Sinanthropus as a degenerate neanderthaler, and suddenly decided that his own find must be ascribed to a gibbon-like ape."

While in peking, von Koenigswald received from his collector Rusman a new Pithecanthropus fossil, a thickly encrusted

upper jaw. Later von Koenigswald's Javanese servants sent from the same site another piece of rock that appeared to match the broken piece of rock in which the Jaw had been embedded. in this new piece of rock were found the fragments of the rear part of a cranium. Although the cranium had no direct connection with the upper Jaw, both were attributed to the same individual, Pithecanthropus IV (von Koenigswald 1956, pp. 105-106; Oakley et al. 1975, p. 109).

von Koenigswald said that the jaw was found in the upper layer of the Black clay stratum of the putjangan formation at Sangiran ( Weidenreich 1945, p. 14). Although von Koenigswald reported on this find in five different publications (1939 pp. 926 -929, with Weidenreich as coauthor; 1940a, p. 52; 1947, p. 48; 1949a, p. 92; 1956, pp. 105-111), in none of these did he state the exact location in the stratum of the Jaw or the rear braincase. He did not state if the stratum was undisturbed or at what depth the fossils (S4 in table 7.2, p. 498) were found.

considering the importance of these discoveries, the reader is entitled to the detailed stratigraphic information that should have been provided.

One can simply imagine what might have happened if the discovery had been a fossil skull of the modern human type. Authorities such as Hrdlicka would have pointed out that it had been discovered by a native collector and not a trained scientist, that the exact location of its discovery was unknown, and that there was, therefore, sufficient reason to reject the find. But sloppiness that would be fatal to an anomalous find is easily tolerated in the case of a find that fits in nicely with accepted ideas about human evolution.

#### 7.3.6 Weidenreich's Reconstruction

in 1945, Weidenreich used the S1b lower Jaw, found in 1936 near Kalijoso, and the upper Jaw and rear braincase of the so-called *Pithecanthropus* IV individual (S4) to put together his famous *Pithecanthropus robustus* reconstruction. this was surely a strange mix. von Koenigswald and more recent authors such as Le Gros Clark and Campbell (1978) uncritically accepted that the rear braincase and upper jaw belonged to the same individual although no empirical data was ever brought forward to prove this. the S1b lower jaw was found at a different site. Furthermore, in

Weidenreich's *Pithecanthropus robustus* reconstruction, there were no fossils for the front half of the cranium and the upper part of the face. von Koenigswald (1949a, p. 92) concluded that Weidenreich modeled the facial part of the *Pithecanthropus robustus* skull after the Peking man fossils and the front half of the cranium after Dubois's original *Pithecanthropus* skullcap. despite the considerable guesswork involved with this reconstruction (Figure 7.4) many paleoanthropologists have accepted it as valid.

Figure 7.4. Reconstruction of the Java man (*Pithecanthropus robustus*) skull by Franz Weidenreich (1945, plate 4). the only bone fragments used in the reconstruction comprised the rear part of the cranium and the upper and lower jaws. these were from different sites.

thirty years later, however, Grover S. Krantz presented a very convincing case that the upper Jaw used in the reconstruction did not belong to the same individual as the rear part of the skull.

After making detailed measurements, Krantz (1975, pp. 363-365) concluded that the upper Jaw (palate) was much too wide to fit with the rear braincase, Weidenreich appears to have realized this back in 1945 when he did the reconstruction. Krantz noted that Weidenreich artificially spread apart two key bones of the rear braincase (called the mandibular fossae) "without any explanation," Krantz (1975, p. 366) stated that the mandibular fossae, even after Weidenreich spread them apart, were nevertheless "much too close together to accommodate the palate breadth,"

It is interesting that in the same volume in which Krantz's report appeared, von Koenigswald stated that the width of the palate as Krantz had measured it—94 millimeters—was incorrect (tuttle 1975, p. 377), von Koenigswald arrived at a width narrower than 94 millimeters, which he said allowed the palate to perfectly fit the braincase, But following von Koenigswald's statement came a statement by Krantz saying that his own measurement of the palate width was correct and was identical to Weidenreich's 1945 measurement of 94 millimeters, According to Krantz, before the upper Jaw was discovered it was broken through the socket of the first left incisor; then the greater part of its left half was shifted toward the midline and fossilized in that position. Krantz

reported that both he (working in the 1970s) and Weidenreich (working in 1945) realized this and moved the left part of this upper Jaw back to its original position, Krantz pointed out that von Koenigswald apparently measured the fossil in its distorted condition (without moving the left half back) and that von Koenigswald's measurement was therefore wrong. Following Krantz's statement there was no published reply by von Koenigswald, it seems, therefore, that von Koenigswald was wrong in his measurement and that Krantz was correct. This supports Krantz's contention that the upper Jaw does not belong to the same individual as the rear braincase.

According to Krantz, the teeth in the upper Jaw were positioned like those in the upper Jaw of an ape such as an orangutan. Krantz (1975, p. 369) therefore proposed "that the palate previously related to the Javan *Homo erectus* skull iv should be removed from this association" and "assigned to the genus *Pongo*, large Asiatic apes." If Krantz's assessment is correct, then both Weidenreich and von Koenigswald were apparently unable to distinguish an ape palate from a *Homo erectus* palate, this is especially damaging to Weidenreich's skull reconstruction, which included the questionable palate,

### 7.3.7 More Discoveries by Von Koenigswald

West of Trinil there is an area where the Kabuh formation comes to the surface. At this location primitive stone tools are also present. von Koenigswald (1940b) stated that in this area a fragment of a heavy mandible (S5 in table 7.2, p. 498) came to light in 1939.

By now the reader may be hoping that there might be a detailed report of the strata in which this fossil was discovered so that a proper date can be assigned to it. Such hopes must remain unfulfilled in this case. Von Koenigswald (1949b, p. 110) stated explicitly that this fossil was called "*Pithecanthropus diibius*" because its original position was unknown. In a later report, von Koenigswald (1968a, p. 102) flatly admitted that this fossil was a surface find. He thought that it must originally have come from the Black clay stratum of the Pujangan formation, but he was not sure. Therefore, this fossil cannot be assigned to a particular point in geological time, which makes it next to useless as paleoanthropological evidence.

In 1941, one of von Koenigswald's native collectors, at Sangiran, sent to him, at Bandung, a fragment of a gigantic lower Jaw (S6 in table 7.2, p. 498). According to von Koenigswald (1956, p. III), it displayed the unmistakable features of a human ancestor's Jaw. He named the Jaw's owner *Meganthropus palaeojavanicus* ("giant man of ancient Java") because the Jaw was twice the size of a typical modern human Jaw.

von Koenigswald believed that the S6 Jaw was discovered in the Pujangan formation near the site where the Mojokerto child's skull was found. A careful search of original reports has not revealed a description of the exact location at which the S6 Jaw was found, or who discovered it. If von Koenigswald did report the exact circumstances of this find then it is a well-kept secret. He discussed *Meganthropus* in at least three reports (von Koenigswald 1956, pp. 111-113; 1949a, p. 92; 1949b, p. 107); however, in none of these did he inform the reader of the details of the fossil's original location. All he said was that it came from the Pujangan formation, but no further



Information was supplied. Hence all we really know for certain is that some unnamed collector mailed a Jaw fragment to von Koenigswald. Its age, from a strictly scientific standpoint, remains a mystery.

Meganthropus, in the opinion of von Koenigswald, was a giant offshoot from the main line of human evolution. Von Koenigswald had also found some large humanlike fossil teeth, which he attributed to an even larger creature called Gigantopithecus. According to von Koenigswald, Gigantopithecus was a large and relatively recent ape. But Weidenreich, after examining the Meganthropus Jaws and the Gigantopithecus teeth, came up with another theory. He proposed that both creatures were direct human ancestors. According to Weidenreich, Homo sapiens evolved from Gigantopithecus by way of Meganthropus and Pithecanthropus (Simons and ettel 1970, p. 77). each species was smaller than the next. Most modern authorities, however, consider Gigantopithecus to be a variety of ape, living in the Middle to early pleistocene, and not directly related to humans. the Meganthropus Jaws are now thought to be much more like those of Java man (Homo erectus) than von Koenigswald originally believed. Some researchers, however, have suggested that Meganthropus fossils might be classified as Australopithecus (Oacob 1973, p. 475). this is intriguing, because according to standard opinion, Australopithecus never left its African home.

#### 7.4 Later Discoveries In Java

Megwthropus was the last major discovery reported by von Koenigswald, but the search for more bones of Java man has continued up to the present. We shall now discuss the most important of the later finds, which are uniformly accepted as evidence for Homo erectus in the Javanese Middle and early pleistocene (Le Gros Clark and Campbell 1978, p. 94). the discoveries were all made in the Sangiran region.

In September 1952, P. Marks, a member of the science faculty at the University of Indonesia at Bandung, happened to pass by the fossil beds at Sangiran, at which time a local villager handed him a large fragment of a heavily fossilized mandible (S8 in Table 7.2, p. 498). Later, Marks analyzed this mandible and concluded it belonged to von Koenigswald's Meganthropus. Marks (1953, p. 26) stated that the jaw fragment was found lying loose on the surface north of the village of Glagahombo, on a slope of strongly cemented conglomerate, consisting mainly of small pebbles of volcanic origin, numerous fragments of vertebrate bones were present within this conglomerate. Marks noted that the material clinging to the S8 jaw fragment was of the same type as the conglomerate of the slope. He added, however, that it was not possible "to collect associated vertebrate remains of stratigraphical value." From the information provided by Marks in his report, one cannot assign a specific age to this fossil. In light of this, it is surprising that Marks advocated that this fossil was from the Middle pleistocene Kabuh formation and that this judgement is accepted without question by other modern authorities.

In 1960, near the village of Mlandintgan in the vicinity of Sangiran, a villager discovered a highly fossilized right mandible (S9 in Table 7.2, p. 498) on the surface of a hill slope. the bones of other vertebrates had also been discovered in this area. the S9 fossil consisted of the right half of the

jaw and contained five teeth. T. Jacob, of the department of physical anthropology at Gadjah Mada University in Jogjakarta, Indonesia, proposed that the S9 mandible belonged to one of the species of *Pithecanthropus*.

This right mandible was incrustated by a matrix containing foraminifera (small marine organisms) that S. Sartono (1964) reported were exactly the same as the foraminifera of the Putjangan formation, which is considered older than the Middle Pleistocene Kabuh formation (Section 751).

Jacob (1964) reported that in August 1963 an Indonesian farmer discovered fragments of a fossilized skull "in the Sangiran dome area while working in the field." When assembled, these skull fragments formed what appeared to be a skull (S10 in table 72, p. 498) similar to the type that is designated as *Homo erectus*.

Although Jacob stated that this skullcap was deposited in the Kabuh formation during the Middle Pleistocene, he gave no more information than "a farmer discovered fragments of a fossil skull in the Sangiran dome area while working in the field." Jacob (1964) did not state the exact position of the fragments when found. All we really know is that a farmer discovered some fossil skull fragments that were most likely on or close to the surface.

There were two other reports on the S10 find: Sartono (1964) and Jacob (1966). The report by Sartono (1964, p. 3) provided a diagram of the beds of the Kabuh formation and the overlying Notopuro beds. The S10 skull was assigned to layer 8 in the Kabuh formation. In the general area of the discovery, layer 8 was at the surface. Sartono (1964) did not tell whether the skull was found lying loose on the ground or firmly embedded in layer 8.

Jacob (1966, p. 244) reported that the fragments making up the S10 skull were discovered in two successive months. The first group of fragments was found in July 1963 and was reported by Sartono (1964). The second group was found in August 1963 and was reported in Jacob (1964). Unfortunately, Jacob (1966) did not provide any more definite information about the location and situation of either group of fragments at the time of their discovery than can be found in Sartono (1964) or in his own earlier report (Jacob 1964).

Jacob (1973, p. 476) made this interesting remark about Sangiran, where the S10 find and all the other finds discussed in this section were made: "The site seems to be still promising, but presents special problems. . . this is mainly due to the site being inhabited by people, many of whom are collectors who had been trained in identifying important fossils. Chief collectors always try to get the most out of the primate found accidentally by primary discoverers (Jacob 1964). In

addition, they may not report the exact site of the find, lest they lose one potential source of income. Occasionally, they may not sell all the fragments found on the first purchase, but try to keep a few pieces to sell at a higher price at a later opportunity."

Concerning another find in the Sangiran region, S. Sartono (1967) stated that on January 30, 1965 pieces of a skullcap were obtained by one of his collectors (unnamed) from a local village,

who was also not named. the fossils came into Sartono's possession towards the end of February 1965. this discovery consisted of 44 fragments of a skullcap, which were "collected all around the site of the skull in the field." After restoration, the skullcap (S12 in Table 72, p. 498) consisted of both parietals, the left temporal, part of the left mastoid bone, a large part of the occipital bone, and a small part of the frontal bone. The S12 skull exhibited the features typical of *Homo erectus*.

the pieces of this skullcap were found on the surface (Sartono 1967, p. 85) of a slope southwest of the village of Puljung. At this point, the Kabuh formation was exposed. Sartono stated that the skull was deposited at the same time as the early Middle pleistocene Kabuh beds. Modern authorities have uncritically accepted Sartono's age estimate for the S12 skull. But if the skull had been of modern morphology, one suspects that the same authorities would have used the fact that it was found on the surface to rule out a Middle pleistocene age for it.

On July 20, 1969, one of Sartono's collectors gave him a small fragment of an upper jaw that contained two upper left premolars (S15 in table 72, p. 498). From the jaw's dimensions and shape, Sartono concluded that it belonged to a member of the species *Pithecanthropus modjokertensis*. But modern authorities tend to classify all the *Pithecanthropus* species of earlier researchers (*P. erectus*, *P. modjokertensis*, *P. soloensis*, and *P. robustus*) as *Homo erectus*.

the S15 jaw was said to have been found lying loose on the surface near Ngrejeng village on the northern part of the Sangiran dome, the rock matrix that incrustated it consisted of a grey clay stone. Because the Puljangan beds at this point also have a layer of grey clay stone, Sartono (1974) assumed that this specimen was originally embedded in that layer. Sartono and most other scientists believed the Puljangan formation to be older than the Middle pleistocene Kabuh formation.

there are several reasons why one should be hesitant to accept that the S15 jaw was contemporaneous with the Puljangan formation. First, the credibility of the collector is unknown. He might have obtained this bone from any place where there is a grey clay stone. Second, even if S15 was discovered at the site reported above, because it was found on the surface it may not have been originally a part of the Puljangan beds at this point. Holmes and Hrdlicka, confronted with evidence for anatomically modern human beings in a situation like this, would probably have proclaimed that the fossil had been recently introduced. As always, our point is that a double standard should not be employed in the evaluation of paleoanthropological evidence—an impossibly strict standard for anomalous evidence and an exceedingly lenient standard for acceptable evidence.

Sartono (1972) reported that on September 13, 1969, Mr. Towikromo, a resident of the village of Putjung, accidentally discovered a fossil hominid skull when his plow broke through its crust. According to Sartono (1972, p. 124), the skull (S17 in table 7.2, p. 498) had a low vault.

there are several aspects of Sartono's 1972 report that seem unclear. Sartono stated that Mr. Towikromo was using an iron tool for cultivation of his land when his tool contacted the skull. Sartono also stated explicitly that the skull was embedded in the Kabuh beds, which are here

made of sandstone. Sartono (1972, p. 124) said: "this sandstone forms the base of the blind valley and contains the skull. "

it does not seem probable that the farmer was plowing sandstone. perhaps the lower part of the skull was embedded in the very topmost layer of the Kabuh sandstone, and the upper part of the skull was projecting up into a thin, recently deposited layer of soil that was being cultivated, this may be true, but Sartono (1972) did not state this in his report. It is also not clear just how firmly the skull was embedded in the sandstone layer. perhaps it belonged entirely to a recent soil layer. We must bear in mind that Sartono did not find this skull himself. It is therefore quite possible that it was not firmly embedded in the Kabuh sandstone.

in order to clear up these uncertainties in Sartono's 1972 report. letters were written in 1985 to both him and to T. Jacob for further information about this and several other important discoveries reported by them from Java. no answers were received. One can easily find dozens of popular books describing *Homo erectus* and how this hominid lived between 0.5 and

2 million years ago in Java. but finding a report describing how a particular fossil was situated when discovered is often quite difficult. Many of the popular books do not describe the original situation of the fossil. nor do they give references to reports by the original discoverer. Sometimes these books do give references to reports. But upon reading these reports, one finds no information about the original position of the fossil. Sometimes the references are to reports in journals that are not easily found even at the libraries of major universities. It is therefore often difficult to obtain information describing the original stratigraphic position of a fossil. this means that the fossil cannot be properly assigned a geological age. and if it cannot be assigned a geological age it cannot be inserted into a proposed evolutionary sequence. nevertheless, this is exactly what has been done.

## 7.5 Chemical and Radiometric Dating of The Java *Homo Erectus* Finds

We shall now discuss issues related to the potassium-argon dating of the formations yielding hominid fossils in Java, as well as attempts to date the fossils themselves by various chemical and radiometric methods. See Appendix 1 for general information about these methods.

### 7.5.1 The Ages of the Kabuh and Putjangan Formations

in the foregoing discussion, we have several times referred to the Kabuh formation and the putjangan formation. the original *Pithecanthropus* finds of Dubois were from the trlnil beds of the Kabuh formation. Some of the subsequent finds by von Koenigswald and later researchers were also assigned to the Kabuh formation. Others were assigned to the dJetis beds of the putjangan formation. Many researchers have attempted to establish an age difference between the two formations, placing the Kabuh formation in the Middle pleistocene and the putjangan formation in the early pleistocene.

As we have seen (Section 7.1.5), Eugene Dubois originally attempted, on faunal grounds, to classify the trlnil beds of the Kabuh formation as pliocene (Boule 1923, p. 98), but later researchers have

characterized the Trlnil fauna as post- villafranchian (Le Gros clark and campbell 1978, p. 91) or Middle pleistocene (Hooijer 1951. p. 273; 1956, p. 5).

the trlnil beds of the Kabuh formation have also been dated by the potassium-argon method. potassium-argon dating relies on the fact that volcanic materials contain potassium 40, which decays into the gas argon 40. the argon gas remains trapped in crystals of volcanic material. By comparing the ratios of potassium 40 and argon 40 gas in a sample, one can date volcanic materials (for a fuller discussion of potassium-argon dating, see Section 11.6.5.1). Basalt at Mount Murla, from a layer above the Pithecanthropus erectus level of the trlnil beds, yielded an age of 500,000 years, while tektites (pieces of glass produced by meteors) from the trinil beds yielded potassium-argon dates of 710,000 years (von Koenigswald 1968b, p. 201; Jacob 1973, p. 477). Further potassium-argon tests by G. H. curtis Oacob 1973, p. 477) on pumice from trlnil beds at tanJung and putjung, where the S10 and S12 fossils were found, gave similar ages. Jacob (1973, p. 477) said the average for the above four dates for the trinil beds is

830,0 years. this would put the trinil beds in the early Middle pleistocene.

As far as the Pujangan formation is concerned, it was originally classified as early pleistocene on faunal grounds by von Koenigswald. But d. A. Hooijer (1956) objected to this. He pointed out that both the trinil beds of the Kabuh formation and the dJetis beds of the pujangan formation share fossil species characteristic of the Stegodon-Ailuropoda mammalian fauna of southern china, generally recognized as Middle Pleistocene (Hooijer 1956, p. 7).

According to Hooijer (1956, p. 8), von Koenigswald principally employed molluscan stratigraphy in assigning an early pleistocene age to the dJetis beds. But Hooijer (1956, p. 9), after citing authorities on Javan molluscs, stated: "there is no reason to attach greater importance to the mollusks than to certain mammalian genera of long standing in establishing pleistocene correlations." Hooijer (1956, p. 9) concluded that the trinil and dJetis beds were of roughly the same Middle pleistocene age.

Attempts were later made to establish the geological age of the dJetis beds using the potassium-argon dating method. the dJetis beds of the pujangan formation near ModJokerto yielded an early pleistocene potassium-argon date of about 1.9 million years Oacob 1973, p. 477; Jacob and curtis 1971; Jacob 1972).

The date of 1.9 million years is significant for the following reasons. As we have seen, many Homo erectus fossils (previously designated Pithecanthropus and Meganthropus) have been assigned to the dJetis beds. if these fossils are given an age of 1.9 million years, this makes them older than the oldest African Homo erectus finds, which are about 1.6 million years old (Brown et al. 1985, p. 788). According to standard views, Homo erectus evolved in Africa and did not migrate out of Africa until about 1 million years ago.

Also, some researchers have suggested that von Koenigswald's Meganthropus might be classified as Australopithecus Oacob 1973, p. 475; Jacob and curtis 1971). if one accepts this opinion, this

means that Javan representatives of *Australopithecus* arrived from Africa before 1.9 million years ago or that *Australopithecus* evolved separately in Java. Both hypotheses are in conflict with standard views on human evolution.

it should be kept in mind, however, that the potassium-argon technique that gave the 1.9-million-year date is not anymore reliable than the other dating techniques we discuss in Appendix 1. Jacob and curtis (1971), who attempted to date most of the hominid sites in Java, found "it has been difficult to obtain meaningful dates from most samples." In other words, dates were obtained, but they deviated so greatly from what was expected that Jacob and curtis (1971) had to attribute the unsatisfactory results to "contaminants."

Moreover, according to nilsson (1983, p. 329): 'A much lower [potassium-argon] date for the dJetis Beds, less than 1 million years, is indicated by later studies (Bartstra 1978).' this agrees with Hooijer's conclusion that the dJetis beds are, like the trinil beds, early Middle pleistocene. Finally, M. H. day and T. 1. Molleson (1973, p. 147) reported that fluorine content "analyses of bones from both the dJetis and the trinil faunas at Sangiran showed that it was not possible to distinguish, analytically, the two assemblages at this site."

#### 7.5.2 Chemical Dating of the Trinil Femurs

in Section 7.1.8, we learned that the trinil femurs are indistinguishable from those of modern humans and distinct from those of *Homo erectus*. this has led some to suggest that the trinil femurs do not belong with the *Pithecanthropus* skull and were perhaps mixed into the early Middle pleistocene trinil bone bed from higher levels (day and Molleson 1973, p. 152). Another possibility is that anatomically modern humans were living alongside ape-man-like creatures during the early Middle pleistocene in Java. in light of the evidence presented in this book, this would not be out of the question.

The fluorine content test has often been used to determine if bones from the same site are of the same age. Bones absorb fluorine from groundwaters, and thus if bones contain similar percentages of fluorine (relative to the bones' phosphate content) this suggests such bones have been buried for the same amount of time.

M. H. Day and T. 1. Molleson (1973) analyzed the Trinil skullcap and femurs and found they contained roughly the same ratio of fluorine to phosphate. Middle Pleistocene mammalian fossils at Trinil contained a fluorine-to-phosphate ratio similar to that of the skullcap and femurs. day and Molleson (1973, p. 146) stated that their results (table 7.1) "apparently indicated the contemporaneity of the calotte and femora with the trinil fauna.'

if the trinil femurs are distinct from those of *Homo erectus* and identical to those of *Homo sapiens* sapiens. as day and

Molleson (1973, p. 128) reported, then the fluorine content of the femurs is consistent with the view that anatomically modern humans existed in Java during the early Middle pleistocene, about

800,000 years ago.

Day and Molleson (1973, p. 147) suggested that Holocene bones from the Trinil site might, like the Java man fossils, also have fluorine-to-phosphate ratios similar to those of the Middle Pleistocene animal bones, making the fluorine test useless here. In discussing the La Denise human bones (Appendix 2), Oakley pointed out that the rate of fluorine absorption in volcanic areas, such as Java, tends to be quite erratic, allowing bones of widely differing ages to have similar fluorine contents. This could not be directly demonstrated at the Trinil site, because there only the Middle Pleistocene beds contain fossils.

Day and Molleson (1973, p. 148) showed that Holocene and Late Pleistocene beds at other sites in Java contained bones with fluorine-to-phosphate ratios similar to those of the Trinil bones (table 7.1). But they admitted (Day and Molleson 1973, p. 144) that the fluorine-to-phosphate ratios of bones from other sites "would not be directly comparable" with those of bones from the Trinil site. This is because the fluorine absorption rate of bone depends upon factors that can vary from site to site. Such factors include the groundwater's fluorine content, the groundwater's rate of flow, the nature of the sediments, and the type of bone.

Therefore, the fluorine content test results reported by Day and Molleson remain consistent with (but are not proof of) an early Middle Pleistocene age of about 800,000 years for the anatomically modern human Trinil femurs.

A nitrogen content test was also performed on the Trinil bones. Dubois had boiled the skullcap and the first femur in glue. Day and Molleson (1973, p. 147) attempted to correct for this by "pre-treating the samples in order to remove soluble nitrogen before analysis." Results showed that all of the Trinil bones listed in table 7.1 had very little nitrogen left in them. This is consistent with all of the bones being of the same early Middle Pleistocene age, although Day and Molleson (1973, p. 148) did report that nitrogen in bone is lost so rapidly in Java that even Holocene bones often have no nitrogen. The uranium contents of the Trinil hominid bones and fauna were all almost zero, again consistent with (although not proof of) their being of the same early Middle Pleistocene age.

TABLE 7.1

#### Fluorine Analysis of Bone from Java Sites

Site

$(F/P \times 100) \times 100$

Trinil (early Middle Pleistocene)

Pithecanthropus skullcap

4.1

Femur I shaft

5.8

Femur II shaft

6.2

Femur III shaft

5.7

Femur IV shaft

5.8

Femur V shaft

5.8

Ethos mandible

6.9

Axis antler

6.\_

Wadjak (Holocene)

4.0, 8.2

Ngandong: (Late Pleistocene)

5.6. 9.3

this data is from Day and Molleson (1973, pp. 147-148). the figures represent the ratio of fluorine (F) to phosphate (P<sub>2</sub>O<sub>5</sub>) in the bones.

#### 7.5.3 Uranium Content Testing of the Sangiran Fossils

In our discussion of the Sangiran hominid finds reported by von Koenigswald (Section 7.3) and later researchers (Section 7.4), we learned that almost all occurred on the surface. We suggested that this made their real age uncertain.



Some might infer a Middle or early pleistocene date for the Sangiran fossils on the basis of their equivalent U308 (uranium

oxide) content. For example, the Sia upper jaw discovered in 1936 has an equivalent uranium content of 25 parts per million (ppm), somewhat less than that of 63 ppm for a Cervus (deer) antler from the same general region (Oakley et al 1975, p. 109).

Sangiran la (Sla) was found on the surface. and von Koenigswald simply assumed it weathered out of the putjangan formation. therefore. we are not able to measure the concentration of uranium. either in the groundwater or in other fossils, at the precise location where Sangiran la. if in fact from the putjangan formation, lay buried. if we were able to compare the uranium contents of the SlaJaw and other bones from the same spot and found them similar. that would be consistent with, although not proof of, the view that they were of the same age.

But lacking such evidence, the reported uranium content for the Slajaw itself gives little reason to suggest that the Slajaw is as old as the putjangan formation. We cannot exclude the possibility that Sangiran la is a very young bone that was originally situated in a stratum through which uranium-rich waters percolated.

day and Molleson (1973, p. 148) reported that two Late pleistocene bones from Ngandong had uranium contents of 25 and 30 ppm. These figures are not directly comparable with the uranium content of 25 ppm for the supposedly early Pleistocene Sangiran la jaw, but they do demonstrate the difficulty in interpreting the kind of uranium content data reported by Oakley et al. (1975).

the above line of reasoning is also applicable to the other Sangiran hominid fossils, since they were also found on the surface or in other dubious ways.

## 7.6 Misleading Presentations of The Java Man Evidence

Most books dealing with the subject of human evolution present what appears at first glance to be an impressive weight of evidence for Homo erectus in Java between 0.5 and 2.0 million years ago. One such book is The Fossil Evidence for Human Evolution (1978), by W. e. Le Gros Clark, professor of anatomy at Oxford University. and Bernard G. Campbell, adjunct professor of anthropology at the University of California at Los Angeles. An impressive table showing discoveries of Homo erectus is presented in their book ( Le Gros Clark and Campbell 1978, p. 94). these discoveries (table 7.2, p. 498) have been used widely to support the belief that man has evolved from an apelike being.

t3 is the femur found by Dubois at a distance of 45 feet from the original cranium, T2. We have already discussed how unjustified it is to assign these two bones to the same individual (Sections 7.1.7 and 7.1.8). Yet ignoring many important facts. Le Gros Clark and Campbell stated (1978, p. 91) that "the accumulation of evidence speaks so strongly for their natural association that this has become generally accepted."

t6, t7, t8, and t9 are the femurs found in boxes of fossils in Holland over 30 years after they were originally excavated in Java. Le Gros Clark and Campbell apparently ignored Dubois's statement that he himself did not excavate them, and that the original location of the femurs was unknown. We may also recall von Koenigswald's statement that the femurs were from Dubois's general collection, which contained fossils from "various sites and various ages which are very inadequately distinguished because some of the labels got lost." nevertheless, Le Gros Clark and Campbell (1978, p. 94) assumed that these femurs came from the Trinil beds of the Kabuh formation. But Day and Molleson (1973, p. 130) observed: "if the rigorous criteria that are demanded in modern excavations were applied to all of the Trinil material subsequent to the Calotte and Femur I, it would all be rejected as of doubtful provenance and unknown stratigraphy."

Fossil M1 and fossils S1a through S6 are those discovered by Javanese native collectors employed by von Koenigswald. Only one of them (M1) was reported to have been discovered buried in the stratum to which it is assigned, and even this report is subject to question. The remaining fossils of the S series are the ones reported by Marks, Sartono, and Jacob, and the majority of these were surface finds by villagers and farmers, who sold the fossils, perhaps by way of middlemen, to the scientists. One familiar with the way these specimens were found can only wonder at the intellectual dishonesty manifest in table 7.2 (p. 498), which gives the impression that the fossils were all found in strata of definite age.

In our discussion of the Sheguiandah site in China, where anomalously old stone tools were found by T. E. Lee, we found that an establishment scientist, James B. Griffin, dismissed the discovery because the site did not conform to certain very strict standards (Section 5.4.1.2, pp. 350-352). Griffin and others like him demand to see an intact habitation site, in a clearly defined geological context, complete with stone tools, skeletal remains, signs of deliberate use of fire, remains of animals and plants used as food, and more (1979, p. 44). Otherwise, there is always the chance that an isolated artifact or bone might be intrusive in the layer in which it was found. Griffin felt no hesitation whatsoever in using his criteria to reject as nonsites dozens of places in North America where anomalously old traces of humans had been discovered.

Should not the same strict standards apply in Java? One might argue that Griffin's requirements were intended for Indian sites in North America and not for *Homo erectus* sites in other parts of the world. But according to standard opinion, *Homo erectus* was, like *Homo sapiens*, a toolmaker and user of fire, as shown by *Homo erectus* sites in China, Africa, and Europe. One might therefore expect to find the same kinds of artifacts and signs of habitation at a *Homo erectus* site as at a *Homo sapiens* site. As we have seen, none of the *Homo erectus* sites in Java (over twenty) conform to Griffin's criteria and should therefore be classed as nonsites. No cultural remains whatsoever have been found along with the Java *Homo erectus* fossils, most of which were surface finds.

We regard Griffin's approach as extreme. However, our main objection is not to the stringency of his requirements but to the fairness of their application. If one decides to employ Griffin's criteria, one should do so in all similar cases or none at all. Obviously, if one were to universally apply

Griffin's criteria, much of the paleoanthropological evidence currently accepted by scientists, such as the Java Homo erectus evidence, would have to be thrown out. Since that has not been done, we believe Griffin's strict standards should not be selectively applied to eliminate anomalous discoveries, such as Sheguiandah. Applying the more lenient criteria by which the Java finds have been accepted by the scientific community as evidence for Homo erectus in the Middle pleistocene, the Sheguiandah evidence, and other anomalous evidence, should also be accepted. Although Le Gros Clark and Campbell (1978, p. 93) noted that Hooijer (1951) had said the real location of many of von Koenigswald's finds was unknown, they nevertheless accepted that the fossils must have come from Middle or early pleistocene formations, which they designated 0.7-1.3 and 1.3-2.0 million years of age.

TABLE 7.2 Fossil Hominids from Java

(Kabuli

ation)

(Putjangan

ation)

Sangiran 82 Adult female calotte (1937)

S3 Juvenile calotte (1938)

S8 Right mandible (1952)

SIO Adult male calotte (1963)

S12 Old male calotte (196-)

S 15 Maxilla (1969)

S 17 Cranimn (1969)

S21 Mandible (1973)

Trinil T2 Calotte (1892)

= Pifheca nthropus T3,T6,T7.TS.T9 Femora

Kedimng Bmbus IKBI Right juvenile mandible (1890' )

Sansan Sla Right maxilla (1936)

Slb Right mandible (1936)

this table is reproduced from Le Gros clark and campbell (1978, p. 94). calotte, cranium, and calvaria mean skull, mandible means lower Jaw, maxilla means upper Jaw, and femora means thighbones.

The ages given by Le Gros clark and campbell, derived from the potassiumargon dates discussed in Section

7.5.1, refer only to the age of the volcanic soils, and not to the bones themselves. potassium-argon dates have meaning only if the bones were found securely in place within or beneath the layers of dated volcanic material. But the vast majority of fossils listed in Table 7.2 were surface finds, rendering their assigned potassium-argon dates meaningless.

concerning the age of 1.3-2.0 million years given by Le Gros clark and campbell for the djetis beds of the putjangan formation, we note that this is based on the potassium-argon date of 1.9 million years reported by Jacob and curtis (1971). But Bartstra (1978) obtained a potassium-argon age of less than 1 million years (Section 7.5.1). As we have seen (Section

7.5.1) , other researchers have reported that the fauna of the dJetis and trinil beds are quite similar and that the bones have similar fluorine-to-phosphate ratios.

Le Gros clark and campbell (1978, p. 92) concluded that “at this early time there existed in Java hominids with a type of femur indistinguishable from that of Homo sapiens, though all the cranial

remains so far found emphasize the extraordinarily primitive characters of the skull and dentition.” All in all, the presentation by Le Gros Clark and Campbell was quite misleading. They left the reader with the impression that cranial remains found in Java can be definitely associated with the femurs when such is not the case. Furthermore, discoveries in China and Africa, as previously noted (Section 7.1.8), have shown that *Homo erectus* femurs are different from those collected by Dubois in Java.

Judging strictly by the hominid fossil evidence from Java, all we can say is the following. As far as the surface finds are concerned, these are all cranial and dental remains, the morphology of which is primarily apelike with some humanlike features. Because their original stratigraphic position is unknown, these fossils simply indicate the presence in Java, at some unknown time in the past, of a creature with a head displaying some apelike and humanlike features.

The original *Pithecanthropus* skull (t2) and femur (t3) reported by Dubois were found in situ. and thus there is at least some basis for saying they are perhaps as old as the early Middle Pleistocene Trinil beds of the Kabuh formation. The original position of the other femurs is poorly documented, but they are said to have been excavated from the same Trinil beds as t2 and t3 (Section 7.1.7). In any case, the original femur (t3), described as fully human, was not found in close connection with the primitive skull and displays anatomical features that distinguish it from the femur of *Homo erectus*. There is, therefore, no good reason to connect the skull with the t3 femur or any of the other femurs, all of which are described as identical to those of anatomically modern humans. Consequently, the t2 skull and t3 femur can be said to indicate the presence of two kinds of hominids in Java during the early Middle Pleistocene—one with an apelike head and the other with legs like those of anatomically modern humans. Following the typical practice of giving a species identification on the basis of partial skeletal remains, we can say that the t3 femur provides evidence for the presence of *Homo sapiens sapiens* in Java around

800,000 years ago. Up to now, no creature except *Homo sapiens sapiens* is known to have possessed the kind of femur found in the early Middle Pleistocene Trinil beds of Java.

### The Piltdown Showdown

After Eugene Dubois's discovery of Java man in the 1890s, the hunt for fossils to fill the evolutionary gaps between ancient apelike hominids and modern *Homo sapiens* intensified. It was in this era of strong anticipation that a sensational find was made in England—Piltdown man, a creature with a humanlike skull and apelike jaw.

The outlines of the Piltdown story are familiar to both the proponents and opponents of the Darwinian theory of human evolution. The fossils, the first of which were discovered by Charles Dawson in the years 1908-1911, were declared forgeries in the 1950s by scientists of the British Museum. This allowed the critics of Darwinian evolution to challenge the credibility of the scientists who for several decades had placed the Piltdown fossils in evolutionary family trees.

Scientists, on the other hand, were quick to point out that they themselves exposed the fraud. Some sought to identify the forger as Dawson, an eccentric amateur, or Pierre Teilhard de Chardin, a Catholic priest-paleontologist with mystical ideas about evolution, thus absolving the “real” scientists involved in the discovery.

In one sense, it would be possible to leave the story of Piltdown at this and go on with our survey of paleoanthropological evidence. But a deeper look at Piltdown man and the controversies surrounding him will prove worthwhile, giving us greater insight into how facts relating to human evolution are established and disestablished.

Contrary to the general impression that fossils speak with utmost certainty and conviction, the intricate network of circumstances connected with a paleoanthropological discovery can preclude any simple understanding. Such ambiguity is especially to be expected in the case of a carefully planned forgery, if that is what the Piltdown episode represents. But as a general rule, even “ordinary” paleoanthropological finds are enveloped in multiple layers of uncertainty. As we trace the detailed history of the Piltdown controversy it becomes clear that the line between fact and forgery is often indistinct.

### 8.1 Dawson Gets a Skull

Sometime around the year 1908, Charles Dawson, a lawyer and amateur anthropologist, noticed that a country road near Piltdown, in Sussex, was being mended with flint gravel. Always on the lookout for flint tools, Dawson inquired from the workmen and learned that the flint came from a pit on a nearby estate, Barkham Manor, owned by Mr. R. Kenward, with whom Dawson was acquainted. Dawson visited the pit and asked two workers there to be on the lookout for any implements or fossils that might turn up. In 1913, Dawson wrote: “Upon one of my subsequent visits to the pit, one of the men handed to me a small portion of an unusually thick human parietal bone. I immediately made a search but could find nothing more. ... It was not until some years later, in the autumn of 1911, on a visit to the spot, that I picked up, among the rain-washed spoil-heaps of the gravel pit, another and larger piece belonging to the frontal region of the same skull” (Dawson and Woodward 1913, p. 117). Dawson noted that the pit contained pieces of flint much the same in color as the skull fragments.

Dawson was not a simple amateur. He had been elected a Fellow of the Geological Society and had for 30 years contributed specimens to the British Museum as honorary collector (Weiner 1955, p. 83). Furthermore, he had cultivated a close friendship with Sir Arthur Smith Woodward, keeper of the Geological Department at the British Museum and a fellow of the Royal Society. In February 1912, Dawson wrote a letter to Woodward at the British Museum, telling how he had “come across a very old Pleistocene bed . . . which I think is going to be very interesting . . . with part of a thick human skull in it. . . part of a human skull which will rival *Homo heidelbergensis*” (Bowden 1977, p. 40). Altogether, Dawson had found five pieces of the skull. In order to harden them, he soaked them in a solution of potassium dichromate.

On Saturday, June 2, 1912, Woodward and Dawson, accompanied by Pierre Teilhard de Chardin, a student at a local Jesuit seminary, began excavations at Piltdown and were rewarded with some new discoveries. On the very first day, they found another piece of skull. More followed. Dawson later wrote: "Apparently the whole or greater portion of the human skull had been shattered by the workmen, who had thrown away the pieces unnoticed. Of these we recovered, from the spoil-heaps, as many fragments as possible. In a somewhat deeper depression of the undisturbed gravel I found the right half of a human mandible. So far as I could Judge, guiding myself by the position of a tree 3 or 4 yards away, the spot was identical with that upon which the men were at work when the first portion of the cranium was found several years ago. Dr. Woodward also dug up a small portion of the occipital bone of the skull from within a yard of the point where the Jaw was discovered, and at precisely the same level. The Jaw appeared to have been broken at the symphysis and abraded, perhaps when it lay fixed in the gravel, and before its complete deposition. The fragments of the cranium show little or no sign of rolling or other abrasion, save an incision at the back of the parietal. probably caused by a workman's pick" (Dawson and Woodward 1913, p. 121). A total of nine fossil skull pieces were found, five by Dawson alone and an additional four after Woodward joined the excavation.

Dawson and Woodward decided to keep their discovery quiet until such time as they would officially announce it, but news of the fossils circulated privately among scientists with interest in human prehistory. Sir Ray Lankester wrote to J. Reid Moir in 1912: "It seems possible that it is our Pliocene Man—the maker of rostro-carinate flints! At any rate if they say to us 'you say we call in vague, unknown agencies such as torrents and pressure to produce these flints by natural force, but you are in the same position of calling in a hypothetical man. You have no other evidence that such a man was there!' Now we can say, 'Here he is.' It is wonderful that, after so many years, man's bones should turn up in a gravel. I do not despair now of you finding a sub-Crag human cranium and lower Jaw. You must keep this dark for a month or so yet as the discoverers will not be ready to publish before that lapse of time and more will be found some day in the same place" (Millar 1972, p. 125).

Others also received previews of the coming attraction, among them, Lewis Abbott, an amateur geologist associated with Benjamin Harrison of Ightham. Harrison's eoliths (Section 3.2) had, like Moir's Red Crag tools (Section 3.3), convinced many researchers that human fossils would be found in southern England's Pliocene and Early Pleistocene formations. After consulting Abbott about the Piltdown fossils, Dawson wrote to Woodward, "Abbott is in no doubt. They are man and man all over" (Weiner 1955, p. 100). Sir Arthur Keith also appears to have heard whispers of the Piltdown discoveries, for in a paper presented at the British Association meeting of 1912, he spoke of new fossil evidence for human beings of the modern type in Britain in the Middle Pleistocene, predating the Neanderthals of Europe (Millar 1972, p. 108).

In December 1912, Dawson and Woodward presented their formal report on the fossils they had discovered at the Piltdown site to the Geological Society of London. The report was published in the *Journal of the Society* in 1913. Concerning the geological context of the discovery, Dawson and Woodward (1913, p. 119) stated: "At Piltdown the gravel-bed occurs beneath a few inches of the

surface-soil, and varies in thickness from 3 to 5 feet; it is deposited upon an uneven bottom, consisting of hard yellow sandstone of the Tunbridge Wells Sands (Hastings Beds). . . . Portions of the bed are rather finely stratified, and the materials are usually cemented together by iron oxide, so that a pick is often needed to dislodge portions —more especially at one particular horizon near the base. It is in this last mentioned stratum that all the fossil bones and teeth discovered in situ by us have occurred." They added: "The gravel is situated on a well-defined plateau of large area, lying above the 100-foot contour line, averaging about 120 feet at Piltdown, and lies about 80 feet above the level of the main stream of the Ouse" (Dawson and Woodward 1913, p. 119).

In addition to the human fossils, the 1912 excavations at Piltdown yielded a variety of mammalian fossils. Dawson listed them as: "two small broken pieces of a molar tooth of a rather early Pliocene type of elephant, also a much-rolled cusp of a molar of Mastodon, portions of two teeth of Hippopotamus, and two molar teeth of a Pleistocene beaver." He added: "In the adjacent field to the west, on the surface close to the hedge dividing it from the gravel bed, we found portions of a red deer's antler and the tooth of a Pleistocene horse. These may have been thrown away by the workmen, or may have been turned up by a plough. . . . in the spoil heaps occurred part of a deer's metatarsal. . . . All the specimens are highly mineralized with iron oxide" (Dawson and Woodward 1913, p. 121).

Stone tools were also found: "Among the flints we found several undoubted flint implements, besides numerous 'Eoliths.' The workmanship of the former is similar to that of the Chellean or pre-Chellean stage" (Dawson and Woodward 1913, p. 122). In a footnote, Dawson stated: "Father P. Teilhard, S.J., who accompanied us on one occasion, discovered one of the implements in situ in the middle stratum of the gravel-bed, also a portion of the tooth of a Pliocene elephant from the lowest bed" (Dawson and Woodward 1913, p. 122).

The report of Dawson and Woodward (1913, p. 123) concluded: "It is clear that this stratified gravel at Piltdown is of Pleistocene age, but that it contains, in its lowest stratum, animal remains derived from some destroyed Pliocene deposit probably situated not far away, and consisting of worn and broken fragments. These were mixed with fragments of early Pleistocene mammalia in a better state of preservation, and both forms were associated with the human skull and mandible, which show no more wear and tear than they might have received in situ. Associated with these animal remains are 'Eoliths,' both in a rolled and an unrolled condition; the former are doubtless derived from an older drift and the latter in their present form are of the age of the existing deposit. In the same bed, in only a very slightly higher stratum, occurred a flint implement, the workmanship of which resembles that of implements found at Chelles; and among the spoil-heaps were found others of a similar, though perhaps earlier, stage. From these facts it appears probable that the skull and mandible cannot safely be described as being earlier than the first half of the Pleistocene Epoch. The individual probably lived during a warm cycle in that age. "

In the decades that followed, many scientists agreed with Dawson and Woodward that the Piltdown man fossils belonged to the Early Pleistocene fauna, contemporary with the Piltdown gravels. Others, such as Sir Arthur Keith and A. T. Hopwood thought the Piltdown man fossils



belonged with the older Late Pliocene (or Villafranchian) fauna that had apparently been washed into the Piltdown gravels from an older horizon (Oakley and Hoskins 1950, p. 379).

From the beginning, the Piltdown skull was deemed morphologically humanlike, although there was some disagreement about the cranial capacity. In 1913, Woodward estimated the brain capacity at 1,070 cc, perhaps more (Dawson and Woodward 1913, p. 126). This falls well below the average adult male human capacity of about 1,500 cc. But Sir Arthur Keith later proposed a reconstruction of the skull that yielded a brain capacity of 1,500 cc, matching the average adult male human capacity (Dawson and Woodward 1914, p. 98). Interestingly enough, von Koenigswald (1956, p. 179) said that Keith's reconstruction actually yielded a brain capacity of 1,370 cc, and Keith said that the original reconstruction by Dawson and Woodward was around 1,200 cc (Dawson and Woodward 1914, p. 98).

On the human appearance of the skull, Woodward stated in 1913: "A detailed examination of the several bones of the skull is interesting, as proving the typically human character of nearly all the features that they exhibit. . . . there cannot have been any prominent or thickened supraorbital ridge, and the missing region above the glabella may be restored on the plan of an ordinary modern human skull" (Dawson and Woodward 1913, p. 127).

Woodward then compared the apelike Piltdown Jaw with the Heidelberg Jaw, which is larger and heavier than the Piltdown specimen. "When it is remembered that *Eoanthropus dawsoni* and *H. heidelbergensis* are almost (if not absolutely) of the same geological age," he wrote, "we are thus led to the interesting conclusion that at the end of the Pliocene Epoch the representatives of man in Western Europe were already differentiated into widely divergent groups" (Dawson and Woodward 1913, pp. 137-138).

In addition, Woodward observed that the humanlike skull of Piltdown man was quite different from the more recent skulls of Java man and Neanderthal man, with their low foreheads and prominent brow ridges.

Woodward believed that in general the evolution of a species mirrored the growth of an individual of that species from birth to adulthood. For example, infant apes have rounded skulls, with high foreheads and almost no brow ridges, whereas adult apes have low foreheads with prominent brow ridges. Woodward therefore predicted that the skulls of adult apes from the early Tertiary, when discovered, would be much like those of modern infant apes.

"Hence," stated Woodward, "it seems reasonable to interpret the Piltdown skull as exhibiting a closer resemblance to the skulls of the truly ancestral mid-Tertiary apes than any fossil human skull hitherto found. If this view be accepted, the Piltdown type has gradually become modified into the later Mousterian type [the Neanderthals] by a series of changes similar to those passed through by the early apes as they evolved into the typical modern apes, and corresponding with the stages in the development of the skull in an existing ape-individual. It tends to support the theory that Mousterian man was a degenerate offshoot of early man, and probably became extinct; while

surviving man may have arisen directly from the primitive source of which the Piltdown skull provides the first discovered evidence" (Dawson and Woodward 1913, pp. 138-139).

Woodward had come up with his own theory about human evolution, which he thus wanted to support by fossil evidence, however limited and fragmentary. Today, a version of Woodward's proposed lineage survives in the widely accepted idea that *Homo sapiens sapiens* and *Homo sapiens neanderthalensis* are both descendants of a species called archaic or early *Homo sapiens*. Not at all widely accepted, but quite close to Woodward's idea, is Louis Leakey's proposal that both *Homo erectus* and the Neanderthals are side branches from the main line of human evolution (Section 11.4.3). But all of these proposed evolutionary lineages ignore the evidence, catalogued in this book, for the presence of anatomically modern humans in periods earlier than the Pleistocene.

## 8.2 Reactions to Piltdown Man

The notes of the discussion following the presentation made by Dawson and Woodward at the meeting of the Geological Society in December of 1912 stated: "Prof. A. Keith regarded the discovery of fossil human remains just announced as by far the most important ever made in England, and of equal, if not greater consequence than any other discovery yet made, either at home or abroad" (Dawson and Woodward 1913, p. 148).

Sir Ray Lankester, who had earlier written a favorable note to J. Reid Moir about the newly discovered Piltdown man, now expressed an opinion that the jaw and skull might not be from the same individual (Dawson and Woodward 1913, p. 148). David Waterston, professor of anatomy at King's College, also thought the jaw did not belong to the skull. He believed it had probably washed down from some older Pliocene stratum along with other mammalian fossils (Weiner

1955, p. 7; Dawson and Woodward 1913). Waterston felt that connecting the jaw with the skull was akin to linking a chimpanzee's foot with a human leg (Millar 1972, p. 140). If Waterston was correct, he was confronted with a skull that appeared to be very much like that of a human and was quite possibly from the Early Pleistocene.

So right from the start, some experts were uncomfortable with the seeming incompatibility between the humanlike skull and apelike jaw of the Piltdown man (Figure 8.1). Sir Grafton Eliot Smith, an expert in brain physiology, tried to defuse this doubt. Smith wrote in an appendix to the report by Dawson and Woodward (1913, p. 146) that the cranial cast of Piltdown man "presents more primitive features than any known human brain or cranial cast." This was quite a remarkable judgement considering the otherwise almost unanimous view that the skull itself was very much like that of a human being. Smith added: "we must consider this as being the most primitive and most simian human brain so far recorded; one, moreover, such as might reasonably have been expected to be associated in one and the same individual with the [apelike] mandible and which so definitely indicates the zoological rank of its original possessor" (Dawson and Woodward 1913, p. 147). But according to modern scientists, the Piltdown skull is a fairly recent *Homo sapiens*

sapiens skull that was planted by a hoaxer. If we accept this, that means Smith, a renowned expert, was seeing simian features where none factually existed.

Figure 8.1. Restoration of the Piltdown skull and jaw by Dawson and Woodward (1914, p. 89).

### 8.3 A Canine Tooth and Nose Bones

It was hoped that future discoveries would clarify the exact status of Piltdown man. The canine teeth, which are more pointed in the apes than in human beings, were missing from the Piltdown Jaw. Woodward thought a canine would eventually turn up, and even made a model of how a Piltdown man canine should look (Bowden 1977, p. 5).

On August 29, 1913, Teilhard de Chardin did in fact find a canine tooth in a heap of gravel from the Piltdown excavation site, near the place where the mandible had been uncovered (Dawson and Woodward 1914, p. 85). The point of the tooth was worn and flattened like that of a human canine. Woodward (Dawson and Woodward 1914, p. 87) stated: "In the upper half of the outer face the thin layer of enamel is . . . marked by the usual faint transverse striations (or Imbrications)." Such markings are characteristic of human canines. According to von Koenigswald (1956, p. 159), it was not clear whether the tooth was an upper or lower canine, but the British scientists placed it in the lower jaw discovered at Piltdown.

Some nose bones were also found. Dawson stated: "While our laborer was digging the disturbed gravel within 2 or 3 feet from the spot where the mandible was found, I saw two human nasal bones lying together with the remains of a turbinated bone beneath them in situ. The turbinal, however, was in such bad condition that it fell apart on being touched. and had to be recovered in fragments by the sieve; but it has been pieced together satisfactorily by Mrs. Smith Woodward" (Dawson and Woodward 1914, p. 85). Turbinals are thin, platelike bones with ridged surfaces: they line the nasal chambers.

Also discovered in the 1913 excavations were a tooth of *Stegodon* (an extinct elephant), an incisor and jaw fragment of a beaver, a fragment of a rhino tooth, and more flint tools (Dawson and Woodward 1914, pp. 84-85). A mastodon bone, apparently intentionally modified to form a pointed tool, was also found.

By this time, Piltdown had become quite a tourist attraction. Visiting researchers were politely allowed to assist in the ongoing excavations. Motor coaches came with members of natural history societies. Dawson even had a picnic lunch at the Piltdown site for the Geological Society of London (Millar 1972, p. 132). Dawson achieved celebrity. Indeed, the scientific name for the Piltdown hominid became *Eoanthropus dawsoni*, meaning "Dawson's dawn man." But Dawson's enjoyment of his fame was short-lived: he died in 1916.

#### 8.4 A Second Dawn Man Discovery

Doubts persisted that the Jaw and skull of *Eoanthropus* belonged to the same creature, but these doubts weakened when Woodward (1917) reported the 1915 discovery of a second set of fossils about 2 miles from the original Piltdown site.

Woodward (1917, p. 3) stated: "One large field, about 2 miles from the Piltdown pit, had especially attracted Mr. Dawson's attention, and he and I examined it several times without success during the spring and autumn of 1914. When, however, in the course of fanning, the stones had been raked off the ground and brought together into heaps, Mr. Dawson was able to search the material more satisfactorily; and early in 1915 he was so fortunate as to find here two well-fossilized pieces of human skull and a molar tooth, which he immediately recognized as belonging to at least one more individual of *Eoanthropus dawsoni*. Shortly afterwards, in the same gravel, a friend met with part of the lower molar of an indeterminable species of rhinoceros, as highly mineralized as the specimens previously found at Piltdown itself. "

Woodward (1917, p. 3) added: "The most important fragment of human skull is part of. . . a right frontal bone. . . It is in exactly the same mineralized condition as the original skull of *Eoanthropus*, and deeply stained with iron oxide." The second fragment was from the occipital, the bone of the lower rear portion of the skull.

The tooth found at what came to be called the Piltdown II site was a left lower first molar, which according to Woodward (1917, p. 5) was "stained brown with iron oxide in the usual manner. "

The report on the fossils found at the Piltdown II site included these remarks by W. P. Pycraft about the molar found there: "If the new tooth be compared with the corresponding molars of a Melanesian, a Tasmanian, and a Chimpanzee, of approximately the same size, it will readily be recognized as essentially human. In the considerable depth of the crown and its gradual passage into the root, it agrees with the human tooth and differs from that of the Chimpanzee, in which the crown is very brachyodont [broad] and overhangs the root. . . These comparisons are made because it has been stated that the molar teeth in the Piltdown mandible are those of a Chimpanzee" (Woodward 1917, p. 6).

Gerritt T. Miller, of the Smithsonian Institution, had sent Pycraft a chimpanzee Jaw with molars flattened by wear, like those in the original Piltdown Jaw. The molars of human beings are generally worn flat, while the surfaces of ape and chimp molars are usually more pointed (Figure 8.2). The flat molars of the otherwise chimpanzeelike Piltdown mandible were taken as a sign that

the mandible was not that of a chimpanzee or other member of the ape family. So by presenting a chimp Jaw with flattened molars, Miller was implying that the Piltdown mandible might still be that of an ape rather than an early human. This would mean that the Piltdown cranium and Jaw belonged to two different creatures, the former to a human and the latter to an ape.

Pycraft replied that the flat molar surfaces on the chimpanzee Jaw Miller had sent him were due "not to normal wear, but to some interference in the normal 'bite.'" Pycraft added: "In no other chimpanzee that the speaker had examined had he ever found anything in the matter of wear comparable with the molars of Mr. Miller's specimen. These are quite abnormal in this regard, and therefore of no value as evidence that the Piltdown teeth might, even in the wear of their crowns, agree with the teeth of chimpanzees" (Woodward 1917, p. 6).

In the published summary of the discussion among scientists that took place following Arthur Smith Woodward's report on the Piltdown II discoveries, it is recorded: "Prof. A. Keith said that these further Piltdown 'finds' established beyond any doubt that *Eoanthropus* was a very clearly differentiated type of being—in his opinion a truly human type" (Woodward 1917, p. 6). In the discussion, Sir Ray Lankester stated: "The present 'find' therefore makes it impossible to regard the Piltdown man as an isolated abnormal individual" (Woodward 1917, p. 6).

Figure 8.2. The crown of a human molar (middle) is generally worn flat, while the crown of a chimpanzee molar (right) generally remains pointed. In this respect, a Piltdown II molar (left) resembles a human molar (Woodward 1917, plate 1).

## 8.5 One Creature or Two?

Keith and Lankester, like Dawson and Woodward, accepted the idea that the humanlike skull and apelike Jaw belonged to the same creature, which represented an Early Pleistocene ancestor of *Homo sapiens*. After all, what were the odds of finding a human skull and an ape's jaw in such close proximity, with no sign of the matching human jaw and ape's skull? But two German scientists interpreted the Piltdown finds somewhat differently. Franz Weidenreich said the molar of the Piltdown II specimen was human, indicating Piltdown II was a fully human find. As for the original Piltdown jaw, von Koenigswald (1956, p. 179) informs us: "A pupil of Weidenreich's wanted to assign the mandible to a new ape, *Boreopithecus*. the 'Northern Ape.' Weidenreich compared it in the first place to the orang-utan, because, like the latter, it lacked certain muscle-attachments on the under margin of the Jaw." If Weidenreich's view were to have been accepted, this would have left scientists with a fully human skull and an ape Jaw, from an ape living at the same time as Piltdown man. Still, H. Weinert thought the original Piltdown jaw could easily be reconstructed as

human (Weiner et al. 1955, p. 231). It is interesting to note the widely varying interpretations by professional anthropologists.

So after years of study, debate continued about whether the Jaw and skull belonged to the same creature. Ales Hrdlicka, among other American scientists, was convinced they were from different creatures. Eager to resolve the controversy in his own mind, the famous American anthropologist Henry Fairfield Osborn, accompanied by two other scientists, journeyed to England to view the Piltdown fossils. Osborn was no stranger to controversy. Around this same time he proposed a new hominid genus on the basis of a single molar found by a geologist in western Nebraska. The ape-man was named *Hesperopithecus* (Cousins 1971, pp. 40-41). In England, Sir Grafton Elliott Smith wrote a three-page article for the *Illustrated London News* (June 24, 1922), in which there appeared a full-page picture of the brutish creature walking along carrying a club, with his wife preparing food in the background. Later, Dr. W. K. Gregory demonstrated to the satisfaction of the scientific community that the Nebraska tooth belonged to an extinct pig (Cousins 1971, p. 40). Thereafter everyone was silent about *Hesperopithecus*.

Osborn and his two companions had tended to favor Gerritt S. Miller's proposal that Piltdown man's jaw was actually that of a separate chimpanzee-like creature. In his book *Ancient Hunters*, W. J. Sollas (1924, pp. 189-190) wrote: "As a consequence, Profs. Osborn, Matthews and McGregor, who had previously been much impressed by Mr. Miller's observations, took the opportunity when they last visited Europe to make a special pilgrimage to the British Museum in order that they might see and handle the actual bones themselves of the Piltdown man, previously known to them only as represented by plaster casts. The result was eminently satisfactory, the doubts these observers had previously entertained were dissipated and they fully recognised that the jaw and skull had rightly been assigned to a single individual" (Sollas 1924, pp. 189-190).

In his book *Man Rises to Parnassus*, Osborn (1927, pp. 45-74) recalled how he had approached the British Museum feeling greatly thankful this treasure house had been spared destruction from German bombardment during the Zeppelin raids of World War 1. After spending several hours with Woodward examining the Piltdown fossils, and finally concurring that the jaw and skull belonged to the same creature, Osborn recalled the opening words of a prayer sung at Yale: "Paradoxical as it may appear O Lord, it is nevertheless true."

## 8.6 The Effect of New Discoveries On Piltdown Man

But as more hominid fossils were found, the Piltdown fossil, with its *Homo sapiens* type of cranium, introduced a great deal of uncertainty into the construction of the line of human evolution. At Choukoutien (now Zhoukoudian), near Peking (now Beijing), researchers initially uncovered a primitive-looking jaw resembling that of Piltdown man. But when the first Peking man skull was uncovered in 1929, it had the low forehead and pronounced brow ridge of *Pithecanthropus erectus* of Java, now classified with Peking man as *Homo erectus* (Millar 1972, p. 173). In the same decade, Raymond Dart uncovered the first *Australopithecus* specimens in Africa. Other *Australopithecus* finds followed, and like Java man and Peking man they also had low foreheads and prominent brow ridges. Most British anthropologists, however, decided that *Australopithecus*

was an apelike creature that was not a human ancestor. That lessened the threat to Piltdown man, who was, nevertheless, beginning to seem out of place.

In spite of the new evidence, Sir Arthur Smith Woodward remained a champion of Piltdown man until his death. Von Koenigswald (1956, p. 182) wrote: "Sir Arthur Smith Woodward was so convinced of the significance of Piltdown man that he had a small house built at Haywards Heath, not far from the site of the find, so that he could always keep an eye on it. He was a man with a strong sense of fair play, and when he felt he had been passed over on the occasion of a promotion, he left his beloved British Museum, never to set foot in it again. From now on he dedicated his whole life to Piltdown man. When we visited him at Haywards Heath in 1937 he talked of nothing else. ... In spite of the bad weather we had to go out in a taxi to the site of the discovery. Standing under a big umbrella, Sir Arthur showed us the spot at which he had unearthed the celebrated find.'

But after World War II, new finds by Robert Broom led the British to change their minds about *Australopithecus*. Sir Arthur Keith telegraphed Broom: "All my landmarks have gone, you have found what I never thought could be found: a man-like jaw associated with an ape-like skull—the exact reverse of the Piltdown evidence" (Goodman 1982, p. 94). So now what was to be done with Piltdown man, who was thought to be as old as the *Australopithecus* finds that had by then been made?

In addition to *Australopithecus*, some of the anomalous human fossils discussed previously in this book also appeared to contradict the evidence provided by Piltdown man. In his book *Meeting Prehistoric Man*, von Koenigswald (1956, pp. 179-180) addressed this problem stating: "Apart from Piltdown man there was a whole series of allegedly very ancient sapiens fossils, none of which, however, possessed such a simian jaw. The Foxhall mandible, which already has a chin, is said to have come from deep in the Red Crag on the East Anglian coast. Then there is the complete skeleton from Galley Hill near Northfleet in Kent, the finds at Denise in southern France, and various others. In the past, there was no conclusive method of determining the age of skeletal remains. Since man is in the habit of burying his dead, human bones occur in strata of differing ages. In most cases, of course, it is not difficult to ascertain whether remains are those of a modern interment or not. But there are finds that remain doubtful, and these have misled certain anthropologists into ascribing a very great geological age to *Homo sapiens* as such. This is naturally very important for the interpretation of our evolutionary history; for if we [i.e., *Homo sapiens*] really go back to the Tertiary, then all the forms of primordial man discovered up to the present are merely parallel forms, interesting in themselves but without any bearing on the history of our own stock." This conclusion becomes even stronger when we consider discoveries von Koenigswald neglected to mention, such as those made at the Castenedolo site (Section 6.2.2). Putting all the evidence on the table, we appear to be dealing with the coexistence of various fully human and ape-man-like fossils rather than evolutionary relationships in which primitive apes clearly give rise to the modern human type... Von Koenigswald (1956, p. 180) added: 'Hence Piltdown man, more than any other find, introduced an element of uncertainty into our ideas on the course of human evolution; and anyone who takes the trouble to read several books on fossil

men will see to his horror that practically every author holds a different view as to the connexions between the known human forms. It is tempting to take refuge in the theory of 'parallel evolution,' according to which *Homo sapiens* is derived direct from *Eoanthropus* ("Dawn Man") and all other early hominids are simply 'collateral forms.'" Parallel evolution, of the kind von Koenigswald described above, is exactly the position that some British anthropologists maintained for several decades.

## 8.7 Marston's Crusade

Meanwhile, an English dentist named Alvan Marston kept badgering British scientists about Piltdown man, contending that something was not quite right about the fossils. In 1935, Marston discovered a human skull at Swanscombe, accompanied by fossil bones of 26 kinds of Middle Pleistocene animals. Desiring that his discovery be hailed as "the oldest Englishman," Marston challenged the age of the Piltdown fossils Oohanson and Edey 1981, pp. 79-80).

In 1949, Marston convinced Kenneth P. Oakley of the British Museum to test both the Swanscombe and Piltdown fossils with the newly developed fluorine content method. The Swanscombe skull had the same fluorine content as the fossil animal bones found at the same site, thus confirming its Middle Pleistocene antiquity. The test results for the Piltdown specimens were more confusing.

Oakley, It should be mentioned, apparently had his own suspicions about Piltdown man. Oakley and Hoskins, coauthors of the fluorine content test report, wrote (1950, p. 379) that "the anatomical features of *Eoanthropus* (assuming the material to represent one creature) are wholly contrary to what discoveries in the Far East and in Africa have led us to expect in an early Pleistocene hominid."

Oakley tested the Piltdown fossils in order to determine whether the cranium and Jaw of Piltdown man really belonged together. The fluorine content of four of the original Piltdown cranial bones ranged from 0.1 to 0.4 percent. The Jaw yielded a fluorine content of 0.2 percent, suggesting it belonged with the skull. The bones from the second Piltdown locality gave similar results.

The fluorine content of some of the Piltdown animal bones was for the most part substantially higher, with one group (Early Pleistocene forms) ranging from

1. 9 to 3.1 percent. But another group (Middle to Late Pleistocene forms) ranged from 0.1 to 1.5 percent (Oakley and Hoskins 1950, p. 381). Oakley concluded: 'Comparison of the fluorine values of the specimens attributed to *Eoanthropus* and of the bones and teeth the geological ages of which are certain leaves little doubt that: (1) all the specimens of *Eoanthropus* including the remains of the second skull found two miles away, are contemporaneous; (2) *Eoanthropus* is, at the earliest Middle Pleistocene" (Oakley and Hoskins 1950, p. 381).

Giving a more exact estimate of the date, based in part upon his Interpretation of the geological age of the Piltdown gravels, Oakley wrote: "*Eoanthropus* may be provisionally referred to the last warm Interglacial period (Riss-Wurm interglacial); that is, early Upper Pleistocene, although here it



should be noted that some authorities count Riss-Wurm as Middle Pleistocene" (Oakley and Hoskins 1950, p. 382).

Oakley's opinion that the Piltdown man fossils belonged to the last interglacial period is consistent with the view of R. G. West (1968, p. 343), an expert on the Pleistocene geology of England, who assigned the Piltdown gravels to the Late or Middle Pleistocene. Modern authorities place the last Interglacial at 75,000 to 125,000 years ago, spanning the boundary of the Late Pleistocene and Middle Pleistocene. This is quite a bit more recent than the Early Pleistocene date originally ascribed to the Piltdown fossils, but it is still anomalously old for a skull of the fully human type in England. According to current theory, *Homo sapiens sapiens* arose in Africa about 100,000 years ago and only much later migrated to Europe, at around 30,000 years ago (Gowlett 1984, p. 118).

Oakley apparently still accepted that the skull and jaw belonged to the same creature. He suggested that 'Piltdown man, far from being a primitive type, may have been a late specialized hominid which evolved in comparative isolation" (Goodman 1983, p. 100).

This did not entirely satisfy Marston, who was convinced the Piltdown jaw and skull were from completely different creatures. From his knowledge of medicine and dentistry, Marston concluded that the skull, with its closed sutures, was that of a mature human, while the jaw, with its incompletely developed molars, was from an immature ape (Goodman 1983, p.

101). He also felt that the dark staining of the bones, taken as a sign of great antiquity, was caused by Dawson soaking them in a solution of potassium dichromate to harden them.

## 8.8 Evidence of Forgery

Marston's ongoing campaign about the Piltdown fossils eventually drew the attention of J. S. Weiner, an Oxford anthropologist. Weiner himself soon became convinced that something was wrong with the Piltdown fossils. He noticed on teeth abrasion marks that to him indicated artificial filing. As early as 1916, C. W. Lyne, a dentist, had noted that the Piltdown molars, which apparently belonged to a fairly young individual, should not be as worn down as they appeared (Goodman 1983, p. 102).

J. S. Weiner reported his suspicions to W. E. Le Gros Clark, head of the anthropology department at Oxford University, but Le Gros Clark was at first skeptical. On August 5, 1953, Weiner and Oakley met with Le Gros Clark at the British Museum, where Oakley removed the actual Piltdown specimens from a safe so they could examine the controversial relics. At this point, Weiner (1955, pp. 44-45) presented to Le Gros Clark a chimpanzee tooth he had taken from a museum collection and then filed and stained. The resemblance to the Piltdown molar was so striking that Le Gros Clark authorized a full investigation of all the Piltdown fossils (Weiner et al. 1953, pp. 141-142). One wonders if this step would have been taken if the Piltdown man fossils had fit more comfortably within the emerging hominid evolutionary progression.

In any case, a second fluorine content test, using new techniques, was applied to the Piltdown human fossils. Three pieces of the Piltdown skull now yielded a fluorine content of .1 percent. By this time, all the other fossil material from Piltdown was also suspect. Consequently, the Piltdown skull was compared with Late Pleistocene fossils from other sites in the same region, which showed a minimum fluorine content of 1 percent. But the Piltdown Jaw and teeth yielded a much lower fluorine content of .01-.04 percent. A modern chimpanzee tooth had .06 percent fluorine. Because fluorine content increases with the passing of time, the results indicated a younger age for the Jaw and teeth than the skull. The test results were reported in a paper authored by Weiner, Le Gros Clark, and Oakley, who stated: "the results leave no doubt that, whereas the Piltdown cranium may well be Upper Pleistocene as claimed in 1950 [by Oakley], the mandible, canine tooth and isolated molar are quite modern" (Weiner et al. 1953, p. 143). The conclusion that the Jaw and cranial bones were of different ages is a correct application of the fluorine content test, which, as we have seen, is best used as an indicator of the relative ages of bones found in close proximity in the same deposit. However, the attribution of a Late Pleistocene date to the skull by comparison with fossils from other sites is not quite as sound. As we have seen, the fluorine content in groundwater and its rate of absorption are subject to quite a bit of variation at different sites and over long periods of time, making relative dating estimates by this method alone subject to doubt. Thus the fluorine content test results do not completely rule out an earlier—perhaps Middle Pleistocene— date for the cranial bones discovered at Piltdown.

Regarding the two fluorine content tests by Oakley, we see that the first indicated both the skull and Jaw were of the same age whereas the second indicated they were of different ages. It was stated that the second set of tests made use of new techniques—that happened to give a desired result. This sort of thing occurs quite often in paleoanthropology—researchers run and rerun tests, or refine their methods, until an acceptable result is achieved. Then they stop. In such cases, it seems the test is calibrated against a theoretical expectation.

Nitrogen content tests were also run on the Piltdown fossils. Examining the results, Weiner found that the skull bones contained 0.6-1.4 percent nitrogen while the Jaw contained 3.9 percent and the dentine portion of some of the Piltdown teeth contained 4.2-5.1 percent. The test results therefore showed that the cranial fragments were of a different age than the Jaw and teeth, demonstrating they were from different creatures. Modern bone contains about 4-5 percent nitrogen, and the content decreases with age. So it appeared the Jaw and teeth were quite recent, while the skull was older (von Koenigswald

1956, p. 181; Weiner et al. 1953, p. 144).

By including for comparison in their report a nitrogen content of 0.7 percent for a Late Pleistocene bone from London, Weiner and his coauthors indicated the Piltdown skull fragments, with a similar nitrogen content, were probably also Late Pleistocene in age. However, as discussed in connection with the Galley Hill fossils in Chapter 6 and Appendix 1, the rate of nitrogen decay is subject to many variables. This greatly reduces the usefulness of comparing nitrogen contents of

bones from different sites. In any case, the results of the nitrogen content test still allowed one to believe that the skull, at least, was native to the Piltdown gravels.

But finally even the skull came under suspicion. Weiner and his associates wrote in a lengthy report published by the British Museum: "As the fluorine and nitrogen content of the cranial bones were consistent with their being fairly ancient [1950 report], it seemed at first that the hoax had been based on a genuine discovery of portions of a skull in the gravel. and that animal remains and implements had been subsequently 'planted' to suggest that it was Pliocene or Early Pleistocene in age. As the investigations proceeded the skull too became suspect. Dr. G. F. Claringbull carried out an X-ray crystallographic analysis of these bones and found that their main mineral constituent, hydroxy-apatite, had been partly replaced by gypsum. Studies of the chemical conditions in the Piltdown sub-soil and ground-water showed that such an unusual alteration could not have taken place naturally in the Piltdown gravel. Dr. M. H. Hey then demonstrated that when sub-fossil bones are artificially iron-stained by soaking them in strong iron sulphate solutions this alteration does occur. Thus it is now clear that the cranial bones had been artificially stained to match the gravel, and 'planted' at the site with all the other finds" (Weiner et al. 1955, p. 257).

#### 8.9 Was The Piltdown Skull Genuine?

Despite the evidence presented in the British Museum report, it can still be argued that the skull was originally from the Piltdown gravels. All of the skull pieces were darkly iron-stained throughout, while the Jaw bone, also said to be a forgery, had only a surface stain (Bowden 1977, p. 13). Furthermore, a chemical analysis of the first skull fragments discovered by Dawson showed that they had a very high iron content of 8 percent, compared to only 2-3 percent for the Jaw (Weiner et al. 1953, p. 145). This evidence suggests that the skull fragments acquired their iron-staining (penetrating the entire bone and contributing 8 percent iron to the bones' total mineral content) from a long stay in the iron-rich gravels at Piltdown. The Jaw, with simply a surface stain and much smaller iron content, appears to be of a different origin.

If the skull fragments were native to the Piltdown gravels and were not artificially stained as suggested by Weiner and his associates, then how is one to explain the gypsum (calcium sulfate) in the skull fragments? One possibility is that Dawson used sulfate compounds (along with or in addition to potassium dichromate) while chemically treating the bones to harden them after their excavation, thus converting part of the bones' hydroxy-apatite into gypsum.

Another option is that the gypsum accumulated while the skull was still in the Piltdown gravels. The British Museum scientists claimed that the concentration of sulfates at Piltdown was too low for this to have happened. But Bowden (1977, p. 15) observed that sulfates were present in the area's groundwater at 63 parts per million and that the Piltdown gravel had a sulfate content of 3.9 milligrams per 100 grams. Admitting these concentrations were not high, Bowden said they could have been considerably higher in the past. We note that Oakley appealed to higher past concentrations of fluorine in groundwater to explain an abnormally high fluorine content for the Castenedolo bones (Appendix 1).

Significantly, the Piltdown Jaw contained no gypsum. The fact that gypsum is present in all of the skull fragments but not in the Jaw is consistent with the hypothesis that the skull fragments were originally from the Piltdown gravel while the Jaw was not.

Chromium was present in the five skull fragments found by Dawson alone, before he was joined by Woodward. This can be explained by the known fact that Dawson dipped the fragments in potassium dichromate to harden them after they were excavated. The additional skull fragments found by Dawson and Woodward together did not contain any chromium.

The Jaw did have chromium, apparently resulting from an iron-staining technique involving the use of an iron compound and potassium dichromate.

To summarize, it may be that the skull was native to the Piltdown gravels and became thoroughly impregnated with iron over the course of a long period of time. During this same period of time, some of the calcium phosphate in the bone was transformed into calcium sulfate (gypsum) by the action of sulfates in the gravel and groundwater. Some of the skull fragments were later soaked by Dawson in potassium dichromate. This would account for the presence in them of chromium. The fragments found later by Dawson and Woodward together were not soaked in potassium dichromate and hence had no chromium in them. The Jaw, on the other hand, was artificially iron-stained, resulting in only a superficial coloration. The staining technique involved the use of a chromium compound, which accounts for the presence of chromium in the Jaw, but the staining technique did not produce any gypsum.

Alternatively, if one accepts that the iron-staining of the skull fragments (as well as the Jaw) was accomplished by forgery, then one has to assume that the forger used three different staining techniques: (1) According to the British Museum scientists, the primary staining technique involved the use of an iron sulfate solution with potassium dichromate as an oxidizer, yielding gypsum (calcium sulfate) as a byproduct. This would account for the presence of gypsum and chromium in the five iron-stained skull fragments first found by Dawson. (2) The four skull fragments found by Dawson and Woodward together contained gypsum but no chromium (Weiner et al. 1955, p. 269; Woodward 1948, p. 10). These must have been stained by another method. In connection with some Piltdown beavers' teeth, which also contained gypsum but no chromium, the British Museum report said: "These were presumably stained by another technique, which dispensed with the use of a dichromate solution as an oxidizer" (Weiner et al. 1955, p. 252). (3) The Jaw, which contained chromium but no gypsum, must have been stained by a method that involved use of iron and chromium compounds, but which did not produce gypsum. It is hard to see why a forger would have used so many methods when one would have sufficed. We must also wonder why the forger carelessly stained the Jaw to a far lesser extent than the skull, thus risking detection.

Additional evidence, in the form of eyewitness testimony, suggests that the skull was in fact originally from the Piltdown gravels. The eyewitness was Mabel Kenward, daughter of Robert Kenward, the owner of Barkham Manor. On February 23, 1955, the Telegraph published a letter from Miss Kenward that contained this statement: "One day when they were digging in the

unmoved gravel, one of the workmen saw what he called a coconut. He broke it with his pick, kept one piece and threw the rest away" (Vere 1959, p. 4). Particularly significant was the testimony that the gravel was unmoved.

Francis Vere amplified this information in a book published the same year that Miss Kenward wrote her letter: "the first discovery of the skull was most graphically described to me by Miss Mabel Kenward herself. She remembers seeing from her window her father, Mr. Robert Kenward, standing by the pit looking at the workmen, while they were digging in the gravel. One of them said there was something just like a coconut in the pit, and her father said that they should take care how they got it out, but before he could stop them, a blow from the pick shattered the skull and pieces flew in all directions. He picked up as many pieces as he could find and came into the house, whereupon Miss Kenward exclaimed, 'What on earth have you loaded up your pockets with all those old stones for?' He laid them out on the table and looked at them, but later returned them to the workmen, telling them to give them to Mr. Dawson next time he came. She could not say, of course, whether all the pieces were given to Dawson by the workmen. Presumably, as recounted by Miss Kenward and Woodward, the workman kept one piece which he later handed to Dawson, and threw away the rest" (Bowden 1977, p. 12).

Even Weiner himself (1955, p. 193) wrote: "we cannot easily dismiss the story of the gravel diggers and their 'coconut' as pure invention, a plausible tale put about to furnish an acceptable history for the pieces. . . . Dawson told frequently of the labourers' part (even if he did not clearly record the coconut episode) in the next few years and could hardly have had reason to fear anyone's seeking confirmation of the men. Granting, then the probability that the workmen did find a portion of skull, it is still conceivable that what they found was not the semi-fossil *Eoanthropus* but some very recent and quite ordinary burial." Weiner suggested that the culprit, whoever he may have been, could have then substituted treated skull pieces for the ones actually found. But if the workmen were dealing with "a very recent and quite ordinary burial" then where were the rest of the bones of the corpse? At least some harder bones, like the femurs, should have remained.

In the end, Weiner suggested that an entire fake skull was planted, and the workmen broke it. But Mabel Kenward testified that the surface where the workman started digging was unbroken. The spot was also on the manor grounds, quite near the house, it appears from the testimony, and the gravel was so compact and cemented that it took a pick to break into it. It would not have been easy, it seems, for some unknown person to enter onto private property in England and excavate a deep hole with a pick by the drive near a manor house, anytime of day or night, without being questioned. In any case, if we accept the plant hypothesis in this instance, then practically any discovery of human fossil remains anywhere could also be said to have been a plant and forgery.

What about the altered animal fossils allegedly planted at Piltdown? Those could have been introduced without arousing suspicion, because after the skull's discovery the investigators would often be searching through the gravel, which was already broken up.

Robert Essex, a scientist personally acquainted with Dawson in the years

1912 to 1915, provided interesting testimony about the Piltdown Jaw, or Jaws, as it turns out. Essex wrote in 1955: "Another Jaw not mentioned by Dr. Weiner came from Piltdown much more human than the ape's jaw, and therefore much more likely to belong to the Piltdown skull parts which are admittedly human. I saw and handled that Jaw and know in whose bag it came to Dawson's office. The Jaw was also seen by Mr. H. H. Wakefield, then an articled clerk of Dawson's, and he has given written evidence of seeing it. Dawson never saw it, and the owner himself probably never knew until 1953 that anybody but himself had seen it" (Bowden 1977, p. 37).

Essex then gave more details. At the time, he had been science master at a local grammar school, located near Dawson's office. Essex stated: "One day when I was passing I was beckoned in by one of the clerks whom I knew well. He had called me in to show me a fossil half-Jaw much more human than an ape's and with three molars firmly fixed in it. When I asked where this object came from, the answer was 'Piltdown.' According to the clerk, it had been brought down by one of the 'diggers' who, when he called and asked for Mr. Dawson, was carrying a bag such as might be used for carrying tools. When he was told that Mr. Dawson was busy in court he said he would leave the bag and come back. When he had gone, the clerk opened the bag and saw this Jaw. Seeing me passing he had called me in. I told him he had better put it back and that Mr. Dawson would be cross if he knew. I found afterwards that when the 'digger' returned, Mr. Dawson was still busy in court, so he picked up his bag and left" (Bowden 1977, p. 37). Essex later saw photographs of the Piltdown Jaw. Noting the Jaw was not the same one he had seen in Dawson's office, he communicated this information to the British Museum.

These reports are significant for the following reason. It is unlikely that a forger would have planted a human Jaw at Piltdown, along with everything else. So the story about the discovery of a human Jaw tends to confirm the view that the human skull found at Piltdown was native to the gravels. Even if we grant that every other bone connected with Piltdown is a forgery, if the skull was found in situ, we are confronted with what could be one more case of *Homo sapiens sapiens* remains from the late Middle Pleistocene or early Late Pleistocene.

#### 8.10 The Identity of The Forger

Most recent writing, totally accepting that all the Piltdown fossils and implements were fraudulent, has focused on identifying the culprit. Weiner and Oakley insinuated that Dawson, the amateur paleontologist, was to blame. Woodward, the professional scientist, was absolved.

But Dawson's honor was defended by, of all people, Alvan Marston. At a meeting of the Geological Society of London, held on November 25, 1953, Weiner and Oakley made a slide presentation detailing the evidence that led them to conclude the Piltdown Jaw had been forged from a modern orangutan Jaw. Marston, who strongly objected to any suggestion that Dawson was guilty of forgery, showed his own slides proving that the jaw, though that of an ape, was a genuine fossil with no sign of deliberate staining or filing. Marston apparently believed that although Dawson may have been mistaken about the age of the Jaw, and in connecting it with the human cranium

found with it, he was not a forger. Marston accused the British Museum of making Dawson a scapegoat. Dawson could not fight back, but he, Marston, could. Marston shouted, "Let them try and tackle me!" A British author wrote: "The hubbub at this meeting was wrongly reported in the United States to have developed into a series of fist fights" (Millar 1972, pp. 218-219).

Von Koenigswald, like Weiner and Oakley, blamed Dawson: "It is certainly not nice to accuse a dead man who cannot defend himself: but everything quite clearly points to his responsibility for the forgery. Indeed, it has now turned out that neither the fossils nor the tools belong to this locality at all, and that the whole find was carefully planted" (Wendt 1972, p. 154). But as we have seen, the evidence is not so clear cut in this regard. Indeed, it is difficult to see how anyone could say that anything "quite clearly points" to anything in regard to the Piltdown controversy.

For example, if Dawson were involved in chemical forgery, why did he immediately send the five pieces of skull originally found at Piltdown to a public chemist for analysis? Furthermore, Dawson openly admitted he had treated the fragments with potassium dichromate to harden them, and it was well known that he was performing some experiments on staining techniques in his offices (Bowden 1977, p. 26). If Dawson had really been involved in some deliberate fraudulent staining and planting of the original skull pieces and all the other fossils found at Piltdown, it seems he would have been more careful.

Furthermore, it would appear that the Piltdown forgery (even excluding the skull) demanded extensive technical knowledge and capability—beyond that seemingly possessed by Dawson, an amateur anthropologist. Gavin De Beer, a director of the British Museum of Natural History, wrote in a foreword to a report by Weiner, Le Gros Clark, and Oakley: "We are now in a position to give an account of the full extent of the Piltdown hoax. The mandible has been shown by . . . anatomical and X-ray evidence to be almost certainly that of an immature orang-utan; that it is entirely recent has been confirmed by a number of microchemical tests, as well as by the electron-microscope demonstration of organic (collagen) fibers; the black coating on the canine tooth, originally assumed to be an iron encrustation, is a paint (probably Vandyke brown); the so-called turbin bone is shown by its texture not to be a turbin bone at all, but thin fragments of probably non-human limb bone; all the associated flint implements have been artificially iron-stained; the bone implement was shaped by a steel knife; the whole of the associated fauna must have been 'planted,' and it is concluded from radioactivity tests and fluorine analysis that some of the specimens are of foreign origin" (Weiner et al. 1955, p. 228). It appears that a professional scientist, who had access to rare fossils and knew how to select them and modify them to give the impression of a genuine faunal assemblage of the proper age, had to be involved in the Piltdown episode.

Some have tried to make a case against Teilhard de Chardin, who had studied at a Jesuit college near Piltdown and who had become acquainted with Dawson as early as 1909. A *Stegodon* tooth found at Piltdown was believed by Weiner and his associates to have come from a North African site that might have been visited by Teilhard de Chardin in the period from 1906 to 1908, during which time he was a lecturer at Cairo University (Millar 1972, p. 232).

Woodward is another suspect. Over the course of several decades, he tightly controlled access to the original Piltdown fossils, which were stored under his care in the British Museum. This could be interpreted as an attempt to prevent evidence of forgery from being noticed by other scientists. It is interesting, for example, that Woodward originally reported the following facts about the Piltdown site: "Portions of the bed are rather finely stratified, and the materials are usually cemented together by iron oxide, so that a pick is often needed to dislodge portions—more especially at one particular horizon near the base. It is in this last mentioned stratum that all the fossil bones and teeth discovered in situ by us have occurred" (Dawson and Woodward 1913, p. 119). If all the Piltdown fossils are fakes, then why did Woodward not notice something was wrong? There are three possible reasons: (1) the planting was done in an incredibly clever fashion, so as to exactly reproduce the fine stratification and the hard, unbroken consistency of the bed in question; (2) Woodward, supposedly an expert, missed obvious signs of planting; (3) Woodward was involved in the planting. It is also suspicious that Woodward so tightly controlled access to the original Piltdown specimens, compelling all but a select few researchers to examine only casts.

Ronald Millar, author of *The Piltdown Men*, suspected Grafton Eliot Smith. Having a dislike for Woodward, Smith may have decided to entrap him with an elegant deception. Smith, like Teilhard de Chardin, had spent time in Egypt, and so had access to fossils that could have been planted at Piltdown (Oohanson and Edey 1981, pp. 81- 82).

Frank Spencer, a professor of anthropology at Queens College of the City University of New York, has written a book that blames Sir Arthur Keith, conservator of the Hunterian Museum of the Royal College of Surgeons, for the Piltdown forgery (Wilford 1990). Keith believed that modern humans evolved earlier than other scientists could accept, and this, according to Spencer, impelled him to conspire with Dawson to plant evidence favoring his hypothesis.

Another suspect was William Sollas, a professor of geology at Cambridge. He was named in a tape-recorded message left by English geologist James Douglas, who died in 1979 at age 93. Sollas disliked Woodward, who had criticized a method developed by Sollas for making plaster casts of fossils. Douglas recalled he had sent mastodon teeth like those found at Piltdown to Sollas from Bolivia and that Sollas had also received some potassium dichromate, the chemical apparently used in staining many of the Piltdown specimens. According to Douglas, Sollas had also "borrowed" some ape teeth from the Oxford museum collection (Oohanson and Edey 1981, p. 83). According to this view, Sollas secretly enjoyed seeing Woodward duped by the Piltdown forgeries.

But if Piltdown does represent a forgery, it is likely that something more than personal revenge was involved. Spencer said that the evidence "had been tailored to withstand scientific scrutiny and thereby promote a particular interpretation of the human fossil record" (Wilford 1990).

Possible motivations for forgery by a professional scientist may be sensed when we consider the inadequacies of the evidence for human evolution that had accumulated by the beginning of the twentieth century. Darwin had published *The Origin of Species* in 1859, setting off almost immediately a search for fossil evidence connecting *Homo sapiens* with the ancient Miocene apes. Leaving aside the discoveries suggesting the presence of fully modern humans in the Pliocene and



Miocene, Java man and the Heidelberg jaw were the only fossil discoveries that science had come up with. And as we have seen in Chapter 7. Java man in particular did not enjoy unanimous support within the scientific community.

Right from the start there were ominous suggestions that the apelike skull did not really belong with the humanlike thighbone found 45 feet away from it. As we have seen in this chapter, a number of scientists in England and America. such as Arthur Smith Woodward, Grafton Eliot Smith. and Sir Arthur Keith were developing alternative views of human evolution in which the formation of a high-browed humanlike cranium preceded the formation of a humanlike jaw. Java man, however, showed a low-browed cranium like that of an ape.

Since so many modern scientists have indulged in speculation about the identity and motives of the presumed Piltdown forger, we would also like to introduce a tentative hypothesis. Consider the following scenario. Workmen at Barkham Manor actually discovered a genuine Middle Pleistocene skull, in the manner described by Mabel Kenward. Pieces of it were given to Dawson. Dawson, who had regularly been communicating with Woodward, notified him. Woodward, who had been developing his own theory of human evolution and who was very worried about science's lack of evidence for human evolution after 50 years of research, planned and implemented the forgery. He did not act alone, but in concert with a select number of scientists at the British Museum, who assisted in acquiring the specimens and preparing them so as to withstand the investigations of scientists not in on the secret.

Oakley. who played a big role in the Piltdown expose. himself wrote (Boule and Vallois 1957, p. 3): 'The Trinil [Java man] material was tantalizingly incomplete, and for many scientists it was inadequate as confirmation of Darwin's view of human evolution. I have sometimes wondered whether it was a misguided impatience for the discovery of a more acceptable 'missing link' that formed one of the tangled skein of motives behind the Piltdown Forgery (1912).'

Weiner also admitted the possibility: "Behind it all we sense, therefore, a strong and impelling motive. . . The planning . . must betoken a motive more driving than a mere hoax or prank. . . There could have been a mad desire to assist the doctrine of human evolution by furnishing the requisite 'missing link.' . . . Piltdown might have offered irresistible attraction to some fanatical biologist to make good what Nature had created but omitted to preserve" (Weiner 1955, pp. 117-118).

Unfortunately for the hypothetical conspirators. the discoveries that turned up over the next few decades did not support the evolutionary theory represented by the Piltdown forgery. The discoveries of new specimens of Java man and Peking man. as well as the Australopithecus finds in Africa, were accepted by many scientists as proving the low-browed ape-man ancestor hypothesis, the very idea the high-browed Piltdown man was meant to discredit and replace.

Time passed, and the difficulties in constructing a viable evolutionary lineage for the fossil hominids increased. At a critical moment. the remaining insiders in the British Museum chose to

act. Perhaps enlisting unwitting colleagues. they organized a systematic expose of the forgery the Museum had perpetrated earlier in the century. In the course of this expose, perhaps some of the specimens were further modified by chemical and physical means to lend credence to the idea of forgery.

The idea of a group of conspirators operating out of the British Museum, perpetrating a forgery and then later exposing the same is bound to strike many as farfetched. But it is founded upon as much, or as little, evidence as the indictments made by others. Doubt has been cast on so many British scientists individually, including some from the British Museum, that this conspiracy theory does not really enlarge the circle of possible wrongdoers.

Perhaps there were no conspirators at the British Museum. But according to many scientists, someone with scientific training, acting alone or with others, did carry out a very successful forgery.

Gavin De Beer, a director of the British Museum of Natural History, believed the methods employed in uncovering the Piltdown hoax would "make a successful repetition of a similar type of forgery virtually impossible in the future" (Weiner et al. 1955, p. 228). But a forger with knowledge of modern chemical and radiometric dating methods could manufacture a fake that would not be easily detectable. Indeed, we can hardly be certain that there is not another Piltdownlike forgery in one of the world's great museums Just waiting to be uncovered.

The impact of Piltdown remains, therefore, damaging. But incidents of this sort appear to be rare, given our present knowledge. There is, however, another more insidious and pervasive kind of cheating—the routine editing and reclassifying of data according to rigid theoretical preconceptions.

Vayson de Pradenne, of the Ecole d'Anthropologie in Paris, wrote in his book *Braudes Archeologiques* (1925): "one often finds men of science possessed by a pre-conceived idea, who, without committing real frauds, do not hesitate to give observed facts a twist in the direction which agrees with their theories. A man may imagine, for example, that the law of progress in pre-historic industries must show itself everywhere and always in the smallest details. Seeing the simultaneous presence in a deposit of carefully finished artefacts and others of a coarser type, he decides that there must be two levels: the lower one yielding the coarser specimens. He will class his finds according to their type, not according to the stratum in which he found them. If at the base he finds a finely worked Implement he will declare there has been accidental penetration and that the specimen must be re-integrated with the site of its origin by placing it with the items from the higher levels. He will end with real trickery in the stratigraphic presentation of his specimens: trickery in aid of a pre-conceived Idea, but more or less unconsciously done by a man of good faith whom no one would call fraudulent. The case is often seen, and if I mention no names it is not because I do not know any" (Vere 1959, pp. \-2).

This sort of thing goes on not Just in the British Museum, but in all museums, universities, and other centers of paleoanthropological research the world over. Although each separate incident of

knowledge filtration seems minor, the cumulative effect is overwhelming, serving to radically distort and obscure our picture of human origins and antiquity.

An abundance of facts suggests that beings quite like ourselves have been around as far back as we care to look—in the Pliocene, Miocene, Oligocene, Eocene, and beyond. Remains of apes and apelike men are also found throughout the same expanse of time. So perhaps all kinds of hominids have coexisted throughout history. If one considers all the available evidence, that is the clearest picture that emerges. It is only by eliminating a great quantity of evidence—keeping only the fossils and artifacts that conform to preconceived notions—that one can construct an evolutionary sequence. Such unwarranted elimination of evidence, evidence as solidly researched as anything now accepted, represents a kind of deception carried out by scientists desiring to maintain a certain theoretical point of view. This deception is apparently not the result of an deliberately organized plot, as with the Piltdown man forgery (if that is what Piltdown man was). It is instead the inevitable outcome of social processes of knowledge filtration operating within the scientific community.

But although there may be a lot of unconscious fraud in paleoanthropology, the case of Piltdown demonstrates that the field also has instances of deception of the most deliberate and calculating sort.

#### Peking Man and Other Finds in China

After the discoveries of Java man and Piltdown man, ideas about human evolution remained unsettled. Dubois's *Pithecanthropus erectus* fossils did not win complete acceptance among the scientific community, and Piltdown simply complicated the matter. Scientists waited eagerly for the next important discoveries—which they hoped would clarify the evolutionary development of the Hominidae. Many thought the desired hominid fossils would be found in China.

Eventually, such fossils did turn up, at Choukoutien, near Peking. The creature to which the bones originally belonged was designated Peking man or *Sinanthropus*. The Peking man fossils were lost to science during World War II, but more fossil discoveries were made in the postwar era. In this chapter, we will discuss the controversial nature of the Peking man fossils and the questionable practice of dating later Chinese hominid fossils by their morphology, in the absence of more secure means of determining their actual age.

In the course of this discussion, the reader will be confronted with various spellings of names of Chinese geographical locations and scientists. Over the years, scholars have adopted different conventions for rendering Chinese names into English. For example, Peking is now spelled Beijing. And Choukoutien is now spelled Zhoukoudian. In the first part of the chapter, we use Peking and Choukoutien, and in the later part of the chapter we use Beijing and Zhoukoudian. As far as names of scientists are concerned, what Westerners regard as the last name comes first in the Chinese name. For example, Wu Rukang will be listed in our bibliography as Wu, R. Complicating the matter are variant spellings, such as Woo Jukang. In our text and citations, we will use the modern

spellings of most authors. In the bibliography, we will also give the modern spelling with the older variant in brackets: Wu, R. [Woo, J.].

### 9.1 Discoveries at Choukoutien

The ancient Chinese called fossils dragon bones. Believing dragon bones to possess curative powers, Chinese druggists have for centuries powdered them for use in remedies and potions. For early Western paleontologists, Chinese drug shops therefore provided an unexpected hunting ground.

In 1900, Dr. K. A. Haberer collected mammalian fossils from Chinese druggists and sent them to the University of Munich, where they were studied and catalogued by Max Schlosser in *The Fossil Mammals of China* (1903). Among the specimens, Schlosser found a tooth from the Peking area that appeared to be a "left upper third molar, either of a man or hitherto unknown anthropoid ape" (Goodman 1983, p. 63). Schlosser suggested China would be a good place to search for primitive man.

Among those who agreed with Schlosser was Gunnar Andersson, a Swedish geologist employed by the Geological Survey of China. Andersson, a keen hunter of dragon bones, traced out their sources from druggists and other informants. He then carried out excavations for fossils. Andersson was particularly interested in discoveries that might increase the evidence for the theory of human evolution. In his paleoanthropological research, Andersson enjoyed the support of the Swedish government, members of the Swedish royal family, and wealthy patrons such as Ivar Kreuger, who monopolized match- manufacturing in several countries.

In 1918, Andersson visited a place called Chikushan, or Chicken Bone Hill, near the village of Choukoutien, 25 miles southwest of Peking. There, on the working face of an old limestone quarry, he saw a fissure of red clay filled with fossil bones.

In 1921, Andersson again visited the Chikushan site. He was accompanied by Otto Zdansky, an Austrian paleontologist who had been sent to assist him, and Walter M. Granger, of the American Museum of Natural History. Their first excavations were not very productive, resulting only in the discovery of some fairly recent fossils.

Then some of the local villagers told Zdansky about a nearby place with bigger dragon bones, near the small Choukoutien railway station. Here Zdansky found another limestone quarry, the walls of which, like the first, had fissures filled with red clay and broken bones. Andersson visited the site and discovered some broken pieces of quartz, which he thought might be very primitive tools. The mineral quartz did not occur naturally at the site, so Andersson reasoned that the quartz pieces must have been brought there by a hominid. Zdansky, who did not get along very well with Andersson, disagreed with this interpretation.

Andersson, however, remained convinced. Looking at the limestone wall, he said, "I have a feeling that there lies here the remains of one of our ancestors and it's only a question of finding him"

(Hood 1964, p. 65). He asked Zdansky to keep searching a filled-in cave, saying. 'Take your time and stick to it until the cave is emptied if need be" (Goodman 1983, p. 65).

#### 9.1.1 The First Teeth

In 1921 and 1923, Zdansky, somewhat reluctantly, conducted brief excavations. He uncovered signs of an early human precursor—first one tooth and then a second, tentatively dated to the Early Pleistocene. Of the first tooth Zdansky said: "I recognized it at once, but said nothing" (Goodman 1983, p. 65).

Even after finding the second tooth, Zdansky kept both secret. The teeth, a lower premolar and an upper molar, were crated up with other fossils and shipped to Sweden for further study (Hood 1964, p. 66). Back in Sweden, Zdansky published a paper in 1923 on his work in China, with no mention of the teeth.

There the matter rested until 1926. In that year, the Crown Prince of Sweden, who was chairman of the Swedish China Research Committee and a patron of paleontological research, planned to visit Peking. Professor Wiman of the University of Uppsala, asked Zdansky, his former student, if he had come across anything interesting that could be presented to the Prince. Zdansky sent Wiman a report, with photographs, on the teeth he had found at Choukoutien. The report, later published in the Bulletin of the Geological Survey of China, was duly presented by J. Gunnar Andersson to a meeting in Peking, attended by the Crown Prince. Andersson declared in regard to the teeth: "The man I predicted had been found" (von Koenigswald 1956, p. 41).

#### 9.1.2 Davidson Black

Another person who thought Zdansky's teeth represented clear evidence of fossil man was Davidson Black, a young Canadian physician residing in Peking.

Davidson Black graduated from the University of Toronto medical school in 1906. To satisfy his strong interest in anatomy, he took a post at Western Reserve University in Ohio, where he worked with T. Wingate Todd, a noted English anatomist.

Todd was an associate of Grafton Elliot Smith, familiar to us from our discussion of Piltdown man (Chapter 8). A forceful advocate of human evolutionary theory, Todd organized at Western Reserve University an extensive skeletal museum, including casts of bones from all known forms of fossil man. Under Todd, Davidson Black therefore had an opportunity to become acquainted with the latest developments in the field of paleoanthropology.

In 1914, Black went to Manchester, England, to work under Grafton Elliot Smith, who was then occupied with Piltdown man. Black also developed a friendly relationship with Sir Arthur Keith, accompanying him to the Piltdown site.

In a letter of recommendation, Smith wrote of Black: "during his stay in my department he has seized every opportunity of familiarizing himself with the problem of human phylogeny [evolution]" (Hood 1964, p. 27).

After returning to Western Reserve, Black read *Climax and Evolution* by William Diller Matthew. In 1911, Matthew had said in an address to the National Academy of Sciences of the United States: "All authorities are today agreed in placing the center of dispersal of the human race in Asia. Its more exact location may be differently interpreted, but the consensus of modern opinion would place it probably in or about the great plateau of central Asia" (Osborn 1928, p. 192).

Today the center of dispersal is viewed as Africa rather than central Asia, and all fossil evidence must therefore be interpreted in light of an African origin. For example, most paleoanthropologists now believe that *Homo sapiens sapiens* evolved in southern Africa about 100,000 years ago, and then spread throughout the world, diversifying into the present races. But other scientists concerned with human origins, such as Carleton S. Coon (1969), have said the fossil evidence shows that the several modern human races evolved separately from *Homo erectus* in Africa, Europe, and Asia. However, as we have several times noted, it is only by excluding or reinterpreting vast quantities of reported evidence that any evolutionary hypothesis whatsoever can be maintained.

From the time he first became acquainted with Matthew's ideas in 1915, Black intended to go to northern China to search for the center of human origins (Hood 1964, p. 35). But the First World War delayed his plans.

#### 9.1.3 The Rockefeller Foundation Sends Black to China

In 1917, Black joined the Canadian military medical corps. Meanwhile, a friend of Black, Dr. E. V. Cowdry, was named head of the anatomy department at the Rockefeller Foundation's Peking Union Medical College. Cowdry asked Dr. Simon Flexner, director of the Rockefeller Foundation, to appoint Black as his assistant. After meeting Flexner in New York, Black was accepted and wrote to a colleague: "In addition to my work at the school I shall have the privilege of accompanying such scientific expeditions as may be organized to explore and collect material in central China, Tibet, etc." (Hood 1964, pp. 41-42).

After Rockefeller Foundation officials petitioned the Surgeon General of Canada, Black won his release from the Canadian military and proceeded to Peking, arriving in 1919.

At the Peking Union Medical College, Black did everything possible to minimize his medical duties so he could concentrate on his real interest—paleoanthropology. In November 1921, he went on a brief expedition to a site in northern China, and other expeditions followed. Black's superiors were not pleased,

In 1921, Dr. R. M. Pearce, the Rockefeller Foundation's advisor on medical education, visited Peking on an inspection tour. Afterward, Pearce wrote to Black: "If you think of anatomy for nine months out of the year, it is no one's business what you do with the other three months in the

summer in connection with anthropology, but for the next two years at least give your entire attention to anatomy" (Hood 1964, p, 55),

But gradually the Rockefeller Foundation would be won over to Black's point of view, The series of events that caused this change to take place is worth looking into,

Late in 1922, Black submitted a plan for a Siam (now Thailand) expedition to Dr. Henry S, Houghton, director of the medical school Black expertly related his passion for paleoanthropology to the mission of the medical school Houghton wrote to Roger Greene, the school's business director: "While I cannot be certain that the project which Black has in mind is severely practical in its nature, I must confess that I have been deeply impressed by ... the valuable relationship he has been able to establish between our department of anatomy and the various institutions and expeditions which are doing important work in China in the fields which touch closely upon anthropology research, With these points in mind I recommend the granting of his request" (Hood 1964, p, 56), Here can be seen the importance of the intellectual prestige factor—ordinary medicine seems quite pedestrian in comparison with the quasi-religious quest for the secret of human origins, a quest that had, since Darwin's time, fired the imaginations of scientists all over the world, Houghton was clearly influenced, The expedition took place during Black's summer vacation in 1923, but unfortunately produced no results,

In 1924, Black took a year's paid leave to travel around the world, visiting early man sites, museums, and scholars in the field of human evolution, Black returned to Peking determined to give more time to his pet research projects,

#### 9.1.4 Black and the Birth of Sinanthropus

In 1926, Black attended the scientific meeting at which J. Gunnar Andersson presented to the Crown Prince of Sweden the report on the molars found by Zdansky at Choukoutien in 1923, Excited on learning of the teeth, Black accepted a proposal by Andersson for further excavations at Choukoutien, to be earned out jointly by the Geological Survey of China and Black's department at the Peking Union Medical School Dr. Amadeus Grabau of the Geological Survey of China called the hominid for which they would search "Peking man,"

On October 27, 1926, Black wrote to Sir Arthur Keith about Zdansky's teeth: "There is great news to tell you—actual fossil remains of a man-like being have at last been found in Eastern Asia, in fact quite close to Peking, This discovery fits in exactly with the hypothesis as to the Central Asiatic origin of the Hominidae which I reviewed in my paper 'Asia and the Dispersal of Primates'" (Hood 1964, p, 84), Black in China, like Dubois in Java, had found what he was looking for,

Hood (1964, p, 85) stated in her biography: "Black's next task was to approach the Rockefeller Foundation through Roger Greene to ask for funds with which to make a large-scale excavation at the caves of Chou-K'ou-tien, To his delight and relief a generous sum was forthcoming, This response showed a marked change in the attitude of the authorities in New York towards Black's efforts to promote research into China's prehistory from his experience in 1921."

By spring 1927, work was underway at Choukoutien, in the midst of the Chinese civil war. During several months of painstaking excavation, there were no discoveries of any hominid remains. Finally, with the cold autumn rains beginning to fall, marking the end of the first season's digging, a single hominid tooth was uncovered. On the basis of this tooth, and the two previously reported by Zdansky (now in Black's possession), Black decided to announce the discovery of a new kind of fossil hominid. He wrote in *Nature*: "The newly discovered specimen displays in the details of its morphology a number of interesting and unique characters, sufficient, it is believed, to justify the proposal of a new hominid genus *Sinanthropus*, to be represented by this material" (Black 1927, p. 954),

Black was eager to show the world his discovery. Dr. Heinrich Neckles, a friend of Black, later recalled: "One night he came to my office very excited, to show the precious tooth of *homo pekinensis*. He wanted me to advise him about the safest method to take the invaluable find to England (where he was going shortly) safe against loss or theft. I suggested a brass capsule with a screw closure and a ring at the top, with a strong ribbon through it, so he could wear it around his neck. We had a good Chinese mechanic in the Physiology Department who made a very nice capsule for him and he was as happy as a little boy" (Hood 1964, p. 90),

In the course of his travels with his newly found tooth, Black discovered that not everyone shared his enthusiasm for *Sinanthropus*. At the annual meeting of the American Association of Anatomists in 1928, some of the members heavily criticized Black for proposing a new genus on so little evidence.

In addition, Zdansky was not at all very happy regarding the purposes for which his teeth were being used: "I am indeed convinced that the existing material provides a wholly inadequate foundation for many of the various theories based upon it. ... I decline absolutely to venture any far-reaching conclusions regarding the extremely meager material described here, and which, I think, cannot be more closely identified than as *Homo* sp. [species undetermined] . . . my purpose here is only to make it clear that my discovery of these teeth should be regarded as decidedly interesting but not of epoch-making importance" (Bowden 1911, pp. 80- 81).

Regarding such criticism of Black's activities, Grafton Elliot Smith wrote: "It had no other effect upon him, beyond awaking his sympathies for anthropologists who are unfairly criticized and to make him redouble his efforts to establish the proof of his claim" (Hood 1964, p. 93).

Black kept making the rounds, showing the tooth to Ales Hrdlicka in the United States and then journeying to England, where he met Sir Arthur Keith and Sir Arthur Smith Woodward. At the British Museum, Black had casts made of the Peking man molars, for distribution to other workers. This is the kind of propaganda work necessary to bring a discovery to the attention of the scientific community. This serves to illustrate that even for a scientist political skills are not unimportant.

On returning to China, Black kept in close touch with the excavations at Choukoutien. Dynamite was used to blast out sections of rock. Crews of workers then searched through the debris, sending the larger chunks back to Peking, where any fossils were carefully extracted. The sole aim



of the whole project was, of course, to find more Peking man remains. For months nothing turned up.

But Black wrote to Keith on December 5, 1928: "It would seem that there is a certain magic about the last few days of the season's work for again two days before it ended Bohlin found the right half of the lower jaw of *Sinanthropus* with the three permanent molars in situ" (Hood 1964, p. 97).

Now a financial problem loomed. The Rockefeller Foundation grant that supported the digging would run out in April of 1929. So in January, Black wrote the directors, asking them to support the Choukoutien excavations by creating a Cenozoic Research Laboratory (the Cenozoic includes the periods from the Paleocene to the Holocene). In April, Black received the funds he desired.

#### 9.1.5 The Transformation of the Rockefeller Foundation

Just a few years before, Rockefeller Foundation officials had actively discouraged Black from becoming too involved in paleoanthropological research. Now they were backing him to the hilt, setting up an institute specifically devoted to searching for remains of fossil human ancestors. Why had the Rockefeller Foundation so changed its attitude toward Black and his work? This question bears looking into, because the financial contribution of foundations would turn out to be vital to human evolution research carried out by scientists like Black. Foundation support would also prove important in broadcasting the news of the finds and their significance to the waiting world.

As Warren Weaver, a scientist and Rockefeller Foundation official, said (1967, p. 82): "In a perfect world an idea could be born, nourished, developed and made known to everyone, criticized and perfected, and put to good use without the crude fact of financial support ever entering into the process.

Seldom, if ever, in the practical world in which we live, does this occur. The influence of money on ideas can be powerful; it can be good, or it can be downright vicious. . . . Money can be used to lure the gullible to devote their time to spiritualism, to fanatical religions, to pseudo-science, and so on."

For Weaver, biological questions were of the highest importance. Writing in

1967, Weaver stated that he regarded the highly publicized particle accelerators and space exploration programs as something akin to scientific fads. He added: "The opportunities not yet rigorously explored lie in the understanding of the nature of living things. It seemed clear in 1932, when the Rockefeller Foundation launched its quarter-century program in that area, that the biological and medical sciences were ready for a friendly invasion by the physical sciences. . . . the tools are now available for discovering, on the most disciplined and precise level of molecular actions, how man's central nervous system really operates, how he thinks, learns, remembers, and forgets. . . . Apart from the fascination of gaining some knowledge of the nature of the mind-brain-body relationship, the practical values in such studies are potentially enormous. Only thus may we

gain information about our behavior of the sort that can lead to wise and beneficial control” ON. Weaver 1967. p. 203).

It thus becomes clear that at the same time the Rockefeller Foundation was channeling funds into human evolution research in China, it was in the process of developing an elaborate plan to fund biological research with a view to developing methods to effectively control human behavior. Black's research into Peking man must be seen within this context in order to be properly understood.

Over the past few decades, science has developed a comprehensive cosmology that explains the origin of human beings as the culmination of a 4-billion-year process of chemical and biological evolution on this planet, which formed in the aftermath of the Big Bang, the event that marked the beginning of the universe some 16 billion years ago. The Big Bang theory of the origin of the universe, founded upon particle physics and astronomical observations suggesting we live in an expanding cosmos, is thus inextricably connected with the theory of the biochemical evolution of all life forms, including human beings. The major foundations, especially the Rockefeller Foundation, provided key funding for the initial research supporting this materialistic cosmology, which has for all practical purposes pushed God and the soul into the realm of mythology—at least in the intellectual centers of modern civilization.

The extent of the Rockefeller Foundation's support of biological research is remarkable. The Foundation funded the fruit fly genetics work of Thomas Hunt Morgan and Theodosius Dobzhansky. Dr. Max Perutz said the Cambridge Medical Research Council Laboratory of Molecular Biology in England owed its existence to the Rockefeller Foundation. The Foundation furnished funds for the Laboratory's X-ray diffraction equipment, which provided critical research results used by Watson and Crick in their pioneering work on DNA's helical structure (W. Weaver 1967, p. 235).

The Foundation was equally supportive of selected projects in the realm of the physical sciences. Lee A. DuBridge, President of the California Institute of Technology, wrote: “The sciences of physics and astronomy could hardly have emerged from the primitive state in which they found themselves in America in the first two decades of the twentieth century had it not been for the generosity of the great private foundations” ON. Weaver 1967, p. 252). As we have seen, the Carnegie Foundation built the Mt. Wilson Observatory. The Rockefeller Foundation built the Mt. Palomar Observatory, where much of the work on the Big Bang theory of the origin of the universe took place. The Foundation also gave funds to Ernest O. Lawrence for building the world's first particle accelerators.

If the Big Bang and biochemical evolution represent the Godless and soulless cosmology of the scientific world view, psychiatry and psychology represent its secular moral code and guidelines for practical behavior. In the early 1930s, around the time the Choukoutien excavations were in full swing, the medical division of the Rockefeller Foundation chose psychiatry as its principal focus, establishing schools of psychiatry at major medical colleges. Later the Foundation would fund the famous Kinsey reports on sexual behavior.

During the 1930s, psychiatry was fairly well dominated by the figure of Sigmund Freud, who had encountered ideas about human evolution as a youth and later wrote: "The theories of Darwin, which were then of topical interest, strongly attracted me, for they held out hopes of an extraordinary advance in our understanding of the world" (Oones 1953, pp. 27-28).

In *Totem and Tabu*, Freud explained Christianity and all organized religion in terms of his Oedipus complex. According to one of his biographers, Freud "took into account, too, the work of Charles Darwin. He recalled Darwin's conjecture that originally men had lived in hordes, each horde dominated by a single, powerful, violent, suspicious man" (Puner 1947, p. 167). In his autobiography, Freud wrote: "The father of the primal horde, since he was an unlimited despot, had seized all the women for himself; his sons, being dangerous to him as rivals, had been killed or driven away. One day, however, the sons came together and united to overwhelm, kill and devour their father, who had been their enemy but also their ideal. The primal father, at once feared and hated, honored and envied, became the prototype of God himself. . . . This view of religion throws a particularly clear light upon the psychological basis of Christianity" (Puner 1947, pp. 167-168). If one takes seriously the theory of evolution, one must explain the origin of God and religion as an historical occurrence within the mind of evolving man, though perhaps not in the exact manner suggested by Freud.

The Rockefeller Foundation saw in psychiatry a way to influence human social behavior. Dr. Alan Gregg, head of the Medical Sciences Division of the Foundation, wrote: "I should not be satisfied with the definition of psychiatry as that specialty in medicine which deals with mental disorders." He believed its "province is the conduct of man, his reactions, his behavior as an indivisible sentient being with other such beings" (Fosdick 1952, p. 130). During the Second World War, Gregg served as an Army consultant and wrote of "the possibility that through psychiatric understanding our successors may be able to govern human politics and relationships more sagely" (Fosdick 1952, p. 133). The desire to bring about better human relations is certainly laudable. But our main point is that the Rockefeller Foundation scientists believed this goal could best be achieved by having science establish beneficial control over human society.

All this is quite remarkable, when one considers that John D. Rockefeller's charity was initially directed toward Baptist churches and missions. Raymond D. Fosdick, an early president of the Rockefeller Foundation, said (1952, p. 2) that both

Rockefeller and his chief financial adviser, Frederick T. Gates, were "inspired by deep religious conviction." Rockefeller believed "a man should make all he can and give all he can" (Fosdick 1952, p. 6).

According to Fosdick (1952, p. 6), Rockefeller was at first "giving to a multiplicity of small causes mostly related to his church interests—schools, hospitals, and missions." As a result, he was continually being approached by Baptist ministers. To relieve Rockefeller from personally having to handle individual requests, Gates organized a system whereby Rockefeller would give a lump sum to a mission board that would distribute the funds in an appropriate fashion.

Moving on to bigger things. Rockefeller and Gates gave 35 million dollars for building the University of Chicago. which, according to Fosdick (1952, p. 7). started out as "as an idea for a Baptist institution of higher learning. under Baptist auspices and control." It is hard to imagine such a school promoting the idea that humans evolved from extinct apelike creatures. Gates, it may be noted. was formerly head of the American Baptist Education Society.

In 1913, the present Rockefeller Foundation was organized. The trustees included Frederick T. Gates; John D. Rockefeller, Jr.; Dr. Simon Flexner, head of the Rockefeller Institute for Medical Research; Henry Pratt Judson. president of the University of Chicago; Charles William Eliot, former president of Harvard; and A. Barton Hepburn. president of the Chase National Bank.

At first, the Foundation concentrated its attention on public health, medicine, agriculture, and education, avoiding anything controversial. Thus the Rockefeller Foundation began to distance itself from religion, particularly the Baptist Church. Exactly why this happened is difficult to say. Perhaps it had something to do with the fact that Rockefeller was coming to realize that his fortune was founded on exploiting the advances of modern science and technology. Perhaps it was the increasing role that science was beginning to play in the objects of traditional charitable giving—such as medicine. But whatever the reason, Rockefeller began to staff his foundation with scientists, and the giving policies reflected this change.

Even Gates. the former Baptist educator. seemed to be changing his tune. He wanted to create a nonsectarian university in China. But he noted that the "missionary bodies at home and abroad were distinctly and openly, even threateningly hostile to it as tending to infidelity" (Fosdick 1952. p. 81). Furthermore, the Chinese government wanted control, an idea that the Foundation could not support.

President Eliot, who had overseen the Harvard Medical School in Shanghai, proposed a solution: a medical college, which would serve as an opening to the rest of Western science. Fosdick (1952, p. 81) wrote: "To President Eliot there was no better subject than medicine to introduce to China the inductive method of reasoning which lies at the basis of all modern science. He thought it would be the most significant contribution that the West could make to the East." Here mechanistic science shows itself a quiet but militant ideology, skillfully, yet somewhat ruthlessly, promoted by the combined effort of scientists, educators, and wealthy industrialists, with a view towards establishing worldwide intellectual dominance.

The medical hospital strategy outlined by Eliot worked. The Chinese government approved establishment of the Peking Union Medical College under Foundation auspices. Meanwhile. Dr. Wallace Buttrick, director of Rockefeller's newly created China Medical Board. negotiated with the Protestant mission hospitals already in China. He agreed to provide financial support for these hospitals. in effect bribing them (Fosdick 1952. pp. 83-84).

In 1928. the Rockefeller Foundation and other Rockefeller charities underwent changes to reflect the growing importance of scientific research. In 1923, Wycliffe Rose, head of the General Education Board. had said: "All important fields of activity, from the breeding of bees to the

administration of an empire, call for an understanding of the spirit and technique of modern science.

. Science is the method of knowledge. It is the key to such dominion as man may ever exercise over his physical environment. Appreciation of its spirit and technique, moreover, determines the mental attitude of a people, affects the entire system of education, and carries with it the shaping of a civilization" (Fosdick 1952, p. 141).

All programs in various Rockefeller charities "relating to the advance of human knowledge" were shifted to the Rockefeller Foundation, which was organized into five divisions: international health, medical sciences, natural sciences, social sciences, and the humanities (Fosdick 1952, pp. 137-138). Each division was run by a highly competent academic and technical staff, who advised the trustees of the Foundation where to give their money. Raymond D. Fosdick, president of the Foundation at the time, said (1952, p. 140) that the year of 1928 marked "the end of an era in philanthropy." And the beginning of a new one.

The change reached right to the top, with Dr. Max Mason, a scientist himself, taking over as president. Mason, a mathematical physicist, was formerly president of the University of Chicago. According to Fosdick (1952, p. 142), Mason "emphasized the structural unity involved in the new orientation of program. It was not to be five programs, each represented by a division of the Foundation; it was to be essentially one program, directed to the general problem of human behavior, with the aim of control through understanding."

The Foundation also saw itself engaged in a kind of thought control. Fosdick (1952, p. 143) said: 'The possession of funds carries with it power to establish trends and styles of intellectual endeavor. '

The theme of control was echoed in 1933 by Warren Weaver, who headed the Rockefeller Foundation's natural sciences division, which funded the Cenozoic Research Laboratory in Peking. In a report to the trustees, Weaver, a mathematician from the University of Wisconsin, said: "The welfare of mankind depends in a vital way on man's understanding of himself and his physical environment. Science has made magnificent progress in the analysis and control of inanimate forces, but it has not made equal advances in the more delicate, more difficult, and more important problem of the analysis and control of animate forces" (Fosdick 1952, p. 157). The Rockefeller Foundation's annual report for the year 1933 (p. 199) asked: "Can we develop so sound and extensive a genetics that we can hope to breed in the future superior men? ... In short, can we rationalize human behavior and create a new science of man?"

The Foundation scientists outlined a coordinated program, approved by the Foundation trustees, to attain this goal. Fosdick (1952, p. 158) stated: "the trustees, in the spring of 1933, voted to make experimental biology the field of primary interest. ... It was conceived, moreover, as being closely linked with other aspects of the Foundation's program, notably the program in psychiatry of the Medical Sciences division and the social-science program in human relations. Biology is important because it has the potentiality of contributing to the problem of understanding

ourselves, and the three programs—in widely separated fields— could be thought of as a unified endeavor to stimulate research in the sciences underlying the behavior of man.”

Some commentators make light of research into the reproductive habits of earthworms and other apparently obscure research projects. But these have their purpose. According to Weaver: "Before we can be wise about so complex a subject as the behavior of a man, we obviously have to gain a tremendous amount of information and insight about living organisms in general, necessarily starting with the simpler forms of life. Experimental biology is the means for such exploration. It furnishes the basis necessary for progress in solving the sequence of problems which begins with the strictly biological and moves through the mental to the social" (Fosdick 1952, p. 158). Here once more, the intent to use science for perfecting methods of social control (and who would the controllers be but the scientists?) is stated explicitly.

And what about something as apparently innocent as stargazing through the 200-inch telescope at Mt. Palomar? Fosdick (1952, p. 179) stated: "Superficially the 200-inch and the lesser projects in astronomy which have received Foundation aid would seem to be far removed from the main interest of the Natural Sciences program. What possible relationship can there be between the stars and experimental biology? Fosdick (1952, p. 180) answered that astronomy gives the first glimmers of regularity in nature, the understanding of which will lead to control of humanity and the universe.

It bears repeating that one should see Black's Peking man research within the larger framework of the explicitly stated goal of the Rockefeller Foundation, which reflected the implicit goal of big science—control, by scientists, of human behavior. In particular, Peking man strengthened the concept of human evolution, by which scientists attempt to determine the way we think about ourselves. Essentially, evolution defines human nature in a totally materialistic way. This materialistic definition of human nature tends to justify making the primary goal of human life the attainment of control, by science, over the visible universe.

#### 9.1.6 An Historic Find and a Cold-Blooded Campaign

With the financial backing of the Rockefeller Foundation for the Cenozoic Research Laboratory secure, Black resumed his travels for the purpose of promoting Peking man. In May of 1929, he arrived in Java, for the Fourth Pacific Science Congress. There he was able to give a report on *Sinanthropus* before an audience that included Grafton Eliot Smith. Black stated: "Elliot Smith's cordial backing after my presentation of the material at the conference made all the difference in the world to its reception there" (Hood 1964, pp. 100 -101). Nevertheless, Peking man still had not achieved the worldwide celebrity he would later enjoy. While in Java, Smith and Black visited the Trinil site, where Dubois had originally discovered *Pithecanthropus*, the southern relative of *Sinanthropus*.

Black then returned to China, where work was proceeding slowly at Choukoutien, with no new major *Sinanthropus* finds reported. Enthusiasm seemed to be waning among the workers. But then on the first of December, at the very end of the season, W. C. Pei (Pei Wenzhong) made an

historic find. Pei later wrote: "At about four o'clock next afternoon I encountered the almost complete skull of *Sinanthropus*. The specimen was imbedded partly in loose sands and partly in a hard matrix so that it was possible to extricate it with relative ease" (Hood 1964, p. 104).

In order to protect the skull, Pei immediately wrapped it in paper and cloth soaked with flour paste. He then rode 25 miles on a bicycle to the Cenozoic Research Laboratory, where he presented the skull to Black, who gave him full credit for the discovery.

By early 1930, Black had published two preliminary papers on the skull and set about publicizing the find around the world. His secretary, Miss Hempel, recalled: "For weeks and months we did nothing but write letters" (Hood 1964, p. 109).

Black wrote to Dr. Pearce at the Rockefeller Foundation: "Yes, *Sinanthropus* is growing like a bally weed. I never realized how great an advertising medium primitive man (or woman) was till this skull turned up. Now everybody is crowding around to gaze that can get the least excuse to do so and it gets embarrassing at times. Being front page stuff is a new sensation and encourages a guarded manner of speech" (Hood 1964, pp. 110-111).

Black worked busily, carefully freeing the skull from its stone matrix and later making a cast of it. Copies of the cast were sent to museums all over the world. The site itself was purchased by the Geological Survey of China.

In September of 1930, Sir Grafton Elliot Smith arrived in Peking to inspect the site of the discovery and examine the fossils. During Smith's stay, Black primed him for a propaganda blitz on behalf of Peking man. Smith then departed, and apparently did his job well. In December, Black wrote an extremely candid letter to Dr. Henry Houghton, director of the Peking medical school, who was vacationing in America: "I am thrilled beyond words to know how much you enjoyed Grafton Elliot Smith. ... he is Irish to the extent that a friend is always spoken of in lurid hyperbole and, though I love him for it, I get the collywobbles when I reflect the brazen way I have plotted to have him exercise his talent in this respect on my behalf. . . I warned him to hold off'n me ... but your letter makes it clear that that balloon is busted and I'm the chappee who must spend the rest of his days trying to live up to and live down the reputation acquired by his own rash act." This rash act appears to have been the *Sinanthropus* discovery.

Black went on to say: "But you, too, are dripping with the gore of the same hegoat and I love you, for your soul is white if your hood be scarlet and your aid, comfort and participation in the plot from its inception made success possible and doubly enjoyable. . . . You must admit that we have not been any blushing roses when it came to turning our wolf loose (if you don't mind mixed metaphors)—if I blushed every time I thought of the cold-blooded advertising campaign I thought of and G. E. S. has carried through, I'd be permanently purple" (Hood 1964, p. 115).

Cold-blooded advertising campaign? That is not the way most people think scientific discoveries normally make their way into academic acceptance and public notice. Black is to be commended for his forthright statements. In any case, having turned the wolf of *Sinanthropus* loose on the

world, he received many honors, including appointments as honorary fellow of the Royal Anthropological Institute and honorary member of America's National Academy of Sciences (Hood 1964, p. 116). Black was later elected a fellow of the Royal Society, Britain's foremost assembly of scientists.

His newly won fame also insured continued access to Rockefeller Foundation funds. Black wrote to Sir Arthur Keith: "We had a cable from Elliot Smith yesterday so he is evidently safe home after his strenuous trip. He characteristically has not spared himself in serving the interests of the Survey and the Cenozoic Laboratory and after his popularizing *Sinanthropus* for us in America I should have a relatively easy task before me a year from now when I will have to ask for more money from the powers that be" (Hood 1964, p. 116).

Peking man had come at just the right moment for advocates of human evolution. A few years previously, in one of the most famous trials in the world's history, a Tennessee court had found John T. Scopes guilty of teaching evolution in violation of state law. Scientists wanted to fight back hard. Thus any new evidence bearing on the question of human evolution was highly welcome.

Then there had been the matter of *Hesperopithecus*, a highly publicized prehistoric ape-man constructed in the minds of paleoanthropologists from a single humanlike tooth found in Nebraska. To the embarrassment of the scientists who had promoted this human ancestor, the humanlike tooth had turned out to be that of a fossil pig.

Meanwhile, the lingering doubts and continuing controversy about Dubois's *Pithecanthropus erectus* also needed to be resolved. In short, scientists in favor of evolutionary ideas, reacting to external threat and internal disarray, were in need of a good discovery to rally their cause.

Concerning the Java fossils, Jia Lanpo wrote: "The problem of what species did the owner of the remains belong to had not been settled. Sceptics asserted that they might belong to a deformed ape, or an abnormally developed animal which had no relation to man whatsoever. The most vociferous critics were from the religious community, who held that man's ancestor was Adam, and that man's history dates back only 4,004 years before Christ. Anyone who held that those specimens were related to man was accused of being a heretic. In the end, because of the pressure or some other reason, Dubois himself gave in and stated that what he had discovered was the remains of a 'giant gibbon.' It was not until 1929 after Professor Pei Wenzhong discovered the Peking Man skullcap and later, stone artifacts and traces of the use of fire in association with it that the absurd clamor gradually died down" (Ula 1980, p. 27).

Peking man caught on like wildfire, not only in the world of science, which needed him, but among the general public as well. Therefore scientists went overboard to confirm his status as a genuine human ancestor. And as suggested by Jia, "stone artifacts and traces of the use of fire" were to be an important element in this confirmation.

#### 9.1.7 Evidence for Fire and Stone Tools at Choukoutien



It was In 1931 that reports showing extensive use of fire and the presence of well-developed stone and bone tools at Choukoutien were first published. What is quite unusual about these announcements is that systematic excavations had been conducted at Choukoutien by competent investigators since 1927, with no mention of either fire or stone tools. For example, Black wrote in 1929 (p. 208): "though thousands of cubic meters of material from this deposit have been examined, no artifacts of any nature have yet been encountered nor has any trace of the usage of fire been observed. "

On the question of tools, P. Teilhard de Chardin and C. C. Young (Yang Zhongjian) wrote: "Embedded in the fine grained material of the Lower Cave, Mr. Pel picked up an angular piece of quartz—a type of stone which is not found within one mile at least of the locality. Similar quartz fragments have been found from time to time in the course of the excavations, the first ones being noticed by Dr. J. G. Andersson, but none of them has ever shown any recognizable trace of artificial breaking" (1929, p. 182). In addition, Teilhard de Chardin (1965, pp. 62-63) wrote in an article published in 1930: "since the beginning of the excavation no trace has yet been found on the site suggesting the use of fire or any industry of any kind."

Grafton Eliot Smith, who had personally visited the Choukoutien site, wrote (1931, p. 36): "It is a very significant phenomenon that at Chou Kou Tien, in spite of the most careful search in the caves during the last three years, no trace whatever of implements of any sort has been found. . . . It must not be forgotten, however, that Dr. Andersson in 1921 found pieces of quartz in association with the fossil bones, and that in the later stages of the excavation Mr. Pel found further examples of this alien material. Those who have been searching in vain for evidence of human craftsmanship on this site are being forced to the conclusion that Peking Man was in such an early phase of development as not yet to have begun to shape implements of stone for the ordinary needs of his daily life."

Then Teilhard de Chardin, while visiting Paris in 1930, showed a piece of stag horn from Choukoutien to Henri Breuil, without telling Breuil its source. Breuil studied the specimen and noted that it showed signs of having been deliberately burned by fire. He also concluded it had been modified by hammering for use as a tool, and noted cut marks that appeared to have been made by a stone implement (Breuil 1932, pp. 1-2). At that point, Teilhard de Chardin revealed the source of the bone and suggested Breuil visit Choukoutien. There are mysterious undercurrents here. At this point in time, Teilhard de Chardin was on record as saying there were no signs of human industry at Choukoutien. But then why was he carrying around a rather ordinary piece of deer horn? And why did he show it to Breuil? It does not make much sense, unless we assume that Teilhard de Chardin himself had suspicions that the bone showed signs of intentional work.

While still in Paris, Teilhard de Chardin presented, at the Institute of Human Paleontology, a paper that was published the following year in *L'Anthropologie*. In this paper, Teilhard de Chardin (1931) cautiously suggested that the use of fire by *Sinanthropus* might be established after further study of blackened bones and antlers recovered from the site. Nevertheless, he still made no mention of any beds of ashes or hearths at the Choukoutien cave.

As requested by Teilhard de Chardin, Breuil visited Choukoutien In the fall of 1931 and encountered extensive signs of fire as well as stone and bone tools, many of advanced type. He reported his findings on November 3 at a meeting of the Geological Society of China in Peking, and published essentially the same material In an article for L 'Anthropologie the following year.

Teilhard de Chardin himself had also begun to cautiously mention fire and implements in his writings, but Breuil's exceedingly direct and thorough report was explosive in its impact. Concerning the Quartz 2 level in the Kotzetang cave section, Breuil (1932, p. 3) said: "I observed the black layer indicated by Pei was a veritable hearth. or rather a hearth covered by very light-colored ash, doubtlessly mixed with clay. Soot-covered stones and burned bones were both brought out in my presence. " Breuil here used the French word foyer, which can be variously translated as hearth, fireplace, or furnace.

Describing level 4 of the main cave. Breuil (1932, p 5) stated: "I first observed, in scraping away the surface from top to bottom, a succession of a number of ribbonlike layers of bright colors—grey, yellow. and occasionally violet, which constitute level 4. The appearance of this uncompacted deposit is exactly that of a mass of ash derived from vegetal matter, comparable. for example, to the ash deposit, called 'ribbons' by E. Piette, In the Azilian levels of Mas d’Azil. In these masses one can observe numbers of particles of carbon, stones. occasionally in heaps, covered in soot, and fragments of burned bone. I did not encounter anything else, but I did observe between layers of ash numerous nodules and slabs of a bubbly concretion that appeared to be composed of phosphates derived from the alteration of bone. At the base of the great mass of ash, almost 7 meters [about 23 feet] in depth, one finds an ink-black layer, which. according to analysis by the Geological Survey and its color, shows itself to be composed of wood carbon reduced to particles. Worked quartz and other stones belong to this layer."

Breuil (1932, p. 5) further stated: 'The layer of breccia covered by the mass of cinders is actually inaccessible, but I have examined a great heap of blocks that have been removed and broken apart for examination. These blocks are literally pastes of chipped quartz (horizon 1) and bone burned to various degrees."

In some final remarks on the evidence for fire, Breuil (1932, pp. 6 -7) said: "As a result of these facts, confirmed by chemical analysis of the burned bones in Paris and Peking, It can be concluded that fire was used on a large scale at Chou Kou Tien. Perhaps the fact that such a mass of ash corresponds with a single black. carbonaceous basal level could enable one to deduce that the fire, ignited Just once, was constantly maintained for a considerable period, enough to have produced the enormous accumulation of almost 7 meters that I have mentioned. This amount would actually correspond to a much greater accumulation at the time."

About the presence of stone implements, Breuil (1932, p. 7) wrote: "I might add that I collected some chipped quartz in the great mass of ash at that place. .. at the base of that mass. lying on the stalagmitic floor ... I extracted, along with M. Pel and Pierre Licent, a series of decomposed pieces of very compact volcanic rock. . . . It was the residue, unfortunately very much decomposed, of a

great collection of tools made from large flakes of volcanic rock. I do not know if they are found in other levels. ”

Breuil (1932) also recorded the presence of many other stone tools, including some rounded bola stones. He reported that in some features the stone tool industry was similar to that of the Mousterian period in Europe, although he mentioned that it would be pointless to attempt to fit the Choukoutien stone tool industry exactly into the European classifications. The Mousterian period is identified with the Neanderthals.

Black, along with Teilhard de Chardin, Pei, and Yang (Young), stated in similar fashion: “In a very broad sense, the Choukoutien culture could be defined as an industry of old palaeolithic type, showing some external Mousterian analogies. But no close comparison with any Asiatic or European industry can well be made at present” (Black et al. 1933. p. 133).

Later investigators added considerably to the collection of stone tools from Choukoutien. To date, over 100,000 have been found, including a variety of choppers, scrapers, and small pointed flakes. According to Jia Lanpo, the most common material is quartz, followed by sandstone and opal. Jia (1980, p. 28) described a ‘mastery of rather complex methods of manufacture.’ He further stated: ‘The assemblage consists mainly of small tools but there are also larger ones, such as bifacial handaxes. . . . Scrapers of various types made on flakes are the most numerous. The blade after secondary working of the edge may be linear, convex, concave, multi-edged or disk-like. . . . The finest of the lot are the ‘points.’ About a hundred of them have been collected . . . their process of manufacture clearly indicates a higher level of skill. To make one, a flake is first struck from a core, then the edges are shaped until a slender point is achieved at one end. Up to now, nowhere in the world has yielded such finds of comparable quantity and workmanship” (Jia 1980, pp. 28-29).

Jia’s description suggests a relatively advanced industry at Choukoutien, but other researchers have expressed differing opinions about the quality of the stone tools found there. David Pilbeam (1972, p. 166) quoted Kenneth Oakley as saying that the stone tools were similar to the crude Oldowan tools from Africa. Paleoanthropologists have highlighted different features of the stone industry at Choukoutien—hence one may get a completely different impression depending upon whose account one reads.

As far as bone tools were concerned, Breuil noted that the ancient population at Choukoutien had systematically employed a sizable industry. Large antlers, too big to be effectively used in one piece, had been cut down into manageable tool shapes. Since deer antlers are extremely difficult to cut, the place where an incision was to be made was first burned with fire, then a V-shaped groove was gouged out, and finally the bone was broken by a blow.

In 1931, Black, apparently embarrassed by the new revelations about fire and tools from Choukoutien, sought to explain how such important evidence had for several years escaped his attention and that of the other researchers at the site. In a report delivered at the same time as that of Breuil, Black (1931, p. 107) tried to cover himself on the critical question of fire: “From time to time since 1929 occasional specimens of apparently charred or partly calcined animal bones

have been recovered from among the material excavated from the Main Deposit at Choukoutien. The physical appearance of these specimens left little room for doubt that they had been subjected at some time to the action of fire. But until the present season it has remained a question whether or not such specimens had been burned within the Choukoutien caves while the latter were occupied by *Sinanthropus* or were altered simply as the result of a surface fire from natural causes and had subsequently been washed within the deposit. In view of this uncertainty no report on these specimens has hitherto been published."

This seems unusual, especially when considered in the light of the following statement, published in 1933, by Black: "Traces of artificial fire in the Locality 1 deposit are so clear and abundant that they require only to be mentioned without any further demonstration" (Black et al 1933, p. 113). If this was true in 1933, why not in 1931, or 1929, or even earlier? And even if, as Black said in his 1931 report, signs of fire had been noted but not reported because of doubts about the origin of the fire, this does not absolve him of responsibility. The burned bones could at least have been mentioned, and the alternative explanations discussed.

Teilhard de Chardin also thought it wise to explain why he had not reported the presence of stone or bone implements at the time of his discussions with Breuil in Paris. In 1934, he stated in the *Journal Revue des Questions Scientifiques* (vol. 25): "In writing my first article here on Choukoutien three years ago, I was still able to say that 'up to now', despite certain indications, no trace of industry had yet been certainly recognised in association with the bone remains of *Sinanthropus*. Two months later, returning to the site with Mr. W. C. Pei [Pei Wenzhong], the young scholar in charge of the excavation, I gathered with him in situ incontestable fragments of flaked stone and burnt bones. These traces had hitherto escaped attention because the works have been carried on for some years in a part of the site where they would have been extremely hard to recognise. . . . But once we recognised the first flakes of quartz, all became clear. . . . From that moment, archaeological discoveries multiplied—the most important being the discovery (Summer 1931) of a red, yellow, and black clay bed about two metres [about six and a half feet] thick, extremely rich in stone and bone debris" (Teilhard de Chardin

1965, pp. 70-71). Again, it does seem quite unusual that such experienced researchers as Teilhard de Chardin and Pei could have completely overlooked the presence of literally thousands of implements at Choukoutien.

In reference to the question of fire, Teilhard de Chardin and C. C. Young (Yang Zhongjian) wrote in 1929 about Layer 4 in the Choukoutien cave deposits: "Very conspicuous fine grained, sedimentary zone, formed by red loam and sandy clay of various colors (yellow, reddish, brown, gray, etc.) thinly bedded and interbedded. At several levels some black layers occur which are full of Rodent remains and other micro fauna. . . Thickness 6.7 meters [22 feet]" (Teilhard de Chardin and Yang 1929, p. 181). A few years later, in 1932, this same layer would be described by Teilhard de Chardin and Pei in the *Bulletin of the Geological Society of China* (vol. 11) as "an ashy deposit" almost 7 meters deep (Bowden 1977, p. 92). The main ash piles were 300 feet long by 100 feet

wide (Fix 1984, p. 118). It is quite remarkable that Teilhard and Young (Yang) could have examined this same formation in 1929 and reported on it with no suggestion at all of fire.

Concerning the failure of Teilhard de Chardin, Black, Pei, and others to report abundant tools and signs of fire at Choukoutlen, there are two possible explanations. The first is the one they themselves gave—they simply overlooked the evidence or had so many doubts about it that they did not feel justified in reporting it. The second possibility is that they were very much aware of the signs of fire and stone tools, before Breuil reported them, but deliberately withheld this information.

But why? At the time the discoveries were made at Choukoutlen, fire and stone tools at a site were generally taken as the work of *Homo sapiens* or Neanderthals. According to Dubois and von Koenigswald, no stone tools or signs of usage of fire were found in connection with *Pithecanthropus erectus* in Java. The Selenka expedition did report remnants of hearths at Trinil, but this information did not attain wide circulation.

So perhaps the original investigators of Choukoutlen purposefully held back from reporting stone tools and fire because they were aware such things might have confused the status of *Sinanthropus*. Doubters might have very well attributed the fire and tools to a being contemporary with, yet physically and culturally more advanced than *Sinanthropus*, thus removing *Sinanthropus* from his position as a new and important human ancestor.

As we shall see, that is what did happen once the tools and signs of fire became widely known. For example, Breuil (1932, p. 14) said about the relationship of *Sinanthropus* to the tools and signs of fire: "Several distinguished scientists have independently expressed to me the thought that a being so physically removed from Man. . . . was not capable of the works I have just described. In this case, the skeletal remains of *Sinanthropus* could be considered as simple hunting trophies, attributable, as were the traces of fire and industry, to a true Man, whose remains have not yet been found." But Breuil himself thought that *Sinanthropus* was the manufacturer of tools and maker of fire at Choukoutien.

#### 9.1.8 Recent Views

Modern investigators have tended to confirm Breuil's views. Like Breuil, they hold that certain deposits in the Choukoutlen caves are deep layers of ash, indicating the massive use of fire. For example, Wu Rukang and Lin Shenglong (1983, p. 93) reported that there are four "large thick layers of ashes" and that the thickest layer is six meters [about 19.7 feet] thick in certain places.

Paleontologist Jia Lanpo stated (1975, pp. 33-36) that there are four thick layers of ash and that the layer in the upper- middle part of the cave is six meters deep and consists of beds of ash of different colors—purple, red, yellow, white, and black. He also reported burned bones, colored black, blue, white, grey, green, or dull brown. Jia believed that Peking man knew how to use but not make fire, that the fires once lit were kept burning continuously for a long time—even passed down from generation to generation. We are not, however, aware of the discovery anywhere else

in the world of a cave as old as Choukoutien having such huge beds of ash, providing, of course, that the above reports, identifying the deposits as ash layers, are correct.

Father P. O'Connell, a Roman Catholic priest who lived in China during the period of the Peking man discoveries, offered an intriguing explanation for the massive ash deposits at Choukoutien. He suggested that the site had been used for producing lime for the construction of the ancient city of Cambalac, situated on land now occupied by present-day Beijing (O'Connell 1969). Lime, a caustic substance produced by heating limestone to a high temperature, is used in making mortar and plaster.

But almost all modern investigators agree with Breuil that *Sinanthropus* was responsible for the signs of fire. Breuil wrote in the 1930s: "*Sinanthropus* kindled fire and did so frequently, he used bone implements and he worked stone, Just as much as the Paleolithics of the West. In spite of his skull, which so closely resembles that of *Pithecanthropus*, he was not merely a Hominian, but possessed an ingenious mind capable of inventing, and hands that were sufficiently adroit and sufficiently master of their fingers to fashion tools and weapons" (Boule and Vallois 1957, p. 144). One gets an impression of a fairly humanlike being, a hunter who brought game felled with his stone weapons back to his cave home, where he cooked the flesh on fires he kindled for that purpose.

A somewhat different view of *Sinanthropus* at Choukoutien is provided by Lewis R. Binford and Chuan Kun Ho, anthropologists at the University of New Mexico. Concerning the signs of fire, they stated: "The so-called ash layers are not hearths and may not all be ash layers. ... There seems to be little doubt that much of the content of the so-called ash layers is largely owl or other raptor droppings. They are systematically described as dominated by rodent bone. ... It would appear that at least some of them were originally huge guano accumulations inside the cave. In some cases, these massive organic deposits could have burned. ... The assumption that man introduced and distributed the fire is unwarranted, as is the assumption that burned bones and other materials are there by virtue of man's cooking his meals" (Binford and Ho 1985, p. 429).

Binford and Ho's theory that the ash deposits are composed mostly of bird droppings has not received unanimous support. But their assertions about the unreliability of the common picture of Peking man drawn from the presence of bones, ashes, and hominid remains at the site are worthy of serious consideration.

For Binford and Ho, the presence of hominid bones in the caves was not a demonstration that Peking man ever permanently lived there. They gave the following information about the Peking man fossils: "It is not uncommon to find hominid remains in direct association with hyena coprolites and adjacent to cave walls, where larger bones tend to end up in animal dens. Smaller hominid bones, such as isolated teeth, already broken cranial fragments, and mandible parts, are more common in contexts that appear to represent areas near the entrances of the cave. The picture one obtains is one in which hominid carcasses or parts thereof were introduced to the active, entrance area of the cave. It is unclear whether hominids died there or parts of hominid carcasses were brought there by scavenging animals. The extreme bias in body parts [mostly skulls

and lower limb bones] would favor the latter interpretation. These parts were then further dispersed within the cave, most likely by bone-carrying animals such as hyena or wolf ” (Binford and Ho 1985, p. 428).

The presence of stone tools at Choukoutien is generally taken as confirmation of a picture of *Sinanthropus* as a hunter sitting around his hearth cutting up deer carcasses. But Binford and Ho felt that the kind of tools found at Choukoutien, mostly primitive scrapers and choppers, were not very well adapted to hunting. Furthermore, they pointed out “Layers that yield hominid remains only rarely produce stone tools and almost never are they designated as ash layers. In addition, excavations conducted in areas that would have been deep in the interior of the cave, beyond the limits of natural light, may yield hominid remains but only rarely yield tools in any concentration” (Binford and Ho 1985, p. 428). In other words, there is no clear connection between the stone tools and the hominid remains.

The most that can be said of Peking man, if we confine ourselves to the actual evidence at the site, is that he was perhaps a scavenger who may or maynot have used primitive stone tools to cut meat from carcasses left by carnivores in a large cave where organic materials sometimes bumed for long periods. Or perhaps Peking man was himself prey to the cave's carnivores, for it seems unlikely he would have voluntarily entered such a cave, even to scavenge.

Binford and Ho did not believe there existed a bone tool industry at Choukoutien. They said Breuil's recognition of a bone tool industry was founded on “modifications that today we routinely recognize as the by-product of animal gnawing” (Binford and Ho 1985, p. 428).

In making thisJudgement, Binford and Ho were in agreement with the original assessment made by the members of the Cenozoic Research Laboratory. Black, Teilhard de Chardin, Young (Yang), and Pei (1933, p. 130) believed that the recurring types of broken bones, which Breuil said had been shaped intentionally, may have been shaped by purely accidental forces. They believed, however, that further research would be required before this issue could be definitely resolved.

But a modern authority, Jia Lanpo of the Republic of China's Institute for Vertebrate Paleontology and Paleoanthropology, reported, like Breuil, numerous tools shaped from deer bones Oia 1975, p. 31). Jia believed deer antler roots may have served as hammers. Antler tines showing criss-crossed scratches may have been used for digging, and deer skullcaps may have been used as drinking bowls. "Antlers are hard to hack off," stated Jia (1980, p. 29), “but if a spot is first scorched, the cutting is much easier, and this was what Peking man did, for some of the ends bear signs of scorching. ”

A bone industry at Choukoutien is also recognized by a Western authority on Chinese prehistory, J. S. Aigner, who wrote (1981, p. 144): “While Breuil may have been advocating tools overly, there is no question that human alteration of bones through processing activities and to a lesser extent through use (as tools) is clearly indicated. ”

Unfortunately, it is not possible to completely verify these claims, because many of the older pre-World War II specimens were lost. As related by W. C. Pei (Pei Wenzhong) in a introduction to a series of photos of the bones published by Breuil in 1939: 'The specimens described herein by Professor H. Breuil have been placed at the disposal of the Museum of Geological Survey of China in Nanking for exhibition purposes, but owing to the hostilities In 1937 have become lost. It is indeed most unfortunate that these valuable objects, once so carefully studied by such a leading authority in pre-history as Professor Breuil, should no longer be available to science" (Bowden 1977, p 99). Pei, it may be noted, was not very much in favor of the bone tool industry.

#### 9.1.9 The Fossil Bones of Sinanthropus and Signs of Cannibalism

On March 15, 1934, Davidson Black was found at his work desk, dead of a heart attack. He was clutching his reconstruction of the skull of Sinanthropus in his hand. Shortly after Black's death, Franz Weidenreich assumed leadership of the Cenozoic Research Laboratory and wrote a comprehensive series of reports on the Peking man fossils. According to Weidenreich, the fossil remains of Sinanthropus individuals, particularly the skulls, suggested they had been the victims of cannibalism.

Most of the bones discovered at Choukoutien were cranial fragments. Weidenreich (1943, p. 7) stated: "none of the 14 skulls recognised as belonging to Sinanthropus is complete."

Weidenreich said that the skulls had been broken after fossilization by huge masses of stone falling from the roof of the cave. He pointed out that the other animal bones found at Choukoutien were similarly crushed and fragmented. But certain aspects of the cranial fragments of Sinanthropus led Weidenreich to conclude that they had also been broken before being covered by material from collapsed portions of the cave.

Weidenreich particularly noted that the relatively complete skulls all lacked portions of the central part of the base. He observed that in modern Melanesian skulls "the same injuries occur as the effects of ceremonial cannibalism" (Weidenreich 1943, p. 186).

Besides the missing basal sections, Weidenreich also noted other signs that might possibly be attributed to the deliberate application of force. For example, some of the skulls showed impact marks of a type that "can only occur if the bone is still in a state of plasticity," indicating that "the injuries described must have been inflicted during life or soon after death" (Weidenreich 1943, pp. 186-187).

Weidenreich (1943, p. 188) admitted that some of these injuries might have been caused by "stones falling from the roof of the cave on the individuals living in it" or "the bites of big carnivores having their dens near-by," but pointed out that others "look like incisions made by cutting implements."

Weidenreich (1943, p. 188) observed: "Blows inflicted on living individuals or corpses by stones falling accidentally cannot be held responsible for the destruction of the base of the skull. This fact, together with the cut-like lesions, rather points to injuries incidentally practiced by man. "



Some of the skull fragments showed depressions that possibly could be interpreted as animal bites. But Weidenreich (1943, p. 189) stated: "considering the size, form and thickness of the vault it is difficult to imagine how the animal could find adequate points at which to drive its teeth and crack the vault by seizing it between its upper and lower jaws."

Also, according to Weidenreich (1943, p. 189), there were no cases on record that could be cited as "as examples of bites of carnivores inflicted on completely intact human skulls."

Some of the few long bones of *Sinanthropus* found at Choukoutlen also displayed signs that to Weidenreich suggested human breakage. "It seems to be certain" he said (1943, p. 189), "that the lengthwise splitting involving the greater part of the shaft bones, cannot have been executed by carnivores but must have been done by man."

Von Koenigswald (1956, p. 49) agreed with this analysis, stating: "The thigh bones of Peking man found at Chou K'ou Tien are all severely damaged and often smashed into small pieces to extract the marrow. The damage was not the work of beasts of prey, but undoubtedly of humans."

Weidenreich (1943, p. 190) then offered this summary of his observations: "My verdict is that the destruction of the base and the blows on the top of the skull are the incidental work of man, although the possibility cannot be entirely excluded that at least those lesions which indicate they were produced by pointed or blunt agents may have been caused by stones falling from the roof of the cave on a living individual. Later on the skulls were broken as carrion by carnivores, probably hyaenas, which lived in the cave and cracked the bones as long as they were fresh." It is not very likely that *Sinanthropus* and the hyenas inhabited the cave at the same time. Accepting Weidenreich's version of events, *Sinanthropus* would have been an infrequent visitor, or perhaps the cave was inhabited alternately by *Sinanthropus* and other creatures.

As to why mostly cranial fragments were found, Weidenreich believed that with the exceptions of some long bones, only heads were carried into the caves. He stated: "the strange selection of human bones we are facing at Choukoutlen has been made by *Sinanthropus* himself. He hunted his own kin as he hunted other animals and treated all his victims in the same way. Whether he opened the human skulls for ritual or culinary reasons cannot be decided on the basis of the present evidence of his cultural life; but the breaking of the long bones of animals and man alike, apparently for the purposes of removing the marrow, indicates that the latter alternative is the more likely. The remains of his meals became the prey of his predatory neighbors at the foothills of Choukoutlen" (Weidenreich 1943, p. 190).

Some modern authorities have suggested that Weidenreich was mistaken in his interpretation of the fossil remains of *Sinanthropus*. Binford and Ho (1985, p. 414) pointed out that hominid skulls subjected to transport over river gravel are found with the basal section worn away. But the skulls recovered from Choukoutlen were apparently not transported in this fashion.

Binford and Ho also believed that damage to one skull, which Weidenreich thought could have been caused by cutting, was typical of a kind of animal breakage. But Weidenreich (1943, p. 189)

had considered this possibility and still proposed cutting by an Implement as the most likely cause. Even Binford and Ho (1985, p. 415) admitted that the kind of animal breakage they were proposing had "the appearance of cut or hack marks."

Binford and Ho also disagreed with Weldenreich's view that the *Sinanthropus* long bones were deliberately broken. They stated: 'Binford has examined the photographs and casts of the bones in question, and the breakage appears to be unequivocally attributable to weathering. There is no evidence that these bones were broken fresh or by percussion' (Binford and Ho 1985, p. 414).

This statement is contradicted by Weldenreich (1941, p. 5), who said about one of the femurs: "The appearance of the bony surface exposed by ... fractures indicates that the breakage occurred prior to mineralisation. . . The remaining surface of the bone is practically intact and scarcely weathered." It should be kept in mind that Weidenreich was working from the original fossils, while Binford could only study photographs and casts because the original specimens were lost during the Second World War.

Another modern authority objecting to Weldenreich's cannibalism interpretation was Jean S. Aigner. She suggested: "The fact that the base of several brain cases is missing is not a particularly strong point when we recall the numerous examples of Recent burials with this portion missing. The absence is due to natural processes, not artificial ones, with the part of the skull resting in contact with the ground being eroded and dissolved away" (Aigner 1981, p. 128). But the Choukoutlen finds were not burials, and there is no indication that the skulls were oriented in such a way (basal section down) as to produce the effect suggested by Aigner.

Aigner (1981, p. 128) offered this conclusion: "Weldenreich's reconstruction of the practices associated—murder, severing the head, and removing the brain, dissecting the long-bones and depositing just those parts in the cave—is difficult indeed to understand in light of the strong evidence (hearths) that the cave was a habitation site and not simply a dump. " However, we have already seen, according to one modern opinion (Binford and Ho 1985), that there is no real evidence for hearths at Choukoutlen. It is hard to attribute the huge masses of ash found there to campfires. Furthermore, the distribution of *Sinanthropus* remains is rather indicative of a dump. As noted by Weldenreich (1943, p. 186): "The distribution of *Sinanthropus* bones both horizontally and vertically throughout the deposit is an accidental one as is that of the animal bones."

Binford and Ho (1985, p. 428), as mentioned previously, proposed that carnivores had brought the hominid bones into the caves. But Weidenreich (1935, p. 453) said: 'transportation by . . . beasts of prey is impossible. . . traces of biting and gnawing ought to have been visible on the human bones, which is not the case. Therefore, the only possibility is that man himself brought the bones into the cave, by preference brain cases and Jaws.'

As evidence of cannibalism, Weidenreich pointed out that the *Sinanthropus* remains were predominantly those of children and females, the easiest to kill. Weidenreich (1935, p. 456) then cited several examples of cannibalism from Europe, stating: "Matiegka . . . has reviewed rather completely human skeletal material of prehistoric times of all Europe giving testimony about

cannibalism. In many of these cultural places bones have been found which were broken to pieces and mingled with those of animals, with charcoal, ashes, and stone tools or splinters of them. Very frequently there were among the human bones skulls, isolated Jaws, or fragments of them. ... In some finding places the remains of children, adolescents and sometimes also of women were prevailing. The resemblance to the conditions existing in Choukoutien is obvious."

But Marcellin Boule, director of the Institute de Paleontologie Humaine in France, suggested another possibility—namely, that *Sinanthropus* had been hunted by a more intelligent type of hominid. Boule believed that the small cranial capacity of *Sinanthropus* implied that this hominid was not sufficiently intelligent to have created the stone and bone implements that were discovered in the cave.

In his description of the stone tool industry, Boule said: "It is important to note that this industry is not primitive, since M Breuil himself acknowledges that 'many of (its) features are not found in France until the Upper Palaeolithic' . . . . Accompanying a being like *Sinanthropus* one would have expected to find an eolithic industry, and not true gravers and scrapers and other tools 'sometimes of fine workmanship'" (Boule and Vallois 1957, p. 145). Hence Boule concluded that the Choukoutien implements and fires were created by a "true man," *Homo sapiens*. who preyed upon *Sinanthropus*. Professor Boule did not believe that *Sinanthropus* necessarily represented an intermediate link in the chain of evolution from ape to *Homo sapiens*. He believed instead that *Sinanthropus* was simply an apelike being who was hunted for food by *Homo sapiens*.

Boule stated his position quite clearly: "We may therefore ask ourselves whether it is not overbold to consider *Sinanthropus* the monarch of Choukoutien, when he appears in its deposit only in the guise of a mere hunter's prey, on a par with the animals by which he is accompanied" (Boule and Vallois 1957, p. 145).

If the remains of *Sinanthropus* were the trophies of a more intelligent hunter, who was that hunter and where were his remains? Boule pointed out that there are many caves in Europe that have abundant products of Paleolithic human industry, but the "proportion of deposits that have yielded the skulls or skeletons of the manufacturers of this industry is

infinitesimal" (Boule and Vallois 1957, p. 145).

Boule further observed: "We may say that the absence of human bone remains is the rule and their presence the exception" (Boule and Vallois 1957, p. 145). For example, Boule stated that 4,000 cubic yards of deposits were

systematically and carefully removed from the Prince's Cave at Grimaldi in the hope of finding human bones; however, not

a single fragment of a human bone was discovered, despite the discovery of numerous animal bones and stones shaped by humans (Boule and Vallois 1957, p. 145).

Therefore, the hypothesis that a more intelligent species of hominid hunted *Sinanthropus* at Choukoutien is not ruled out simply because its fossil bones have not yet been found at Choukoutien. From our previous chapters, it may be recalled that there is evidence, from other parts of the world, of fully human skeletal remains from periods of equal and greater antiquity than that represented by Choukoutien. For example, the fully human skeletal remains found at Castenedolo in Italy are from the Pliocene period, over 2 million years ago.

#### 9.1.10 Discoveries in the Upper Cave

In the early 1930s, some fully human remains were found at Choukoutien, in the Upper Cave, which lies above the main deposits. Modern researchers, using a combination of carbon 14 tests and studies of faunal remains, have said these human fossils are only about 20,000 years old.

A *Sinanthropus* upper Jaw was found along with the human fossils. The usual explanation is that the *Sinanthropus* Jaw was derived from the Lower Cave deposits. Weidenreich (1943, p. 16) said about the Jaw found in the Upper Cave: "it distinctly differs from the other bones found in this cave by the high degree of mineralization, the special color, the primitiveness of the form, the considerable size of the teeth, and the way in which the bone is broken. In all these particularities the maxilla shows a greater resemblance to the *Sinanthropus* Jaws recovered from Locality 1 than to the maxillae of the Upper Cave Man which have been found in connection with their pertaining skulls. "

Stone implements were also discovered in the Upper Cave. According to Pei (1939, p. 16) some of these quartz implements look "surprisingly similar to some pieces found in the much older *Sinanthropus* deposits." Pei (1939, p. 16) then added that "it is quite possible that the here described quartz implements were collected by the Upper Cave Man or introduced by natural agencies into the Upper Cave from the *Sinanthropus* deposits."

#### 9.1.11 Our Knowledge of Peking Man

All in all, the picture we get of *Sinanthropus* is not very much like the almost human ancestral hominid seen in textbook paintings and museum exhibits—the expert hunter sitting by his hearth in his cave home. Instead, making use of all the available evidence and points of view, we see through the haze of several hundred thousand years the outlines of a somewhat apelike creature, who was most likely a scavenger who sometimes got scavenged himself—perhaps by his own kind, perhaps by a more advanced hominid.

In Boule's opinion, Weidenreich and others tended to overemphasize the humanlike features of *Sinanthropus*. Von Koenigswald (1956, p. 51) wrote: "Our real knowledge of Peking man does not amount to very much. The skull is the best-known factor, and Weidenreich used it to have a rather excessively idealized reconstruction made by the American sculptress Lucille Swan, which came to be known in Peking as 'Nelly.'" Of course, even the very tenn Peking "man" carries with it strong, and undesired, overtones of human attributes and ancestorship.

This idealization was perhaps to be expected, because according to modern evolutionary theory *Sinanthropus* (or *Homo erectus*) is supposed to be the immediate ancestor of *Homo sapiens*. But Boule (1937), considering reports coming from Choukoutien, said: "To this fantastic hypothesis, that the owners of the monkey-like skulls were the authors of the large-scale industry, I take the liberty of preferring an opinion more in conformity with the conclusions from my studies, which is that the hunter was a real man and that the cut stones, etc., were his handiwork" (Fix 1984, pp. 130-131).

Why did Boule say the skull of *Sinanthropus* was monkeylike? There are several reasons. One, of course, is the large brow ridges. When the skull of *Sinanthropus* is seen from directly above, the ridges stick out like handlebars on a bicycle (Figure

9.1, p. 556). Another apelike feature is the "postorbital constriction," or narrowing of the skull in back of the eye sockets. Humans do not have this. Place your fingers at the corners of your eyes and then run them back to your temples, just above the ear. You will notice that the surface is flat. But in *Sinanthropus*, immediately in back of the eyes, there is a very pronounced indentation on either side (Figure 9.1). Another apelike feature is the general shape of the skull when seen from behind. The skull of *Sinanthropus* is somewhat narrower at the top than at the bottom (Figure 9.1). In contrast, human skulls are normally wider at the top than at the bottom (Boule and Vallois 1957, p. 135). Also, as previously noted, the walls of the *Sinanthropus* skull are twice as thick as those of the average human skull.

Figure 9.1. The first *Sinanthropus* skull, discovered in 1929 at Choukoutien, viewed from above (Oia 1975, p. 17) and from the rear (Boule 1937, p. 7). Like the apes, *Sinanthropus* has enormous browridges and a pronounced postorbital constriction (top). Also, the *Sinanthropus* skull, seen from the rear (bottom), is narrower at the top than at the bottom, another apelike feature.

The capacity of the *Sinanthropus* cranium is said to average around 1000 cubic centimeters, more than the anthropoid ape average of 600 cubic centimeters but less than the human average of about 1400 cubic centimeters. There have been suggestions that Black and Weidenreich reconstructed shattered *Sinanthropus* skulls in such a way as to increase their cranial capacity above the range for apes. Concerning the initial skull recovered in 1929, Bowden, after careful study of Black's three reports and accompanying photographs, noted that the bottom edge of the reconstructed skull pictured in the last report was lower than the bottom edge shown in the first photograph, taken shortly after the skull was excavated. Bowden believed this discrepancy could be accounted for if one assumed the lower part of the skull had been carelessly cropped out of the original photograph. More likely, according to Bowden (1977, p. 118), was the possibility that the

"reconstruction of the base of the skull was carried out in such a way that it was made deeper" thus yielding a larger and less apelike volume.

Boule pointed out that there was strong sexual dimorphism in *Sinanthropus*. That is to say, there was great variation between the size of males and females, much more than in human beings. This is an apelike feature, which is reflected in the *Sinanthropus* Jaws found at Choukoutien (Figure 9.2). Weidenreich noticed the dimorphism when studying reconstructed Jaws of a male, female, and child, which showed unusual variation in size. Boule stated: "The total result was so polymorphous that Weidenreich wondered whether it was really a single species, or at least a single race. It is a fact, however, that this polymorphism is a simian characteristic in singular contrast to the slight sexual dimorphism in human Jaws" (Boule and Vallois 1957, p. 138). Another apelike feature of the *Sinanthropus* Jaw is that it has several openings for the dental nerve that reaches out to the skin of the chin, while the human Jaw has only one (Keith 1931, pp. 262-265). Boule also believed the dentition of *Sinanthropus* was less human than some originally thought.

Boule therefore came to the following conclusion: "By the sum total of their characters, the mandibles and teeth of *Sinanthropus* denote a large Primate that was . . . certainly less human than the Mauer [Heidelberg] Jaw, which is probably older than the Peking fossils" (Boule and Vallois 1957, p. 140).

In his physiological demotion of Peking man, Boule is in harmony with some modern researchers, such as Binford and Ho, who hesitate to attribute typically human behavior to the Choukoutien hominids.

Figure 9.2. Restored jaws of an adult *Sinanthropus* male (above) and female (below). They display substantial sexual dimorphism, an apelike feature (Boule 1937, p. 13).

Binford and Ho characterized *Sinanthropus* behaviorally as a simple scavenger who was not clearly responsible for either the animal bones or beds of ashes at the cave of Choukoutien. They concluded: "What, then, was life like in the 'cave home of Beijing man?' We think we must conclude that we do not know" (Binford and Ho 1985, p. 429). We agree with this honest statement, which respects the limitations of the empiric method when applied to such questions.

#### 9.1.12 The Fossils Disappear

As we have previously mentioned, one reason that it may be difficult to resolve many of the questions surrounding Peking man is that the original fossils are no longer available for study. By 1938, excavations at Choukoutien, under the direction of Weidenreich, were halted by guerilla

warfare in the surrounding Western Hills. Later, with the Second World War well underway, Weidenreich left for the United States in April of 1941, carrying a set of casts of the Peking man fossils.

In the summer of 1941, it is said, the original bones were packed in two footlockers and delivered to Colonel Ashurst of the U.S. Marine Embassy Guard in Peking. In early December of 1941, the footlockers were reportedly placed on a train bound for the port of ChInwangtao, where they were to be loaded onto an American ship, the President Harrison, as part of the U.S. evacuation from China. But on December 7, the train was intercepted, and the fossils were never seen again. In a statement published on March 22, 1951 in the New York Times, Pel Wenzhong (W. C. Pei) said the Americans found the fossils at the University of Tokyo after the war and secretly transported them to the American Museum of Natural History. The chairman of the department of anthropology at the Museum denied the charge (Bowden 1977, pp. 106-107).

After World War II, the Chinese Communist government continued the excavations at Choukoutien, adding a few fossils to the prewar discoveries. The present total of *Homo erectus* (*Sinanthropus*) discoveries since 1927 is 6 fairly complete skullcaps along with 12 other skull fragments, 15 pieces of lower jaws, 157 teeth, 3 fragments of upper arm bones, 1 clavicle, 7 fragments of thighbones, 1 fragment of a shinbone, and 1 wrist bone. These are said to represent the remains of 40 individuals (Wu and Lin 1983, p. 89). Recent opinion is that the clavicle is not from a hominid. In any case, most of the

Peking man fossil bones, over 90 percent, were lost during the Second World War.

#### 9.1.13 An Example of Intellectual Dishonesty

In an article about Zhoukoudan (Choukoutien) that appeared in the June 1983 issue of *Scientific American*, two Chinese scientists, Wu Rukang and Lin Shenglong, presented misleading evidence for human evolution,

Wu and Lin made two claims: (1) The cranial capacity of *Sinanthropus* increased from the lowest level of the Zhoukoudan excavation (460,000 years old) to the highest level (230,000 years old), indicating that *Sinanthropus* evolved towards *Homo sapiens*. (2) The type and distribution of stone tools also implied that *Sinanthropus* evolved,

In support of their first claim, Wu and Lin analyzed the cranial capacities of the 6 relatively complete *Sinanthropus* skulls found at Zhoukoudan, Wu and Lin (1983, p. 94) stated: "The measured cranial capacities are 915 cubic centimeters for the earliest skull, an average of 1075 cubic centimeters for four later skulls and 1140 cubic centimeters for the most recent one," From this set of relationships (Table 9.1, column A), Wu and Lin (1983, p. 94) concluded: "It seems the brain size increased by more than 100 cubic centimeters during the occupation of the cave, "

A chart in the *Scientific American* article showed the positions and sizes of the skulls found at Zhoukoudan Locality But in their explanation of this chart, Wu and Lin neglected to state that the earliest skull, found at layer 10, belonged to a child, who according to Franz Weidenreich (1935, p,

448) died at age 8 or 9, and according to Davidson Black died between ages 11 and 13. In the text of their article, Wu and Lin (1983, p, 90) did mention that one of the 6 skulls they considered was from a child who died at age 8 or 9: yet they did not specify the level at which this skull was found, Wu and Lin acknowledged that a child's skull is smaller than an adult's, But to establish an evolutionary trend In their chart, they still compared the child's skull from layer 10 with the other skulls, which are from adults,

TABLE9.1

Evidence for Supposed Evolutionary Increase In Sinanthropus Cranial Capacity at Zhoukoudian, China

Ynn b.p.

Layer

A: Data Reported

by

Wu and Lin, 1983

B: Complete Data

230,000

1-2

230.000

3

1140cc(V)

1140 cc (V)

290,000

4

290.000



5

350,000

6

350,000

7

420,000

8

1075 cc = average of 4 skulls

1225 cc (X), 1015 cc (XD,

1030 cc (Xm, 1025 cc<sup>®</sup>

1225 cc (X), 1015 cc (XI),

420,000

700,000

In Scientific American (June 1983), Wu Rukang and Lin Shenglong used the data in column A to suggest that

Sinanthropus individuals evolved a larger cranial capacity during the 230,000 years they occupied the Zhoukoudian cave. But in their table Wu and Lin did not mention that the oldest skull (HIS was that of a child, making it useless for comparison with the other skulls, which were those of adults. Furthermore, Wu and Lin gave an average for 4 skulls from layers 8 and 9 (H X, XT', and XII), without mentioning that one of these skulls (X) had a cranial capacity of 1225 cc, larger than the most recent skull from layer 3. The complete data, shown in column B, reveals no evolutionary increase in cranial capacity. All of the data in the table was originally reported by Weidenreich (1935, 1943), except for the cranial capacity of the skull found at layer 3. In 1934, Weidenreich reported the discovery of some pieces of this skull, which he later designated skull V (Weidenreich 1943, p. 5). Then in 1966, Chinese paleontologists found other pieces of this same skull Jia 1980, p. 26). The reconstruction of this skull and the cranial capacity measurement were carried out in 1966.

Wu and Lin also neglected to mention that one of the skulls discovered in layers 8 and 9 (skull X) had a cranial capacity of 1,225 cc, which is 85 cc larger than the most recent skull (V) found in layer 3. When all the data is presented, (Table 9.1, column B) It is clear that there is no steady increase in cranial capacity from 460,000 to 230,000 years ago.

Except the skull pieces from layer 3 found in 1966, Weidenreich examined all the skulls in Table 9.1. He saw no evolutionarily significant change in their general shape or cranial capacity from the bottom to the top of the excavation: "The morphological character of the Sinanthropus population of Locality 1, therefore, remained unchanged during the long periods of time necessary for the filling-up of the cave. Viewed from the morphological standpoint the population represents a uniform type" (Weidenreich 1935, p. 450).

Another attempt has been made, using cranial data, to establish that evolution took place during the Zhoukoudian occupation. According to W. W. Howells (1977, p. 70), Chinese paleoanthropologist Ku Yu-min believed that the skull from layer 3 should be "viewed as more progressive than the other known individuals, from the cranial capacity and various other features." Howells (1977, p. 70) described these features: "The bone of the frontal is thinner. The occipital torus is less developed than in most (skull XI excepted). . . . The frontal sinuses are larger than in all others, except skull III. The vault as reconstructed is higher. From this assessment of status . . . , the Chinese believe that the morphology of later Peking man is thus established, as showing evolutionary development within the Peking man phylum. "

The above reasoning is not totally convincing. Concerning cranial capacity, the smallest adult skull (skull XI, 1015 cc) and the largest (skull X, 1225 cc) are both from layers 8 and 9. The skull from layer 3 (skull V, 1140 cc) falls within this range. This hardly demonstrates evolutionary development.

As far as other features of the skulls are concerned, one cannot make a statistically meaningful statement about them. Because of individual variations in a population, many skulls at each time horizon are needed to show that the population has undergone significant change. But at Zhoukoudian only one skull is substantially more recent than the others, and this single skull is by

no means sufficient to establish a trend in any particular direction. Also, from Ku Yu-mIn's description above it appears that some of the supposedly progressive characteristics of the most recent skull (V) are also found in the older skulls—such as the large frontal sinuses in skull III and the less-developed occipital in skull XI.

In addition to discussing an evolutionary increase in cranial capacity, Wu and Lin noted a trend toward smaller tools in the Zhoukoudian cave deposits. They also reported that the materials used to make the tools in the recent levels were superior to those used in the older levels. The recent levels featured more high-quality quartz, more flint, and less sandstone than the earlier levels (Wu and Lin 1983, p. 92).

But a change in the technological skill of a population does not imply that this population has evolved physiologically. For example, consider residents of Germany in a.d. 1400 and residents of Germany in a.d. 1990. The technological differences are awesome—Jet planes and cars instead of horses; television and telephone instead of unaided vision and voice; tanks and

missiles instead of swords and bows. Yet one would be in error if one concluded that the Germans of 1990 were physiologically more evolved than the Germans of 1400. Hence, contrary to the claim of Wu and Lin, the distribution of various kinds of stone tools does not imply that *Sinanthropus* evolved.

The report of Wu and Lin, especially their claim of increased cranial capacity in *Sinanthropus* during the Zhoukoudian cave occupation, shows that one should not uncritically accept all one reads about human evolution in scientific journals. It appears the scientific community is so committed to its evolutionary doctrine that any article purporting to demonstrate it can pass without much scrutiny.

## 9.2 OTHER DISCOVERIES IN CHINA

Although Zhoukoudian is the most famous paleoanthropological site in China, there are many others. Discoveries at these sites have included fossils representative of early *Homo erectus*, *Homo erectus*, Neanderthals, and early *Homo sapiens*, thus providing an apparent evolutionary sequence. But the way in which this progression has been constructed is open to question.

### 9.2.1 Dating by Morphology

As we have seen in our discussion of human fossil remains discovered in China and elsewhere, it is in most cases not possible to date them with a very high degree of precision. Finds tend to occur within what we choose to call a "possible date range," and this range may be quite broad, depending upon the dating methods that are used. Such methods include chemical, radiometric, and geomagnetic dating techniques, as well as analysis of site stratigraphy, faunal remains, tool types, and the morphology of the hominid remains. Furthermore, different scientists using the same methods often come up with different age ranges for particular hominid specimens. Unless one wants to uniformly consider the age judgement given most recently by a scientist as the correct one, one is compelled to take into consideration the entire range of proposed dates.

But here one can find oneself in difficulty. Imagine that a scientist reads several reports about two hominid specimens of different morphology. On the basis of stratigraphy and faunal comparisons, they are from roughly the same period. But this period stretches over several hundred thousand years. Repeated testing by different scientists using different paleomagnetic, chemical, and radiometric methods has given a wide spread of conflicting dates within this period. Some test results indicate one specimen is the older, some that the other is the older. Analyzing all the published dates for the two specimens, our investigator finds that the possible date ranges broadly overlap. In other words, by these methods it proves impossible to determine which of the two came first.

What is to be done? In some cases, as we shall show, scientists will decide, solely on the basis of their commitment to evolution, that the morphologically more apelike specimen should be moved to the early part of its possible date range, in order to remove it from the part of its possible date range that overlaps with that of the morphologically more humanlike specimen. As part of the same procedure, the more humanlike specimen can be moved to the later, or more recent, part of its own possible date range. Thus the two specimens are temporally separated. But keep in mind the following: this sequencing operation is performed primarily on the basis of morphology, in order to preserve an evolutionary progression. It would look bad to have two forms, one generally considered ancestral to the other, existing contemporaneously.

Here is an example. Chang Kwang-chih, an anthropologist from Yale University, stated: "The faunal lists for Ma-pa, Ch'ang-yang, and Liu-chiang [hominid] finds offer no positive evidence for any precise dating. The former two fossils can be anywhere from the Middle to the Upper Pleistocene, as far as their associated fauna is concerned. . . . For a more precise placement of these three human fossils, one can only rely upon, at the present time, their own morphological features in comparison with other better-dated finds elsewhere in China" (Chang 1962, p. 757). This may be called dating by morphology.

Jean S. Aigner (1981, p. 25) stated: "In south China the faunas are apparently stable, making subdivision of the Middle Pleistocene difficult. Ordinarily the presence of an advanced hominid or relict [mammal] form is the basis for determining later and earlier periods." This is a very clear exposition of the rationale for morphological dating. The presence of an advanced hominid is taken as an unmistakable sign of a later period.

In other words, if we find an apelike hominid in connection with a certain Middle Pleistocene fauna at one site and a more humanlike hominid in connection with the same Middle Pleistocene fauna at another site, then we must, according to this system, conclude that the site with the more humanlike hominid is of a later Middle Pleistocene date than the other. The Middle Pleistocene, it may be recalled, extends from 100,000 to 1 million years ago. It is taken for granted that the two sites in question could not possibly be contemporaneous.

With this maneuver completed, the two fossil hominids, now set apart from each other temporally, are then cited in textbooks as evidence of an evolutionary progression in the Middle Pleistocene! This is an intellectually dishonest procedure. The honest thing to do would be to

admit that the evidence does not allow one to say with certainty that one hominid preceded the other and that it is possible they were contemporary. This would rule out using these particular hominids to construct a temporal evolutionary sequence. All one could honestly say is that both were found in the Middle Pleistocene. For all we know, the "more advanced" humanlike hominid may have preceded the "less advanced" apelike one. But by assuming that evolution is a fact, one can then "date" the hominids by their morphology and arrange the fossil evidence in a consistent manner.

#### 9.2.2 Tongzi, Guizhou Province

Let us now consider a specific example of the date range problem. In 1985, Qiu Zhonglang reported that in 1971 and 1972 fossil teeth of *Homo sapiens* were found in the Yanhui cave near Tongzi, in Guizhou province, southern China. The Tongzi site contained a *SlegodonAiluropoda* fauna. *Slegodon* is a type of extinct elephant, and *Ailtiropoda* is the giant panda. This *SlegodonAiluropoda* fauna is typical of southern China during the Middle Pleistocene.

The complete faunal list for the Tongzi site given by Han Defen and Xu Chunhua (Han and Xu 1985, pp. 285-286) contains 24 kinds of mammals, all of which are also found in Middle (and Early) Pleistocene lists given by the same authors (Han and Xu 1985, pp. 277-283). But a great many of the genera and species listed are also known to have survived to the Late Pleistocene and the present.

The author of the report on the Tongzi discoveries stated: "the Yanhui Cave was the first site containing fossils of *Homo sapiens* discovered anywhere in the province. . . . The fauna suggests a Middle-Upper Pleistocene range, but the archaeological [human] evidence is consistent with an Upper Pleistocene age" (Qiu 1985, pp. 205-206).

In other words, the presence of *Homo sapiens* fossils was the determining factor in assigning a Late Pleistocene age to the site. This is a clear example of dating by morphology. But according to the faunal evidence reported by Qiu (1985), all that can really be said is that the age of the *Homo sapiens* fossils could be anywhere from Middle Pleistocene to Late Pleistocene.

But there is stratigraphic evidence suggesting a strictly Middle Pleistocene range. Qiu (1985, p. 206) gave the following information: "The deposits in the cave contain seven layers. The human fossils, stone artifacts, burned bones, and mammalian fossils were all unearthed in the fourth layer, a stratum of greyishyellow sand and gravel." This concentration in a single layer suggests that the human remains and the animal fossils, all of mammals found at Middle Pleistocene sites, are roughly contemporaneous. And yellow cave deposits in South China are generally thought to be Middle Pleistocene (Han and Xu 1985, p. 273; Simons and Ettel 1970, p. 84).

Our own analysis of the faunal list also suggests it is reasonable to narrow the age range to the Middle Pleistocene. *Slegodon*, present at Tongzi, is generally said to have existed from the Pliocene to the Middle Pleistocene (Belyaeva et al. 1962, p. 365). In a list of animals considered

important for dating sites in South China, Aigner (1978) indicated that *Slegodon orientalis* survived only to the late Middle Pleistocene, although she did place a question mark after this entry.

A strictly Middle Pleistocene age for the Tongzi cave fauna is supported by the presence of a species whose extinction by the end of the Middle Pleistocene is thought to be more definite. In her list of mammals considered important for dating sites in South China, Aigner included, in addition to *Slegodon on'entalis*, other species found at Tongzi. Among them is *Megalapirus* (giant tapir), which Aigner (1981, p. 289) said is confined to the Middle Pleistocene. The species found at Tongzi is listed as *Megalapirus atigtistus* Matthew et Granger (Han and Xu 1985, p. 25). Aigner (1981, p. 325) characterized *Megalapirus atigiisliis* as a "large fossil form of the mid-Middle Pleistocene south China collections." We suggest that *Megalapirus atigtistus* limits the most recent age of the Tongzi faunal collection to the end of the Middle Pleistocene (Figure 9.3).

Another marker fossil listed by Aigner (1981, p. 289) is *Crrcula croctila* (the living hyena). which first appeared in China during the middle Middle Pleistocene. Since *Crocula crrcula* is present at Tongzi, this limits the oldest age of the Tongzi fauna to the beginning of the middle Middle Pleistocene (Figure 9.3).

In summary, using *Megalapirus atigtsttisand* *Croctita crocuta* as marker fossils, we can conclude that the probable date range for the *Homo sapiens* fossils found at Tongzi extends from the beginning of the middle Middle Pleistocene to the end of the late Middle Pleistocene (Figure 9.3).

So Qiu (1985), in effect, extended the date ranges of some mammalian species in the *StegodonAilurrpoda* fauna (such as *Megatapirus rngtistus*) from the Middle Pleistocene into the early Late Pleistocene in order to preserve an acceptable date

for the *Homo sapiens* fossils. Qiu's evolutionary preconceptions apparently demanded this operation. Once it was carried out, the Tongzi *Homo sapiens*, placed safely in the Late Pleistocene, could then be introduced into a temporal evolutionary sequence and cited as proof of human evolution. If we place Tongzi *Homo sapiens* in the older part of its true faunal date range, in the middle Middle Pleistocene, he would be contemporary with Zhoukoudian *Homo erectus*. And that would not look very good In a textbook on fossil man in China.

nee

PLEISTOCENE

EARLY

JVfIDDLE

Early Mid Late

Early Mid Late

Early

according to Qiu crocirta

ion

of Site n', sapiens)

Figure 9.3. Age of *Homo sapiens* fossils at Tongzi site, South China. Qiu (1985, p. 206) said the Tongzi mammalian fauna was Middle to Late Pleistocene, but used *Homo sapiens* fossils to date the site to the Late Pleistocene. But if we instead use the mammalian fauna to date the *Homo sapiens* fossils, we arrive at a different age for the site. *Stegodon* became extinct at the end of the Middle Pleistocene, possibly surviving into the early Late Pleistocene (grey part of bar) in some South China locales (Aigner 1981, p. 289). *Megatapirus augustus* (giant tapir) definitely did not survive the Middle Pleistocene (Aigner 1981, p. 289). The presence of *Stegodon* and especially

*Megatapirus augustus* limit the most recent age for the Tongzi site to the end of the Middle Pleistocene. The presence of *Crocota crocuta* (the living hyena), which first appears in the middle Middle Pleistocene (Aigner 1981, p. 289), limits the oldest age for the Tongzi site to the beginning of the middle Middle Pleistocene. Therefore, the allowed range for the *Homo sapiens* fossils at Tongzi extends from the beginning of the middle Middle Pleistocene to the end of the late Middle Pleistocene.

### 9.2.3 Lantian Man



Let us now consider another element in the confusing picture of the Chinese Middle Pleistocene—Lantian man. In 1963, Zhang Yuping and Huang Wanpo, of the Institute of Vertebrate Paleontology and Paleoanthropology (IVPP), discovered a *Homo erectus* mandible (lower jaw) at Chenjiawo village in Lantian county, Shaanxi province. In 1964, another team discovered the tooth of a human being at Gongwangling, also in Lantian county. Chunks of fossil-bearing rock from Gongwangling were transported to Beijing. There a hominid skullcap was discovered, along with an upper jaw bone and 3 molars, one unattached. These specimens were classified as *Homo erectus* Just as the Chenjiawo jaw had been Oia 1980, pp. 13-14).

We will investigate the case of Lantian man, which shows the complexity and ambiguity underlying apparently simple paleoanthropological statements. This complexity and ambiguity allows room for manipulation of data according to preconceptions. If what follows seems complicated, that is because it is. Some authorities placed Lantian man in the same period of time as Beijing man. For example, L. Yung-Chao and his coworkers (Nilsson 1983, p. 335) assigned both Lantian man and Beijing man to China's Taku-Lushan interglacial period, in the middle Middle Pleistocene (Table 9.2).

TABLE 9.2

#### Correlation of Chinese and European Glacials and Interglacials

Period

European

Chinese

Holocene

Present Warm Period

Present Warm Period

Late Pleistocene

Wurm Glacial

Tali Glacial

Late Pleistocene

Eemian Interglacial

TaliLushan Interglacial

Late Middle Pleistocene

Eemian Interglacial

TaliLushan Interglacial

Late Middle Pleistocene

Riss II Glacial

Lushan Glacial

Late Middle Pleistocene

Dford Interglacial

Lushan Glacial

Late Middle Pleistocene

Riss I Glacial

Lushan Glacial

Middle Middle Pleistocene

Holstein Interglacial

Taku-Lushan Interglacial

Middle Middle Pleistocene

Mindel Glacial

Taku Glacial

Middle Middle Pleistocene

Cromerian Interglacial

Poyang-Taku Interglacial

Early Middle Pleistocene

Gunz Glacial

Poyang Glacial

Sources for this table are Delson 1977, p. 45 and Aigner 1981, p. 32.

And Yale professor Kwang-chih Chang wrote in the 1977 edition of his book *The Archaeology of Ancient China*: "Geologically the Lan-fien fossils occurred in strata broadly comparable with the Chou-k'ou-tien sedimentation" (Chang 1977, p. 53). He said the ChenJiawo mandible occurred "in association with fossil remains . . . that recall the Chou-k'ou-tien fauna" (Chang 1977, p. 53). The Gongwangling skull, said Chang (1977, p. 54), was found "in association with mixed Chou-k'ou-tien-Wan Hsien fauna." Wan Hsien is a site in South China, the fauna of which includes a component comparable in age to the fauna discovered at Zhoukoudian (Aigner 1981, p. 288). Chang (1977, pp. 53-54) concluded: "both Ch'en-chia-wo and Kung-wang-ling were probably datable to the Taku-Lushan Interglacial, contemporaneous with Peking Man of Chou-k'ou-tien." The TakuLushan Interglacial is said to be equivalent to the Holstein Interglacial of the European middle Middle Pleistocene (Chang 1977, p. 46). Pollen studies of the Lantian man sites indicated "prevalence of grassy species and broadleaved trees of an interglacial environment" (Chang 1977, pp. 53-54).

But Jia Lanpo (1980, p. 16) pointed out that Lantian man had thicker cranial walls than Beijing man and a much smaller cranial capacity—about 778 cubic centimeters compared with an average of over 1000 cubic centimeters for the Zhoukoudian *Homo erectus* population (Jia 1980, p. 15). Jia therefore concluded that while the Jaw from ChenJiawo might be contemporaneous with Beijing man in the middle Middle Pleistocene, the skullcap from Gongwangling was older.

#### 9.2.3.2 Morphological Dating of Lantian Man

So who was correct, Chang or Jia? J. S. Aigner discussed the Lantian man controversies in her book-length survey of Chinese discoveries. It is generally an author of broad surveys, such as Aigner, who tries to sort out conflicting reports and adjust possible date ranges so that a coherent evolutionary progression of fossil hominids in China, or elsewhere, emerges. And it is on this level that the problems of personal bias and data manipulation are most clearly evident. Adjustments are sometimes made, without adequate supporting evidence, simply to make the fossils fit some preordained scheme.

Here is how Aigner dealt with Chang, who, as we have just seen, did not accept a pre-Zhoukoudian date for the Gongwangling skull. Aigner (1981, p. 82) stated: "Chang (1977) also appears reluctant to accept the [skull's] early dating although he does accept its earliness morphologically speaking, on which point Wu (1973) and I (Aigner and Laughlin 1973) concur." This concept of "morphological earliness" assumes what must be demonstrated, providing a good example of how evolutionary prejudices distort paleoanthropological research.

With this in mind, consider Aigner's concluding statement on the skull of Lantian man (1981, p. 244): 'The massive supraorbital ridges, pronounced postorbital constriction, low frontal squama, and cranial height, extraordinary thickness of the cranial wall, and small cranial capacity [778 cc]

indicate this form is more primitive than both *Sinanthropus* and *Homo erectus* from Trinil. It is morphologically closer to the earlier form from the Djetis beds of Java; in my opinion it must be considered temporally earlier than any of the *Sinanthropus* remains for these same reasons." In other words, Lantian man must be dated morphologically so that he can be integrated into the existing evolutionary sequence. Otherwise, the sequence would be disrupted. Wu Rukang and Dong Xingren provided another example of this prejudice in their statement: "Among China's *H. erectus* fossils, those from Lantian exhibit more primitive morphological characteristics than do those from Zhoukoudian. . . . We believe these differences are a reflection of both spatial and temporal evolutionary diversification" (Wu, R. and Dong 1985, p. 88).

#### 9.2.3.3 Comparison of Faunal Evidence from Gongwangling and Chenjiawo

After arguing that the primitiveness of the Gongwangling skullcap meant that it was older than the Beijing man fossils from middle Middle Pleistocene Locality I at Zhoukoudian, Aigner (1981, pp. 81-82) said about Gongwangling: "The other faunal remains certainly demand an early Middle Pleistocene age for the locality, not contemporaneous with Choukoutien 13 or I as has been suggested by some."

Aigner believed that the Chenjiawo site, where the Lantian man jaw was found, was in fact roughly contemporaneous with Zhoukoudian in the middle Middle Pleistocene. She therefore sought to prove that the Gongwangling site, with the primitive *Homo erectus* skullcap, was older than Chenjiawo, and hence older than Zhoukoudian (Aigner and Laughlin 1973, p. 102).

But W. W. Howells (1977, p. 69) noted: "According to the Chinese this is not the case, since the faunas of the two sites [Gongwangling and Chenjiawo] coincide except for a few species and contain a species of elephant unique to both; also the formation is the same at both localities and has been mapped across the interval between them." As we have seen (Section

9.2.3.1), some of the authorities who considered the jaw and skullcap to be of the same age believed they were both contemporaneous with Beijing man.

One reason why Aigner (1981, p. 329) thought the Chenjiawo jaw was younger than the Gongwangling skull concerned fossils of *Ochotonoides*, an extinct mouse hare that supposedly belongs to the early Middle Pleistocene and earlier. Fossil remains of *Ochotonoides* were found at the same level as the Gongwangling man skullcap. This would appear to place the skullcap in the early Middle Pleistocene, before the middle Middle Pleistocene Zhoukoudian occupation. But, according to Aigner, *Ochotonoides* fossils were found only below the Chenjiawo jaw, suggesting the jaw belonged to a period later than the early Middle Pleistocene—i.e., the middle Middle Pleistocene.

Aigner derived her information about the position of the *Ochotonoides* fossils at Chenjiawo from a 1975 report by the Chinese scientists Zhou Mingzhen and Li Chuankuei. But according to Wu Xingzhi and Wang Linghong, Zhou and Li "reported the discovery of an additional maxilla and mandible of the same animal from the same layer in which the *H. erectus* mandible was found. In fact, no

discontinuity is discernible and this suggests the various levels in question do not represent different interglacial events as Aigner and Laughlin (1973) suggested" (Wu, X. and Wang, L. 1985, p. 37). The interglacial event to which both Gongwangling and ChenJiawo belong remains open to question, but, as we have noted, some authorities assign both Gongwangling and ChenJiawo to the same interglacial period represented at Zhoukoudlan (the Holstein, or Taku-Lushan interglacial).

Given this latter view, the presence of *Ochotonoides* at both Gongwangling and ChenJiawo might be taken to indicate that this genus survived to the time of the Zhoukoudian *Homo erectus* occupation in the middle Middle Pleistocene. Interestingly enough, the initial excavators of Zhoukoudlan Locality 1 listed *Ochotonoides* among the forms found there, which would appear to confirm the idea that Beijing man is contemporaneous with Lantian man. But Aigner (1981, pp. 301, 329) informs us that "the Choukoutlen 1 form is revised [by her] as *Ochotona*" a related but more recent genus that survives today in Mongolia, Tibet, and elsewhere. One cannot help but feel a little suspicious about her motives for this reclassification.

Aigner also tried to use pollen studies to support her view that Gongwangling was earlier than ChenJiawo and Zhoukoudlan. Aigner (1978, p. 26) stated: "The Choukoutlen assemblage is interpreted as a Holstein interglacial temperate flora. . . . Both the limited assemblages from Lantian represent temperate flora dating to the Holstein and to an earlier warm interval." The earlier warm interval would seem to be the Cromerian or Poyang-Taku interglacial (Table 9.2, p. 566). But Chang used the same pollen studies to place both Lantian man sites in the Holstein (Taku-Lushan) interglacial (1977, pp. 53-54).

In her capacity as synthesizer of the hominid discoveries in China, Aigner very obviously wanted to adjust things so that the Lantian skullcap would appear in the geological column before Beijing man of Zhoukoudian. But her reasons for insisting on this are not immune to criticism. Altogether, there appears ample reason to suppose that Gongwangling and Chenjiawo might be of the same age, and contemporary with Zhoukoudian.

In the fourth edition of his book *The Archaeology of Ancient China*, Chang (1986, p. 38) stated: "Geologically the Lan-t'ien fossils occurred in strata broadly contemporary with the Chou-k'outien sedimentation." According to 570 9. *Peking Man and Other Finds in China* Chang (1986, p. 38), both Zhoukoudian and the Lantian sites (Gongwangling and Chenjiawo) are part of the same reddish Li-shih loess deposits. Chang (1986, p. 38) said the Chenjiawo fauna had "elements that recall the Chou'ou-tien fauna." These statements appear to support his original position that Gongwangling and Chenjiawo (and hence Zhoukoudian) were nearly contemporary.

Nevertheless, Aigner's reports apparently induced Chang to change his mind about the relative ages of Gongwangling and Zhoukoudian. Chang (1986, p. 38) went on to say that the Gongwangling fauna "is apparently more archaic than that of Ch'en-chia-wo, which led some scholars to place the Kung-wangling cranium into a much earlier period than the Ch'en- chia-wo mandible." Chang here cited the 1973 paper by Aigner and Laughlin, which he had earlier said was

mistaken. Chang added that the placement of Gongwangling before Chenjiawo and Zhoukoudian Locality 1 was “later confined by paleomagnetic dating.”

But our own reading of the geological evidence, paleomagnetic dates, and faunal analysis presented by Aigner and Chang leads us to conclude that there is not sufficient reason to rule out the possibility that Gongwangling and Zhoukoudian Locality 1 are nearly contemporary.

#### 9.2.3.4 Paleomagnetic Dates

The paleomagnetic evidence reported by Chang is shown in Table 9.3, along with selected dates derived from other methods. Although it is possible that Gongwangling is older than Chenjiawo and Zhoukoudian Locality 1, it is also possible to conclude from the evidence reported by Chang that all three sites are nearly contemporary.

#### 9.2.3.5 Comparison of Faunal Evidence from Gongwangling and Zhoukoudian

We shall now give a detailed analysis of Aigner’s comparison of the Gongwangling fauna with that of Zhoukoudian. Seeking to demonstrate a pre-Zhoukoudian date for the very primitive Gongwangling *Homo erectus* skullcap, Aigner (1981, p. 81) said that at Gongwangling “only 37 percent of the species are modern forms, compared to 50 percent at the ‘type’ locality of the Middle Pleistocene, Choukoutien 1.” Clearly, such determinations depend heavily upon the faunal lists one uses for comparison. Aigner used a short list (Aigner and Laughlin 1973, p. 101; Aigner 1981, pp. 300-302). From a variety of reports (Zhou, M. et al. 1965, Aigner and Laughlin 1973, Aigner 1981, Chang 1977, Han and Xu 1985), we have compiled a composite master faunal list for Gongwangling. Using this longer master list (Figure 9.4, pp. 572- 573), we find that at Gongwangling 23 out of 46 taxa, or 50 percent, are modern forms (marked with dots), about the same as at Zhoukoudian according to Aigner.

TABLE 9.3

Selected Dates Obtained for Zhoukoudian Locality 1

L

Gongwangling, and Chenjiawo

B.P.

and)

Technique

Site

Soi

-507

Fission track

Zhoukoudian 1 10)

<sup>14</sup>C

-610

Thermo-

Zhoukoudian 1 10)

Pei, J.

luminescence

Paleomagnetic

Chenjiawo

Cheng ei

Paleomagnetic

Chenjiawo.

Ma a;

<sup>14</sup>C

Gongwangling

Li and L

-500

Paleomagnetic

Gongwangling

Ma et al

0

Paleomagnetic

Gongwangling

Cheng et al

The data in this table is from a book by Chang (1986, pp. 32 - 33). Level 10 is the oldest level at which hominid fossils have been found at Zhoukoudian Locality 1. Chang (1986, p. 34) said that the paleomagnetic studies supported an older date for Gongwangling than for Chenjiawo and Zhoukoudian Locality 1. This is true if one accepts the older of the two paleomagnetic dates for Gongwangling and rejects the younger one. But if one accepts the younger of the conflicting paleomagnetic dates for Gongwangling, the data allows the possible near contemporaneity of all three sites at about 500,000 years to 600,000 years ago, in the middle Middle Pleistocene.

Let us now further analyze the Gongwangling fauna in comparison with the Zhoukoudian fauna, and see whether or not it is possible to conclude in any other way that Gongwangling must be older. In our comparison, we have not limited ourselves to the fauna at Zhoukoudian Locality I, where Beijing man fossils were found, but have also included the faunal lists from the nearby Zhoukoudian Localities 13 and 15. Locality 13 is about the same age as the basal part of Locality 1 (Aigner 1981, p. 32). Locality 15 is slightly more recent than Locality 1 or perhaps equivalent to its upper phase (Aigner 1981, p. 32; Pei 1939, p. 184). Zhoukoudian Localities I, 13, and 15 all fall within the middle Middle Pleistocene (Aigner 1981, p. 32). Using their combined faunal lists, we discovered that 26 of the 46 Gongwangling taxa (about 57 percent) are found at Zhoukoudian (Figure 9.4, I).



Of the remaining taxa, 7 represent distinctly southern forms (Figure 9.4, II),

Figure 9.4. Analysis of Gongwangling fauna. In 1964, a *Homo erectus*-type cranium was found at Gongwangling. Because it was much more primitive than the *Homo erectus* crania from Zhoukoudian, some researchers thought it demanded an earlier date. These researchers sought support for their conclusion in the Gongwangling fauna (• = living forms), which they claimed was quite distinct from that of Zhoukoudian. But analysis of the Gongwangling fauna shows the following. (I) Twenty-six of the 46 taxa (57 percent) are found at Zhoukoudian Localities 1, 13, and 15. (II) Seven are typical southern forms, which would not be expected at Zhoukoudian, 500 miles to the northeast of Gongwangling. They could represent a geographical rather than a temporal variation. (III) Four of the Gongwangling taxa not found at Zhoukoudian are still living today, so they cannot be used to establish an earlier date for Gongwangling. (IV) Two species are unique to Gongwangling and thus cannot be used for relative dating. (V) Four Gongwangling species not found at Zhoukoudian are found at other Chinese sites with ages similar to Zhoukoudian. Thus far, comparison of the Gongwangling and Zhoukoudian fauna (I-V) do not establish that the Gongwangling site must be older. (VI) The only species that tends to confirm an early date is *Leptobos*, but this species differs significantly from the European type and was said by the original discoverer to resemble *Bison priscus*, which survived to the Late Pleistocene. (VII) Three species reported by original excavators at Gongwangling and Zhoukoudian were inexplicably reclassified by later researchers. The changes tend to support a desired older date for Gongwangling, and are thus suspect. It thus appears that the Zhoukoudian *Homo erectus* and the more primitive Gongwangling *Homo erectus* were contemporary in the middle Middle Pleistocene.

I. Taxa also discovered at Zhoukoudian Localities 1, 13, or 15:

- *Macaca* (monkey)
- *Neomys* (water shrew)
- Hyaena brevirostris sinensis* (Chinese short-faced hyena)
- Megatherium* (saber-tooth cat)
- *Felis* {*panthera*} *pardus* (panther)
- *Felis* {*panthera*} *tigris* (tiger)
- *Mustela* (polecat)

*Meles leucurus* (hog badger)

*Canis variabilis* (Chinese gray wolf)

- *Nyctereutes sinensis* (raccoon dog)

*Equus sanmeiianensis* (horse) *Sus lydekkeri* (Pig) *Megaloceros* (giant deer) *Pseudaxis grayi* (sika deer)

- *Gazella* (gazelle)
- *Petaurista* (flying squirrel)
- *Hystrix subcrinata* (porcupine)
- *Myospalax* (mole rat)
- *Myospalax fonanieri* (mole rat)

*Myospalax tingi* (mole rat)

- *Cricetus griseus* (hamster) *Cricetus v. varrasi* (little hamster) *Microtus ephraasi* (common vole)
- *Apodemus* (field mouse)
- *Gerbillus* (gerbil)

*Ochotonoides complicitus* (pika)

II. Taxa typical of southern China:

*Stegodon orientalis* (elephant)

- *Tapirus* (tapir)
- *Tapirus sinensis* (Chinese tapir) *Megatapirus augustus* (giant tapir) *Nestor chinesis* (clawed, horselike mammal)
- *Elaphodus cephalophus* (tufted deer)
- *Capreolus sumatraensis* *qinlingensis* (goat)

III. Taxa not at Zhoukoudian, but still existing:

- *Scaptochirus moschatus* (musk mole)
- *Ailuropus melanoleuca fovealis* (giant panda)
- *Cervus mandchuricus* (Kashgar deer)
- *Ochotona* (steppe pika)

IV. Taxa unique to Gansu in all of China:

*Dicerorhinus lantianensis* (Lantian ancient rhino)

*Leptobos brevicornis* (early bison)

V. Taxa typical of middle Middle and later Pleistocene (i.e., same age as Zhoukoudian Locality I or younger): *Diceros merki* (early rhinoceros)

*Rhinoceros sinensis* (Chinese rhinoceros)

*Arvicola terrarubra* (water vole)

*Baomys hypsodont* (a rodent first discovered at Lantian)

VI. *Leptobos* sp. (early bison)

VII. Taxa which had genus or species changed in some lists:

*Acinonyx pleistocaenicus* → *Sivapanthera pleistocaenicus* (cheetah) (Gongwangling form changed to an

earlier one)

- *Ursus thibetanus* (bear) or *Ursus thibetanus kokeni* (present at Zhoukoudian) → *Ursus eurasiaticus* (Early

Pleistocene)

*Ochotonoides* → *Ochotona* (Zhoukoudian form changed to a modern one)

One way to interpret this evidence is that the Gongwangling site represents an older and warmer interglacial period than that represented by Zhoukoudian. But there is another possible interpretation—that Gongwangling, about 500 miles southwest of Zhoukoudian, is the same age as Zhoukoudian, but has southern forms because of its warmer weather. This possibility is admitted by Aigner, who said of the southern forms: "Their presence at Kungwangling . . . may be due to the more southerly location of the site" (Aigner and Laughlin

1973, p. 102).

Zhou Mingshen, the Chinese scientist who did the initial faunal studies at Gongwangling, compared the site with others in northern China, such as Zhoukoudian: "The presence of these forms at Konwanling . . . may be interpreted as due to the more southern geographical location of the Lantian district, or to the difference in geological age of this fauna with the others, or to the insufficiency of our knowledge on the distribution of Pleistocene mammals in China in general. Probably all three of these factors are involved in this particular case" (Zhou, M. et al. 1965, p. 1044). As can be seen, there is wide latitude for manipulation of the Gongwangling faunal evidence in accordance with the leanings of a particular researcher and the requirements of evolutionary doctrine.

Is there any strong justification for attributing the presence of the southern forms at Gongwangling to temporal rather than geographical differences from Zhoukoudian? Specifically, are the Gongwangling southern species characteristic of pre-Zhoukoudian times? This does not appear to be the case. Of the 7 southern taxa, 4 are either recent or living forms. These comprise (1) *Capicornis sumatrensis*, the goat-antelope, or serow; (2) *Tapirus* sp.; (3) *Tapirus sinensis*, which some authorities consider a subspecies of the living *Tapirus indicus* (Zhou, M. et al. 1965, p. 1042); and (4) *Elaphodus cephalophus*, the tufted deer. Because they are recent or living, they cannot be used to establish an earlier dating for Gongwangling. The giant tapir *Megatapirus augustus*, according to Aigner (1981, p. 325), occurs in middle Middle Pleistocene assemblages and would thus be contemporary with the Zhoukoudian site. Aigner (1981, p. 289) also states that *Stegodon orientalis* survives through the late Middle Pleistocene.

The only southern form suggesting a pre-Zhoukoudian date for Gongwangling is *Nestoritherium sinensis*, an extinct three-clawed mammal that appears in Pliocene faunal assemblages. Aigner (1981, p. 289) suggested that *Nestoritherium* survived only to the Early Pleistocene in China, although she admitted this dating was open to question. Elwyn L. Simons and Peter C. Ettel (1970, p. 84) reported *Nestoritherium* at Gigantopithecus sites in South China, which they placed in the Middle Pleistocene. *Nestoritherium* also turned up in the Yenchingkuo fissures in Szechuan province, where, according to Aigner (1981, p. 288) "Kahlke distinguishes an early Cromerian-equivalent and a later Holstein-and-later component." The Cromerian interglacial is in the early Middle Pleistocene, the Holstein in the middle Middle Pleistocene, during the Zhoukoudian occupation (Table 9.2, p. 566).

All of this suggests that *Nestoritherium* could very well have appeared as a late survival in the Middle Pleistocene at Gongwangling.

At Gongwangling, *Nestoritherium*, which Zhou Mingzhen considered "unexpected . . . in the Konwanling fauna," is represented by a single "shattered and decayed" jaw fragment (Zhou, M. et al. 1965, p. 1041). Furthermore, Wu Rukang stated that in general the fossils found at Gongwangling "consist of odd, scattered bits and pieces which seem to have been thrown together after being washed down from the wooded areas of the southern slope of Kungwangling Hill" (Wu, R.

1966, p. 85). This opens up the possibility that fossils of different ages may have been incorporated into the deposits at the site.

We conclude, therefore, that the presence of certain southern Chinese animals at Gongwangling, absent at Zhoukoudian, may be a reflection of the more southerly location of Gongwangling rather than a difference in the sites' ages.

Moving on to the remaining Gongwangling taxa, we find that some represent species not present at Zhoukoudian but still living in China (Figure 9.4, II). That they are still living means they cannot justify an early date for Gongwangling.

Two other newly designated species (Figure 9.4, IV), *Leptobos brevicornis* Hu et Qi and *Dicerorhinus lantianensis*, are unique to Gongwangling and thus cannot be used for dating comparisons with other sites, such as Zhoukoudlan.

Still other Gongwangling species (Figure 9.4, V), although not present at Zhoukoudlan, are found at other middle Middle Pleistocene sites in China. Thus they establish contemporaneity with Zhoukoudlan.

Some authors have tried to use the *Leptobos* sp. fossils from Gongwangling (Figure 9.4, VI) to establish a pre-Zhoukoudlan date for the site. Such proposals are, however, open to question. *Leptobos* is an extinct ox that dates back to the Pliocene and Early Pleistocene, but according to Zhou MIngzhen et al. (1965, p. 1043) the fossil skulls with horns discovered at Gongwangling differ substantially from those normally attributed to *Leptobos*. Zhou MIngzhen et al. (1965, p. 1043) and Aigner (1981, p. 81) stated that the Gongwangling variety is comparable to a related species, *Bison priscus* = *Bison palaeosinensis*, which existed throughout the Middle and Late Pleistocene (Nilsson 1983, p. 483). Thus the so-called *Leptobos* fossils cannot securely be used to establish a pre-Zhoukoudlan date for Gongwangling.

Studying various Gongwangling faunal lists, we found that some researchers have changed some of the original species designations reported at Gongwangling (Zhou M. et al. 1965), apparently to reflect a pre-Zhoukoudlan date for the site (Figure 9.4, VII). We have already discussed the implications of Aigner's reclassification of *Ochotonoides* to *Ochotona* at Zhoukoudlan (Section 9.2.3.3).

*Ursus thibetanus kokeni*, reported by Zhou at Gongwangling (Zhou M. et al. 1965, p. 1040), was reclassified *Ursus cf. etruscus* in a later faunal list (Han and Xu 1985, p. 281). *Ursus thibetanus kokeni* is found at Zhoukoudlan Locality 1 (Han and Xu 1985, p. 282), in the middle Middle Pleistocene, while *Ursus etrusci* is an Early Pleistocene form. Zhou was quite definite that the teeth from the Gongwangling specimen were "indistinguishable from the specimens from the Kwangsi caves, which are identical with *U. thibetani* kokeni described by Matthew and Granger from the Yenchingkou fissure deposits" (Zhou, M. et al. 1965, p. 1040). Aigner and Laughlin (1973, p. 101) list the Gongwangling species as *Ursus thibetanus*, the living Asiatic black bear.

Similarly, *Acinonyx pleistocaenicus* (cheetah) reported by Zhou at Gongwangling is reclassified by some as *Sivapanthera pleistocaenicus*, an Early Pleistocene form (Han and Xu 1985, p. 271). But Zhou stated that the Gongwangling *Acinonyx* fossil was "indistinguishable" from the type species, which came from a Late Pleistocene loess formation in China (Zhou, M. et al. 1965, p. 1041). *Acinonyx* reported at Zhoukoudlan (Zhou, M. et al. 1965, p. 1045).

Having concluded our review of the Gongwangling faunal list in relation to that of Zhoukoudlan, we find that the differences between the two faunas do not point in any clear fashion to a difference in their ages.

#### 9.2.3.6 Analysis of Conflicting Opinions

We do not, however, insist that the Gongwangling Homo erectus skull is contemporaneous with Homo erectus of Zhoukoudlan Locality 1. Following our standard procedure, we simply extend the probable date range of the primitive Homo erectus skullcap found at the Gongwangling site to include the time period represented by the Zhoukoudlan occupation.

Other scientists have published reports on Lantian Homo erectus. For example, Wu Rukang (1965) said, on faunal and morphological grounds, that the Gongwangling skull and ChenJlawo Jaw are contemporaneous but are both earlier than Zhoukoudlan. As we have seen, others say Gongwangling and Chenjiawo are contemporaneous not only with each other but with Zhoukoudlan. Still others say that Gongwangling predates ChenJlawo and Zhoukoudlan. Opinions about the relative ages of the Lantian man skull and Jaw, and their temporal relation to Beijing man of Zhoukoudlan, are as diverse as they are numerous.

We have analyzed 25 reports, published between 1964 and 1986, and have graphically displayed their age estimates for the Lantian man skull and Jaw (Figure 9.5). If paleoanthropology were an exact science, we should expect to find only one point for the Jaw and skull marked on the chart, with a small area around it representing errors in measurement. But as one can see, this is not the case. Age estimates for the Jaw and skullcap are widely distributed, with several strong convergences of opinion placing both before the time of Beijing man. Nevertheless, another fairly strong convergence of opinion places (represented by points within rectangle BCEF in Figure 9.5) both the Lantianjaw and skull during the Homo erectus occupation of Zhoukoudlan.

Proposed Dates for Lantian Skull (in millions of y? b.p.)

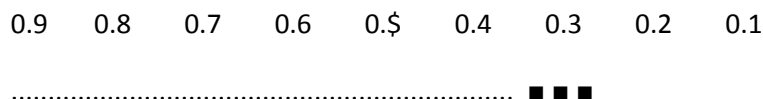


Figure 9.5. Proposed date ranges for jaw and skull of Lantian man, from 25 reports. Darker regions represent higher degrees of agreement, lighter regions lesser degrees of agreement. Dotted lines indicate the time of the Homo erectus occupation of Locality 1 at Zhoukoudlan (.23-46 millions

years b.p.). Any point on the shaded region represents an allowed date for the skull and jaw within the range of expressed opinion. For example the point marked X represents an age for the skull of 0.65 million years and for the jaw of 0.25 million years. Points along the solid diagonal line show the same age for the jaw as for the skull. Aigner (1981) and others said the Lantian Homo erectus jaw contemporary with Zhoukoudian Locality 1, whereas the Lantian Homo erectus skull, more primitive than the Zhoukoudian Homo erectus skulls, was older (points in rectangle ABDE represent this opinion). Others said both the Lantian jaw and skull were earlier than Zhoukoudian Locality 1 (points in DEGH). But, as can be seen, there is a concentrated area of positive opinion placing both the skull and jaw from Lantian within the Zhoukoudian occupation period (points in BCEF). This means that two different grades of Homo erectus may have existed contemporaneously during the period of the Zhoukoudian occupation.

This creates a problem for evolutionary theory. One is confronted with the strong possibility that very primitive Homo erectus coexisted with representatives of Homo erectus considered more advanced in the significant area of brain capacity and in other features of the skull. For evolutionists, it would thus be good to date Lantian man much earlier than Zhoukoudian Homo erectus. But as we have seen, the site stratigraphy, faunal evidence, and dates arrived at by paleomagnetic studies and other methods also allow Lantian man and Peking man to be contemporaries in the middle Middle Pleistocene.

#### 9.2.3.7 Summary

So now we have overlapping possible date ranges in the middle Middle Pleistocene for the following hominids: (1) Lantian man, a primitive Homo erectus. (2) Peking man, a more advanced Homo erectus, and (3) Tongzi man, described as Homo sapiens. We are not insisting that these beings actually coexisted. Perhaps they did, perhaps they did not. What we are insisting on is this—scientists should not propose that the hominids definitely did not coexist simply on the basis of their morphological diversity. Yet this is exactly what has happened. Scientists have arranged Chinese fossil hominids in a temporal evolutionary sequence primarily by their physical type. This methodology insures that no fossil evidence shall ever fall outside the realm of evolutionary expectations. By using morphological differences in the fossils of hominids to resolve contradictory faunal, stratigraphic, chemical, radiometric, and geomagnetic datings in harmony with a favored evolutionary sequence, paleoanthropologists have allowed their preconceptions to obscure other possibilities.

#### 9.2.4 Maba

In 1956, peasants digging for fertilizer in a cave near Maba, in Guangdong province, southern China, found a skull that was apparently from a primitive human being. Wu Rukang thought this hominid skull displayed Neanderthaloid features: "The supra-orbital tori of this skull are heavy and project markedly both forward and sidewise" (Uai 1980, p. 41). According to Chang (1962, p. 754), the data "seem to place the Ma'pa skull within the Neanderthaloid range."



Aigner (1981, pp. 65-66) stated: "On the basis of their measurements and observations, Wu and Peng conclude the remains belong to a grade of organization similar to that of the European Neanderthals. . . Coon (1969) agrees with the relative position of the hominid remains but emphasizes that it is not a Neanderthal in the classic sense of the word. He believes Maba is on the threshold of modern *Homo sapiens* and is 'mostly if not entirely Mongoloid. '"

There seems to be general agreement that the Maba skull is *Homo sapiens* (Han and Xu 1985, p. 285) with some Neanderthaloid features. Coon, it may be noted, believed that *Homo erectus* evolved directly into separate races of *Homo sapiens* in different parts of the world. Thus, according to Coon, the classic Neanderthals would have been restricted to Europe.

It is easy to see that scientists, in accordance with their evolutionary expectations, would want to place the Maba specimen in the very latest Middle Pleistocene or early Late Pleistocene, after *Homo erectus*. And in fact Wu Rukang stated: "Judging from the mammalian fauna associated with the Maba skull, its geological age is probably of late Middle Pleistocene or early Late Pleistocene" (Ola 1980, p. 41). This would give it a chronometric age of about 100,000 years.

Jia Lanpo placed the Maba skull at no earlier than the Riss-Wurm interglacial, in the late Middle Pleistocene (Ola 1980, p. 41). Aigner (1981, p. 65) also agreed: "The primitive hominid and fauna including *Stegodon* suggest a late Middle Pleistocene dating though Kahlke (1961) suggests a Wiirm-equivalent age. " The Wurm glaciation occurred in the early part of the Late Pleistocene.

Now let us take a close look at the associated fauna (Figure 11.6, p. 580), which Chang (1962, p. 754) said was "apparently a typical South China Middle Pleistocene assemblage." The assemblage included mostly fossils that could only be classified according to their genus (Han and Xu 1985, p. 285). All of these genera existed throughout the Pleistocene, from Early to Late.

A probable minimum age for the Maba site is provided by one of the identifiable species, *Elephas* (*Palaeoloxodon*) *namadicus* Falconer et Cautley. This elephant apparently became extinct in the Late Pleistocene (Belyaeva et al. 1962, p. 370). Nilsson (1983, p. 487) stated that *Palaeoloxodon namadicus* was typical of the Pleistocene interglacials, the last of which (the European Eem or Chinese Tali-Lushan) occurred about 90,000-110,000 years ago (Aigner 1981, p. 33). This a minimum age for Maba in the early Late Pleistocene.

*Palaeoloxodon namadicus* also occurs at Hoshantung cave near Kunming in Yunnan province (Aigner 1981, p. 293). This cave is thought to belong to the Holstein interglacial, which would make it the equivalent of Zhoukoudian Locality I (Aigner 1981, p. 286).

According to V. J. Maglio, an authority on elephants, *Palaeoloxodon namadicus* appears at the onset of the Middle Pleistocene, about 1 million years ago ( Nilsson 1983, p. 488). In some lists, *Palaeoloxodon namadicus* also occurs in Early Pleistocene contexts (Han and Xu 1985, p. 279). The Early Pleistocene could thus be taken as a maximum age for the Maba site.

*Stegodon*, another extinct elephant discovered at the Maba site in China, provides an age range similar to that of *Palaeoloxodon namadicus* (Aigner 1981, p. 289). So although Maba might be as

recent as the early Late Pleistocene, the faunal evidence is also consistent with an age anywhere in the Middle Pleistocene, or even the Early Pleistocene. The principal Justification for fXing the date of the Maba cave In the very latest part of the late Middle Pleistocene or In the early Late Pleistocene seems to be the morphology of the hominid remains.

W. W. Howells (1977, p. 72) stated: 'The phylogenetic position of Ma-pa suggested by Woo [Neanderthal] would accord best with the date presently assigned, I.e. early late Pleistocene at latest. Viewed as a really Neanderthal-like fossil (far removed in space from any other known), an early date would seem anomalous.' Maba provides another instance of morphological dating In order to preserve an evolutionary sequence. An early Late Pleistocene date was favored.

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Figure 9.6. Age of *Homo sapiens* cf. *neanderthalensis* at Maba, South China. Most of the mammalian fossils from Maba were identifiable only in terms of their genus, and these genera are present throughout the Pleistocene, from Early to Late. The fauna includes *Hyaena*, *Felis* t<sup>h</sup>, *Mustelidae*, *Ailuropoda*, *Ursus*, *RMnoceros*, *Tapirus*, *Sus*, *Cervus*, *Bos*, *Hystriix*, and *Lepus*. The extinct elephants *Stegodon* and *Palaeoloxodon namadicus* provide boundaries for the age range. Both *Stegodon* and *Palaeoloxodon namadicus* are known from Early Pleistocene sites in China (Han and Xu 1985, p. 279). But according to Aigner (1981, p. 289), *Stegodon* probably became extinct in the late Middle Pleistocene, possibly surviving into the Late Pleistocene (gray part of bar). *Palaeoloxodon namadicus* apparently became extinct during the last interglacial, in the late Middle Pleistocene or early Late Pleistocene (Nilsson 1983, p. 487). The probable age range for the *Homo sapiens* skull from Maba, which is said to have Neanderthaloid features, thus extends from the early Late Pleistocene to the early Early Pleistocene.

This is certainly within the realm of possibility, but a middle Middle Pleistocene date (equivalent to Zhoukoudian Locality 1) or even an Early Pleistocene date of 2 million years are also within the realm of possibility.

What conclusion may here be drawn? It would appear that at Maba we have Homo sapiens, with some Neanderthaloid features, existing within a possible date range that completely overlaps the Homo erectus presence at Zhoukoudian Locality

1 Updating our list, we now find overlapping date ranges In the middle Middle Pleistocene for: (1) primitive Homo erectus ( Lantian); (2) Homo erectus (Zhoukoudian); (3) Homo sapiens (Tongzi); and (4) Homo sapiens with Neanderthaloid features (Maba),

The possibility that Homo erectus and more advanced hominids may have coexisted in China adds new fuel to the controversy about who was really responsible for the broken brain cases of Beijing man and the presence of advanced stone tools at Zhoukoudian Locality 1, Did several hominids, of various grades of advancement, really coexist In the middle Middle Pleistocene? We do not assert this categorically, but It Is definitely within the range of possibilities suggested by the available data. In our study of the scientific literature, we have come upon no clear reason for ruling out coexistence other than the fact that the Individuals are morphologically dissimilar.

Some will certainly claim that the fact of human evolution has been so conclusively established, beyond any reasonable doubt, that It Is perfectly justifiable to engage in dating hominids by their morphology. But we believe this claim does not hold up under close scrutiny, As we have demonstrated In Chapters 2- 6, abundant evidence contradicting current Ideas about human evolution has been suppressed or forgotten. Furthermore, scientists have systematically overlooked shortcomings In the evidence that supposedly supports current evolutionary hypotheses.

If peasants digging for fertilizer In a Chinese cave had uncovered a fully human skull along with a distinctly Pliocene fauna, scientists would certainly have protested that no competent observers were present to conduct adequate stratigraphic studies. But since the Maba skull could be fitted into the standard evolutionary sequence, no one objected to Its similar mode of discovery.

#### 9.2.5 Changyang County

Even after one learns to recognize the highly questionable practice of morphological dating, one may be astonished to note how frequently It is used. In the field of human evolution research In China, It appears to be not the exception but the rule. The Homo sapiens upperjaw (Han and Xu 1985, p. 286) found by workers In 1956 at Longdong (Dragon Cave) In Changyang county, Hubei Province, South China, has provided many authorities with a welcome opportunity for unabashed morphological dating,

The upper jaw, judged Homo sapiens with some primitive features, was found In association with the typical South China Middle Pleistocene fauna Including Ailuropoda (panda) and Stegodon (extinct elephant), Jia Lanpo stated: "No human fossil had been found In association with such fauna, however, until the discovery of Changyang Man. . . . The AiluropodaStegodon fauna had been dated as Middle Pleistocene, contemporaneous with Peking Man, but new evidence puts It to be Upper Pleistocene" Uia 1980, p, 42). Study of the faunal list (Han and Xu 1985, p. 286) shows

that the only "new evidence" is the human fossil, as all the other species are representative of the Early and Middle Pleistocene, Jia concluded from the new evidence that the age of the Stegodon Ailuropoda fauna should be extended to the Late Pleistocene, but an equally valid conclusion is that Changyang Homo sapiens was contemporary with Beijing man, Evolutionary preconceptions do not, however, easily allow this.

Chang (1962, p. 749) wrote: "This fauna is generally believed to be of Middle Pleistocene age, and the scientists working on the cave suggest a late Middle Pleistocene dating, for the morphology of the maxilla shows less 'primitive' features than does that of Sinanthropus." He went on to say that the upper jaw "resembles modern man in most of its features" (Chang 1962, p. 749), It is clear that Chang's primary justification for assigning Changyang Homo sapiens a date later than Beijing Homo erectus was morphological.

Aigner (1981, p. 70) joined in with her statement: "A Middle Pleistocene age is suggested by some of the fauna with the presence of the hominid which is considered near H sapiens indicating a dating late in that period."

That so many scientists could confront the straightforward faunal evidence at Changyang without even considering the possibility that Homo sapiens coexisted in China with Homo erectus is amazing. In this regard, Sir Arthur Keith (1931, p. 256) wrote: "It has so often happened in the past that the discovery of human remains in a deposit has influenced expert opinion as to its age; the tendency has been to interpret geological evidence so that it would not clash flagrantly with the theory of [anatomically modern] man's recent origin."

Aigner (1981, p. 75) went on to state: "Associated with the Ch'angyang hominid are typical members of the Stegodon Ailuropoda fauna. Thus, it appears that the materials date to the Holstein-equivalent or to a later Interglacial, rather than to a glacial phase of the Pleistocene" (Aigner 1981, p. 75). Zhoukoudian Locality 1 is referred to the Holstein-equivalent Interglacial in the Chinese middle Middle Pleistocene. But according to Aigner (1981, p. 75), "The advanced nature of the hominid remains may exclude the early dating." Is any comment required at this point?

Interestingly, Han Defen and Xu Chunhua reported Hyaena brevirostris sinensis fossils at Changyang (Han and Xu 1985, p. 286). Aigner (1981, pp. 289, 322) said this species is not found more recently than the Holstein Interglacial, which is equivalent to the Taku-Lushan interglacial of the Chinese middle Middle Pleistocene (Table 9.2, p. 566). This should have given Aigner reason to refer Changyang Homo sapiens to the Taku-Lushan (Holstein) interglacial.

#### 9.2.6 Liujiang

In 1958, workers found human fossils in the Liujiang cave in the Guangxi Zhuang Autonomous Region of South China. These included a skull, vertebrae, ribs, pelvic bones, and a right femur. Han and Xu (1985, p. 286) said the Liujiang fossils are from Homo sapiens sapiens. Aigner (1981, p. 63) stated: "Many measurements taken fall within the range for living Mongoloids but several are

clearly in the range for 'Australoids.' ... Wu concludes that the remains belong to an early form of (modern) *H. sapiens* and to a primitive Mongoloid."

But the anatomically modern remains were found along with a typical *Stegodon* *Ailuropoda* fauna, giving a date range for the site of the entire Middle Pleistocene. The assemblage included *Hystrix* (porcupine), *Ursus* (bear), and *Sus* (hog). Since the species were not identifiable, these forms are not useful for precise dating (Han and Xu 1985, p. 286). *Ailuropoda melanoleuca fovealis* (panda) survives today and *Rhinoceros sinensis* lived until recently in China. But *Stegodon orientalis* (elephant) and *Megatapirus augustus* (giant tapir) probably did not survive past the Middle Pleistocene (Aigner 1981, pp. 289, 325), suggesting that the fossil-bearing cave deposits are at least that old. This completes the main faunal list at Liujiang, although Jia (1980, p. 46) mentions other unnamed species of cattle and deer.

"The [human] skull was found near the cave mouth no more than four meters [about 13 feet] from the find spot of the panda remains," said Jia (1980, p. 46). "The deposits there consisted of limestone, sand, and earth. The greybrown deposits were very loose and moist in marked contrast to mammalian fossil-bearing deposits found elsewhere in Guangxi, which are hard and yellowish. The types were obviously of different dating."

It seems Jia (1980, p. 47) assigned a recent date of about 40,000 years to the Liujiang *Homo sapiens* fossils simply because the stratum where they were found was of a different color and consistency than that found in other caves in the same province. This is a weak argument, considering that the faunal remains are typical of the Middle Pleistocene. One can easily imagine circumstances that might account for different sorts of cave deposits in different locations.

A more frank explanation of the recent dating is suggested by Chang (1962, p. 753): "Woo Ju-kang, who reported the finding of Liu-chiang Man, assumes that the fossil human skull together with that of *Ailuropoda* is later than Middle Pleistocene. As the human skull is definitely fossilized and of *Homo sapiens* type, it can be assumed that it is of late Pleistocene age." Wu Xinzhi and Zhang Zhenbiao stated: "although in 1958 most of the representatives of the fauna were thought to have been deposited during the Middle Pleistocene, Wu Rukang believes the hominid remains postdate this epoch" (Wu and Zhang 1985, p. 109). These statements imply that the remains of the human and panda were deposited in the cave after the other mammalian fossils. One suspects, however, that if the hominid remains had been of the *Homo erectus* type, scientists would not have felt compelled to interpret the evidence in such a fashion.

Aigner (1981, p. 64) provides an example of some very finely tuned morphological dating: "Based on the descriptions of relative primitiveness of the remains noted by Wu, Coon, and Thoma, a suggested dating of 15,000 or even 25,000 and

40,000 years ago is possible." Yet the faunal evidence clearly indicates that the possible date range for Liujiang *Homo sapiens* extends far back into the Middle Pleistocene, contemporary with Lantian man and Beijing man.

### 9.2.7 Gigantopithecus

Also found in the Pleistocene caves of South China was Gigantopithecus, a very large apelike creature. Weidenreich believed Gigantopithecus was an ancestor of Beijing man, but modern scientists do not accept this. The time range of Gigantopithecus sites in China extends from the Early Pleistocene through the Middle Pleistocene (Han and Xu 1985, pp. 279-284).

### 9.2.8 Dali

The Dali site in Shaanxi province has yielded a skull classified as *Homo sapiens* (Han and Xu 1985, p. 284) with primitive features.

The Dali fauna (Han and Xu 1985, p. 284) includes unidentifiable species of *Palaeoloxodon* (extinct elephant), *Equus* (horse), *Rhinoceros* (rhino), *Megaloceros* (large extinct deer), and *Bubalus* (water buffalo) as well as unidentifiable genera and species of the *Castoridae* (beavers). All of the genera found at Dali are represented throughout the Middle Pleistocene and earlier.

*Megaloceros pachyosteus* (Young), one of the two identifiable species from Dali, occurs at Zhoukoudian Locality 1, and the other species, *Pseudaxis grayi* (axis deer) occurs at the Lantian man sites, said to be roughly contemporaneous with Zhoukoudian Locality 1 in the middle Middle Pleistocene, if not earlier.

Some Chinese paleoanthropologists suggest a late Middle Pleistocene age for Dali (Wu, X. and Wu, M. 1985, p. 92). While this may account for the human skull, the associated fauna does not dictate such a date. Rather it suggests for Dali *Homo sapiens* a possible date range extending further back into the Middle Pleistocene, overlapping, once more, Beijing man at Zhoukoudian Locality 1.

### 9.2.9 Summary of Overlapping Date Ranges

In discussing overlapping possible date ranges, we found that Beijing man *Homo erectus* at Zhoukoudian Locality 1 may very well have lived at the same time as a variety of hominids—early *Homo sapiens* (some with Neanderthaloid features), *Homo sapiens sapiens*, and primitive *Homo erectus* (Figure 9.7).

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Figure 9.7. The probable date ranges of Chinese hominids, as determined by their accompanying mammalian faunas, are shown. Scientists have assigned dates to the hominids, within their



probable date ranges, that conform to evolutionary expectations. These dates are represented by the darker portion of each bar. For example, although the faunal date range for the Maba site extends from the Early Pleistocene to the early Late Pleistocene, scientists have used the presence of a Neanderthaloid skull to fix the date for the site in the most recent part of its date range. At Liujiang, the human fossils were given a date completely outside the faunal date range. We call this phenomenon morphological dating. But putting aside evolutionary expectations, the faunal evidence indicates that it is possible that all of the hominids were contemporary with *Homo erectus* at Zhoukoudian Locality 1 in the middle Middle Pleistocene (shaded vertical bar).

In attempting to sort out this Middle Pleistocene hominid logjam, scientists have repeatedly used the morphology of the hominid fossils to select desirable dates within the total possible faunal date ranges of the sites. In this way, they have been able to preserve an evolutionary progression of hominids. Remarkably, this artificially constructed sequence, designed to fit evolutionary expectations, is then cited as proof of the evolutionary hypothesis.

For example, as we have several times demonstrated, a *Homo sapiens* specimen with a possible date range extending from the middle Middle Pleistocene (contemporary with Beijing man) to the Late Pleistocene will be pushed toward the more recent end of the date range. One would be equally justified in selecting a middle Middle Pleistocene date within the possible date range, even though this conflicts with evolutionary expectations.

#### 9.2.10 Stone Tools and Hominid Teeth at Yuanmou (Early Early Pleistocene)

We conclude our review of fossil hominid discoveries in China with some cases of sites regarded as Early Pleistocene. At Yuanmou, in Yunnan province, southwest China, geologists found two hominid teeth (incisors). According to Chinese scientists, these were more primitive than those of Beijing man, having a more complicated lingual surface (the lingual surface is that facing the tongue). The teeth are believed to have belonged to a very primitive *Homo erectus*, a precursor of Beijing man, descended from *Australopithecus* Oia 1980, pp. 6-7).

Stone tools—three scrapers, a stone core, a flake, and a point of quartz or quartzite—were later found at Yuanmou. Published drawings (Zhang, S. 1985, p. 141) show the Yuanmou tools to be much like the European eoliths and the Oldowan industry of East Africa. Layers of cinders, containing mammalian fossils, were also found with the tools and hominid incisors. According to Jia (1980, p. 8), 'The cinders were in heaps at some spots while sparse and scattered elsewhere.' The strata yielding the incisors gave a probable paleomagnetic date of

1.7 million years within a range of 1.6-1.8 million years Uia 1980, p. 9).

There are problems with this Early Pleistocene age for Yuanmou *Homo erectus*. *Homo erectus* is thought to have evolved from *Homo habilis* in Africa about 1.5 million years ago, and then migrated elsewhere about 1.0 million years ago. *Homo habilis* is not thought to have left Africa. Implicit in Jia's age estimate for the Yuanmou hominid is a separate origin for *Homo erectus* in

China. Jia seems to require the presence in China about 2.0 million years ago of *Australopithecus* or *Homo habilis*, something forbidden by current theory.

In this regard, Lewis R. Binford and Nancy M. Stone (1986, p. 15) stated: "It should be noted that many Chinese scholars are still wedded to the idea that man evolved in Asia. This view contributes to the willingness of many to uncritically accept very early dates for Chinese sites and to explore the possibility of stone tools being found in Pliocene deposits." One could also say that because Western scholars are wedded to the idea that humans evolved in Africa they uncritically reject very early dates for hominid fossils and artifacts around the world.

As previously mentioned, one need not suppose that either Africa or Asia was a center of evolution. There is, as shown in preceding chapters, voluminous evidence, much found by professional scientists, suggesting that humans of the modern type have lived on various continents, including South America, for tens of millions of years. And, during this same period, there is also evidence for various apelike creatures, some resembling humans more than others.

A question encountered in our discussions of anomalous cultural remains (Chapters 2-5) once more arises: What justification does one have for attributing the stone tools and signs of fire at Yuanmou to primitive *Homo erectus*?

The tools and signs of fire were not found close to the *Homo erectus* teeth (Oia 1985, p. 140). Two of the three tools lay 1.5 meters (5 feet) below the level, of the teeth, and the third 1 meter (3 feet) above. The closest tool was 5 meters (about 16.4 feet) from the teeth. The others were up to 20 meters (65.6 feet) away.

Furthermore, as seen in this chapter, there is much evidence that *Homo sapiens* may have existed in China far earlier than is presently admitted, and we have already examined evidence from other parts of the world demonstrating the presence of *Homo sapiens* in the Early Pleistocene and earlier.

Aigner, representing mainline anthropological thought, reacted predictably to Jia Lanpo's suggested early dating for the Yuanmou hominid. She stated: "The hominid and faunal remains, as well as contemporaneous artifacts [occur] in level 25 at the base of the fourth stratigraphic unit, equivalent to earliest Middle Pleistocene times. A paleomagnetic age of 1.7 million years would place the strata and *H. erectus yuanmoensis* equivalent in age to the Olduvai Event and some 1 million years earlier than *H. erectus lantianensis*. Based on current reports, the remains are earliest Middle Pleistocene faunistically and stratigraphically [about 1 million years old]. The paleomagnetic date could be applicable to another stratigraphic unit (the first or second). I am reluctant at this point to accept the date as valid for the hominid teeth" (Aigner 1981, pp. 52-54). This shows how dating procedures are far from exact. Dates are subject to extensive postexperimental revision and interpretation.

In 1983, the original paleomagnetic dating of 1.7 million years for the Yuanmou site, reported by Li Pu and his associates in 1976 and reconfirmed in 1977 by Cheng and his associates, was

challenged by Liu Dongsheng and Ding Menglin (Wu, X. and Wang, L 1985, p. 35). They proposed a different explanation of the magnetostratigraphic sequence at Yuanmou.

According to Wu Xinzhi and Wang Linghong: "Liu and Ding prefer to correlate the normal polarity member at Yuanmou with the Brunhes Epoch rather than with an event of normal polarity within the Matuyama. Furthermore, they have concluded that the layer yielding the fossils of *H. erectus* is situated at the base of the Brunhes Normal Epoch strata [Figure 9.8] and therefore might not be older than 0.73 million years b.p., and possibly only 0.5-0.6 million years old" (Wu, X. and Wang, L 1985, pp. 35-36).

Figure 9.8. Cheng et al. (1977) and Li et al. (1976) established a correlation between the polarity sequence at Yuanmou (center) and the standard polarity sequence (left) that gave the hominid-bearing stratum an age of about 1.7 million years. But in 1983, Liu and Ding established a correlation between Yuanmou and the standard sequence (right) that gave the hominid-bearing stratum an age of approximately .73 million years (Wu, R. and Wang, L. 1985, p. 36).

Paleomagnetic dating is based on the assumption that the earth's magnetic field undergoes shifts in polarity, which are recorded in the magnetic properties of the strata at a site. Upon conducting the required measurements on the strata, one obtains a sequence of normal and reversed polarities, which are grouped into various epochs, such as those named in the above passage (Brunhes Normal, Matuyama Reversed, etc.). The sequence of polarities is typically displayed in a column, with periods of normal polarity shown in black and reversed polarity in white. As can be seen, in Figure 9.8,

there may be brief episodes of normal polarity in a reversed epoch and vice versa. One thus obtains a column of many black and white bars, of various thickness, representing the time of each polarity period. One may then compare the polarity sequence from a particular site, which may be quite complex, with the standard polarity sequence and its known chronometric dates. When the site polarity sequence is properly aligned with the standard sequence, one can then assign dates to the various strata at the site by comparison with the standard sequence. The problem is this: matching the polarity sequence obtained at a particular site with the standard sequence is not always easy—there is much room for interpretation, as we can see from the conflicting interpretations of the polarity sequence at Yuanmou.

In 1979, Chinese scientists using amino acid racemization methods dated animal fossils from Yuanmou to 0.80 million years. But Wu and Wang warned: "Fluctuations in average temperature on the enclosing sediments during burial may have had a significant influence on these determinations, and, as such fluctuations are not at present quantifiable, the resulting

chronometric dates must be viewed with caution" ( Wu, X. and Wang, L. 1985, pp. 36-37). The inventor of amino acid racemization dating, Bada, admitted that In certain cases the technique did not give reliable dates (Appendix 1.3.5). Wu and Wang stated that the fauna at the hominid site is Early Pleistocene, supporting the paleomagnetic date of 1.7 million years. They thus differed from Aigner, who said the fauna favored an early Middle Pleistocene age for Yuanmou.

Replying to the challenges to his proposed date of 1.7 million years, Jia Lanpo wrote: "Today, although the Lower Pleistocene age of the Yuanmou deposits has been called Into question (Liu and Ding 1983), the associated fauna still reflects a Lower Pleistocene antiquity" Qia 1985, p. 140). Reacting specifically to the more recent paleomagnetic date suggested by Liu and Ding, Jia (1985, p. 141) said: "Because of the apparent contradiction between these data and the extremely archaic mammalian fossil assemblage associated with the Yuanmou incisors, and due to the primitive morphological features of the teeth themselves. I do not share the opinions of Liu and Ding In this matter." So it appears there may exist at Yuanmou tools and signs of fire dating back to 1.7 million years ago.

#### 9.2.11 Stone Tools at Xihoudu (Early Early Pleistocene)

In 1960, Jia Lanpo investigated Early Pleistocene sand and gravel deposits at Xihoudu in northern Shanxi province. He found 3 stones with signs of percussion, and more artifacts turned up In 1961 and 1962. Jia (1980, pp. 10-11) said: "In processing the stone artefacts collected at Xihoudu, we took extra care against the possibility of misjudgement by carefully analysing the scars on the stones to see if they were caused by natural, physical, or biological factors. But none could explain them. Later, we asked the late well-known geologist Li Siguang (also known as J. S. Lee) to examine the specimens from the angle of geomechanics to see If they could have been caused by natural agencies. His answer was: 'Unlikely. Shaped by man would be more appropriate.'" Because of Early Pleistocene faunal remains, the site was dated over a million years old. According to Jia (1985, p. 139), "Preliminary palaeomagnetic data indicate an absolute age for the Xihoudu site of about 1.8 million years b.p."

Among the dozens of stone tools were cores, flakes, choppers, scrapers, and heavy triangular points. Jia (1980, p. 11) wrote: "The choppers are single or double faced, and bear marks resulting from use. ... a fragmented deer skull with both antler stumps attached was unearthed here, and on one stump transverse cuts could be observed. The cuts were most likely made by a sharp tool Instead of being caused by erosion or other natural forces. Another antler bears marks of being scraped. These mark-bearing antlers lead us to believe . . . that over a million years ago, hominids at Xihoudu were using antler and bone tools.'

Jia also found what appeared to be charred bones. This was established by visual comparison with burned bones from Zhoukoudian and by laboratory testing. But Jia (1980, p. 12) admitted: "Who

the creators of the Xihoudu culture were has not yet been Identified, for no human fossils have been found, not even so much as a tooth. From the antiquity of the site we may infer that the genus probably belongs to the Australopithecinae."

Aigner (1981, pp. 183-184), as one might well Imagine, disagreed: "Despite the strong support for Lower Pleistocene human activity in north China claimed for Hsihoutu [Xihoudu], I am reluctant to accept unequivocally the materials at this time. ... If Hsihoutu is verified, then humans occupied the north of China some 1,000,000 years ago and utilized fire. This would call into question some of our current assumptions about both the course of human evolution and the adaptational capabilities of early hominids. "

If one could, however, become detached from current assumptions, Interesting possibilities open up. Considering all the evidence gathered around the world over the past century or so, and not just the carefully selected evidence used to support present evolutionary views of human origins, one may Infer something different than did Jia regarding Xihoudu. It is possible that some other hominid, perhaps Homo sapiens, might have been responsible for the cultural remains at Xihoudu. In fact, Homo sapiens is a more likely explanation for the presence of stone tools, bone tools, and fire than an australopithecine.

#### 9.2.12 Concluding Words on China

This ends our review of finds in China. It appears that age determinations of fossil hominids have been distorted by morphological dating. When these ages are adjusted to reflect reasonable faunal date ranges, the total evidence fails to exclusively support an evolutionary hypothesis. Rather, the evidence appears also consistent with the proposal that anatomically modern human beings have coexisted with a variety of humanlike creatures throughout the Pleistocene.

#### Living Ape-Men?

In examining the fossil hominids of China (Chapter 9), we found signs that humans may have coexisted with more apelike hominids throughout the Pleistocene. In this chapter, we suggest that humans and ape-man-like creatures continue to coexist.

Over the past hundred or so years, researchers have accumulated substantial evidence that creatures resembling Neanderthals, Homo erectus, and the australopithecines even now roam wilderness areas of the world.

The existence of living ape-men, if admitted by scientists, offers new ways to Interpret ambiguous paleoanthropological evidence. Hominid fossils once thought to be from the Middle Pleistocene or older periods may in fact be quite recent. The existence of living ape-men also calls into question the reliability of the scientific information processing system in zoology and anthropology.

#### 10.1 Hard Evidence Is Hard To Find

In 1775, Carl Linnaeus, the founder of the modern system of biological classification, listed three existing human species: *Homo sapiens*, *Homo troglodytes* (cave man), and *Homo feras* (wild man). Although Linnaeus knew the latter two species only from travelers' reports and other secondary sources of information, he still included them within his *Systema Naturae* (Shackley 1983, p. 10).

Since 1775, much more evidence for the existence of living apelike wildmen has come to light. Professional scientists have (1) observed wildmen in natural surroundings, (2) observed live captured specimens, (3) observed dead specimens, and (4) collected physical evidence for wildmen, including hundreds of footprints. They have also interviewed nonscientist informants and investigated the vast amount of wildman lore contained in ancient literatures and traditions.

Despite this, no zoo or museum in a civilized nation has in its collection a wildman specimen, alive or dead. Many will say that all the wildman evidence mentioned above exists simply in reports, and that reports alone, even those given by scientists, are not sufficient to establish the existence of wildmen. Hard evidence, available now, to anyone who wants to see it and touch it, is required.

But what is the real status of hard evidence? Can a physical object in and of itself confirm a certain idea about some aspect of human origins? The answer to that question is no. In paleoanthropology, as in many areas of science, evidence exists primarily in the form of reports.

The most important feature of an artifact or a fossil hominid bone, as far as paleoanthropologists are concerned, is its age. As we show in Appendix 1 radiometric and chemical methods are not very reliable age indicators. Therefore the best age indicator is stratigraphic position. Once a bone or artifact is taken out of the ground, evidence of its original stratigraphic position lies principally in the reports of its discovery.

For example, one afternoon in the early 1970s, Donald Johanson, the discoverer of 'Lucy' (the most famous specimen of *Australopithecus afarensis*), found some fossilized bones lying on the surface, near his base camp in the Afar region of Ethiopia. At the very moment his fingers grasped one of those bones, lying upon Pliocene sediments, Johanson was, one might say, in touch with some hard evidence. But Johanson's discovery of those bones had no real scientific meaning until it was reported to other scientists. And from that time on, the discovery has existed, as far as the world of science is concerned, only in reports.

Were the bones discovered in the exact manner described in the reports? The answer to that question depends upon how much faith one places in the reporter and the reporting process.

One might take some comfort in the fact that the "actual" bones—the real hard evidence—are still present in a museum in Ethiopia. Of course, it is not so easy to obtain direct access to rare fossil specimens. But say you did have the proper scientific credentials and were able to go to Ethiopia and inspect the actual bones of Lucy. How would you know for certain that those were the bones picked up years ago by Johanson? You might compare them with the photographs and descriptions in the published reports. But here we go again—everything depends upon how much faith one places in the reports.

In some cases, the bones themselves are not available for inspection. For example, during World War II almost the whole collection of Beijing man (*Homo erectus*) fossils was lost during the Japanese occupation of China. The Beijing man fossils now exist only in the form of old written reports, photographs, and casts. And no one doubts that the originals did in fact exist.

But what about reports by scientists who claim they saw and examined dead specimens of wildmen, the corpses of which were not preserved? Most scientists will grant no credibility at all to such reports.

Here and in the case of Beijing man, the actual physical evidence is no longer available for inspection. Yet in one case the reports are believed, and in the other they are not. Why? We propose that reports about evidence conforming to the standard view of human evolution generally receive greater credibility than reports about nonconforming evidence. Thus deeply held beliefs, rather than purely objective standards, may become the determining factor in the acceptance and rejection of reports about controversial evidence.

## 10.2 Cryptozoology

For some researchers, the study of creatures such as wildmen comes under the heading of a genuine branch of science called cryptozoology. Cryptozoology, a term coined by the French zoologist Bernard Heuvelmans, refers to the scientific investigation of species whose existence has been reported but not fully documented. The Greek word *kryptos* means "hidden," so cryptozoology literally means "the study of hidden animals." There exists an International Society of Cryptozoology, the board of directors of which includes professional biologists, zoologists, and paleontologists from universities and museums around the world. The purpose of the society, as stated in its journal *Cryptozoology*, is "the investigation, analysis, publication, and discussion of all matters related to animals of unexpected form or size, or unexpected occurrence in time or space." A typical issue of *Cryptozoology* usually contains one or more articles by scientists on the topic of wildmen.

Is it really possible that there could be an unknown species of hominid on this planet? Many will find this hard to believe for two reasons. They suppose that every inch of the earth has been quite thoroughly explored. And they also suppose that scientists possess a complete inventory of the earth's living animal species. Both suppositions are incorrect.

First, even in countries such as the United States, there remain vast unpopulated and little-traveled areas. In particular, the northwestern United States still has large regions of densely forested, mountainous terrain which, although mapped from the air, are rarely penetrated by humans on the ground.

Second, a surprising number of new species of animals are still being found each year—about 5,000 according to a conservative estimate (Heuvelmans 1983, pp. 19-20). As might be suspected, the great majority of these, some 4,000, are insects. Yet Heuvelmans (1983, p. 21) noted: "Quite recently, in the mid 1970's, there were discovered each year, around 112 new species of fish, 18



new species of reptiles, about ten new species of amphibians, the same number of mammals, and 3 or 4 new species of birds."

Most of the mammals were small, and this might lead one to doubt that a large mammal, such as a wildman, might someday enter the list of living species. But the twentieth century has seen the discovery of many large species, some "known from native reports which were initially disbelieved" (Shackley 1983, p. 166).

The largest of the bears, the Kodiak bear, was unknown to science until 1899. The largest rhinoceros, Cotton's white rhino, was discovered in 1900. The mountain gorilla, the largest member of the ape family, turned up in 1901. The largest lizard, the Komodo dragon, was first captured in 1912. In 1975, the largest known peccary, or wild hog, *Catagonus wagneri*, was discovered in Paraguay. This animal was previously known only by Pleistocene fossils (Wetzel et al. 1975; Heuvelmans 1983, p. 12). In 1976, a large and entirely new species of shark, 4.5 meters (almost 15 feet) long and weighing over 700 kilograms (over 1,500 pounds), was caught by a U.S. Navy ship in the ocean waters off Hawaii (L. Taylor et al. 1983). So it is not completely outside the realm of possibility that science might someday come to fully accept the existence of wildmen, which may prove to be previously unknown types of hominids or primates, or surviving representatives of fossil hominids such as the australopithecines, *Homo erectus*, or the Neanderthals. It would not be the first time that science has found examples of "living fossils."

### 10.3 European Wildmen

Many art objects of the Greeks, Romans, Carthaginians, and Etruscans bear images of semi-human creatures resembling wildmen. For example, in the Museum of Prehistory in Rome, there is an Etruscan silver bowl on which may be seen, among human hunters on horses, the figure of a large, ape-man-like creature (Wendt 1972, p. 15). Such imagery is, of course, subject to varying interpretations. The Russian scientist Boris Porshnev believed the humanlike creatures represented survivals of prehuman hominids. But British anthropologist Myra Shackley, who said wildmen may in fact exist in some parts of the world, asserted that the figures on classical Graeco-Roman art objects represent purely mythological beings such as satyrs (1983, pp. 18-19).

The satyr is a stylized and very recognizable figure, part human and part animal, occurring mainly on Greek vases. Typically, satyrs have horselike tails and are shown engaged in some kind of sporting or licentious behavior, perhaps connected with the cult of Dionysus. The hairy humanlike figure depicted on the Etruscan silver bowl, however, is shown not with revelers but in the midst of a hunting party of well-armed humans mounted on horses. The creature has no satyr's tail and appears to be carrying a crude club in one hand and a large stone, raised threateningly above his head, in the other.

During the Middle Ages, wildmen continued to be depicted in European art and architecture. A page from Queen Mary's Psalter, composed in the fourteenth century, shows a very realistically depicted hairy wildman being attacked by a pack of dogs (Shackley 1983, p. 25). Wildmen were thought to live in caves and forests, where they subsisted on berries and roots. They were not

considered ordinary humans. Instead, they were said to be members of the animal kingdom, unable to speak or comprehend the existence of God.

#### 10.4 Northwestern North America

For centuries, the Indians of the northwestern United States and western Canada have believed in the reality of wildmen, known by various names, the most familiar of these being Sasquatch. In 1792, the Spanish botanist-naturalist Jose Mariano Moziño, in describing the Indians of Nootka Sound on Vancouver Island, Canada, stated (1970, pp. 27-28): "I do not know what to say about Matlox, inhabitant of the mountainous district, of whom all have an unbelievable terror. They imagine his body as very monstrous, all covered with stiff black bristles: a head similar to a human one, but with much greater, sharper and stronger fangs than those of the bear: extremely long arms: and toes and fingers armed with long curved claws. His shouts alone (they say) force those who hear them to the ground, and any unfortunate body he slaps is broken into a thousand pieces."

In 1784, the London Times printed a report that Indians at Lake of the Woods, Manitoba, had captured a "huge, manlike, hair-covered" creature (Shackley 1983, p. 35).

Describing the Spokane Indians of the Pacific Northwest, Elkanah Walker, a missionary who lived among them for 9 years, wrote in 1840: "They believe in the existence of a race of giants which inhabit a certain mountain, off to the west of us. This mountain is covered with perpetual snow. They inhabit its top. . . . They hunt and do all their work in the night. They are men stealers. They come to people's lodges in the night, when the people are asleep and take them and put them under their skins and take them to their place of abode without their even awakening. . . . They say their track is about a foot and a half long. . . . They frequently come in the night and steal their salmon from their nets and eat them raw. If the people are awake they always know when they are coming very nearby the smell which is most intolerable" (Drury 1976, pp. 122-123).

Indians from the Columbia River region of the northwestern United States produced rock carvings that resembled the heads of apes. Anthropologist Grover Krantz (1982, p. 97) showed photographs of the heads to a number of scientists and noted: "Zoologists who did not know their source unanimously declared them to be representative of nonhuman, higher primates; those who knew the source insisted they must be something else!" Whatever the carvings may actually represent, Krantz's findings are significant. Preconceptions seem to determine what scientists are prepared to see, and one thing most scientists are definitely not prepared to see is apelike creatures in the American Northwest.

U.S. President Theodore Roosevelt included an intriguing wildman report in his book *The Wilderness Hunter* (1906, pp. 255 -261). The incident took place in the Bitterroot Mountains, between Idaho and Montana. Wildman reports still come out of this region.

In the early to middle 1800s, a trapper named Bauman and his partner were exploring a particularly wild and lonely pass, through which ran a stream said to have many beaver. The two

trappers set up camp late one afternoon and went out to explore for a couple of hours. Returning at dusk, they found that something had scattered their belongings around and had in "sheer wantonness" destroyed their lean-to. They rebuilt their lean-to, made supper, and then studied the footprints left by the beast. They noticed, quite to their surprise, that the malicious intruder had apparently walked off on two feet (bears usually go on all fours). This was a bit unsettling, but at last they managed to fall asleep under the lean-to.

Around midnight, they were awakened by some noise and saw a huge body standing at the opening of the lean-to. Their nostrils were assailed by a "strong wild-beast odor." Bauman fired a couple of shots at the creature. They figured he did not hit it, because they heard it move away through the woods.

The next day, the creature again ravaged the camp while Bauman and his partner were checking their traps. They found a trail of prints in the soft dirt, and these confirmed once more that their assailant, unlike a bear, had walked off on just two feet. That evening, they set up a roaring fire, which they kept going all night. Around midnight, the creature was heard moving through the woods, and it several times "uttered a harsh grating, long-drawn moan."

The following morning, Bauman and his partner decided to leave, but first they wanted to check their traps. As they moved through the forest, they sensed they were being followed. Roosevelt (1906, p. 259) said, "In the high, bright sunlight their fears seemed absurd to the two armed men, accustomed as they were, through long years of lonely wandering in the wilderness, to face every kind of danger from man, brute, or element." Bauman's partner returned to the camp before he did. When Bauman finally arrived, he found his partner dead. Said Roosevelt (1906, p. 260): "The footprints of the unknown beast-creature, printed deep in the soft soil, told the whole story."

Roosevelt had some thoughts about the episode. He wrote of Bauman: "he was of German ancestry, and in childhood had doubtless been saturated with all kinds of ghost and goblin lore, so that many fearsome superstitions were latent in his mind: besides he knew well the stories told by Indian medicine men in their winter camps, of the snow-walkers, and the spectres, and the formless evil beings that haunt the forest depths, and dog and waylay the lonely wanderer who after nightfall passes through the regions where they lurk: and it may be that when overcome by the horror of the fate that befell his friend, and when oppressed by the awful dread of the unknown, he grew to attribute, both at the time and still more in remembrance, weird and elfin traits to what was merely some abnormally wicked and cunning wild beast: but whether this was so or not, no man can say" (Roosevelt 1906, pp. 254 - 255).

Roosevelt's psychological explanation of Bauman's tale is typical of the reasoning presently applied by those who have no desire to add wildmen to the North American faunal list. In this case, because of the vagueness of the account, it is not easy to offer counterarguments. Bauman did not get a clear look at the creature. But one might wonder what known large North America mammal typically prowls about on two feet rather than four? Bears will stand for a short time on two legs, but are not known to move any great distance in bipedal fashion. If the creature really was a bear, Bauman, an experienced backwoodsman, should have been able to identify it as such

from the footprints, which he closely inspected. But he did not. What sort of animal could have made the footprints? Roosevelt (1906, p. 261) said that Bauman believed "the creature with which he had to deal was something either half-human or half devil, some great goblin-beast. "

Taken on its own, the Bauman story is not very impressive as evidence for the existence of wildmen in North America, but when considered along with the more substantive reports it acquires greater significance.

On July 4, 1884, the *Colombian*, a newspaper published in Victoria, British Columbia, carried a story titled: "What is it? A strange creature captured above Yale. A British Columbian Gorilla." According to the article, Ned Austin, a railway engineer, spotted a humanlike creature ahead of him on the tracks, blew the whistle, and stopped. The creature darted up the side of a hill, with several railway employees in pursuit. After capturing the animal, described as "half man and half beast" (Shackley 1983, p. 35), the railway employees turned him over to Mr. George Tilbury.

The *Colombian* reported: "Jacko," as the creature has been called by his capturers, is something of the gorilla type, standing about four feet seven inches in height and weighing 127 pounds. He has long, black, strong hair and resembles a human being with one exception, his entire body, excepting his hands (or paws) and feet is covered with glossy hair about one inch long. His forearm is much longer than a man's forearm, and he possesses extraordinary strength" (Shackley 1983, p. 35).

The paper added (Shackley 1983, p. 36): "Mr. Thos. White and Mr. Gouin, C.E., as well as Mr. Major, who kept a small store about half a mile west of the tunnel during the past two years, have mentioned seeing a curious creature at different points between Camps 13 and 17, but no attention was paid to their remarks as people came to the conclusion that they had seen either a bear or a stray Indian dog. Who can unravel the mystery that surrounds Jacko? Does he belong to a species hitherto unknown in this part of the continent?"

That the creature was not a gorilla seems clear—its weight was too small. Some might suppose that Jacko was a chimpanzee. But this idea was apparently considered and rejected by persons who were familiar with Jacko. Sanderson (1961, p. 27) mentioned "a comment made in another paper shortly after the original story was published, and which asked . . . how anybody could suggest that this 'Jacko' could have been a chimpanzee that had escaped from a circus." Was the whole story perhaps a hoax? Myra Shackley thought not. She

noted: "The newspaper account of Jacko was subsequently confirmed by an old man, August Castle, who was a child in the town at the time. The fate of the captive is not known, although some said that he (accompanied by Mr. Tilbury) was shipped east by rail in a cage on the way to be exhibited in a sideshow, but died in transit" (Shackley 1983, p. 36).

Furthermore, there were additional reports of creatures like Jacko from the same region. Zoologist Ivan Sanderson (1961, p. 29) said about Jacko in one of his collections of wildman evidence: "one of his species had been reported from the same area by Mr. Alexander Caulfield Anderson, a well-

known explorer and an executive of the Hudson's Bay Company, who was doing a 'survey' of the newly opened territory and seeking a feasible trade route through it for his company. He reported Just such hairy humanoids as having hurled rocks down upon him and his surveying party from more than one slope. That was in 1864."

In 1901, Mike King, a well-known lumberman, was working in an isolated region in northern Vancouver Island. He had to work alone. His native American employees refused to accompany him, fearing that the dreaded wildman of the woods lived there. Once, as King came over a ridge, he spotted a large humanlike creature covered with reddish brown fur. On the bank of a creek, the creature was washing some roots and placing them in two orderly piles beside him. The creature then left, running like a human being. King said: "His arms were peculiarly long and used freely in climbing and bush-running." Footprints observed by King were distinctly human, except for the "phenomenally long and spreading toes" (Sanderson 1961, pp. 34-35).

In 1941, several members of the Chapman family encountered a wildman at Ruby Creek, British Columbia. In 1959, Ivan Sanderson interviewed the Chapmans, who were native Americans, about what happened. On a sunny summer afternoon, Mrs. Chapman's oldest son alerted her to the presence of a large animal coming down out of the woods near their home. At first, she thought it was a large bear. But then, much to her horror, she saw that it was a gigantic man covered all over with yellow-brown hair. The hair was about 4 inches long. The creature moved directly towards the house, and Mrs. Chapman rounded up her three children and fled downstream to the village.

She estimated that the creature was about 7.5 feet tall. It had a relatively small head and a short, thick neck—practically no neck at all. Its body was completely human in shape, except the chest was immensely thick and the arms unusually long. Its shoulders were extremely wide. The naked regions of its face and its hands were much darker than the hair and appeared to be nearly black.

When Mr. Chapman returned home, a couple of hours after his wife had fled, he saw huge humanlike footprints all around the house. He was greatly alarmed, because, like almost all the native Americans of the Pacific Northwest, he had heard from childhood about the "big wild men of the mountains" (Sanderson 1961, p. 68). For the next week, giant humanlike footprints were found every day.

Moreover, said Sanderson, the Chapmans described the 'strange gurgling whistle' emitted by the creature. According to Sanderson, this cry seemed identical to that heard by other persons in connection with similar creatures elsewhere in the Pacific Northwest.

In October of 1955, Mr. William Roe, who had spent much of his life hunting wild animals and observing their habits, encountered a wildman (Green 1978, pp. 53-56). The incident took place near a little town called Tete Jaune Cache in British Columbia. One day, said Roe in a sworn statement, he climbed up Mica Mountain to an old deserted mine and saw, at a distance of about 75 yards, what he first took to be a bear. When the creature stepped out into a clearing, Roe realized that it was something different: "My first impression was of a huge man, about six feet tall, almost three feet wide, and probably weighing somewhere near three hundred pounds. It was

covered from head to foot with dark brown silver-tipped hair. But as it came closer I saw by its breasts that it was female. And yet, its torso was not curved like a female's. Its broad frame was straight from shoulder to hip. Its arms were much thicker than a man's arms, and longer, reaching almost to its knees. Its feet were broader proportionately than a man's, about five inches wide at the front and tapering to much thinner heels. When it walked it placed the heel of its foot down first, and I could see the grey-brown skin or hide on the soles of its feet" (Green 1978, pp. 53-55).

"It came to the edge of the bush I was hiding in, within twenty feet of me, and squatted down on its haunches," said Roe. "Reaching out its hands it pulled the branches of bushes toward it and stripped the leaves with its teeth. Its lips curled flexibly around the leaves as it ate. I was close enough to see that its teeth were white and even. ... The head was higher at the back than at the front. The nose was broad and flat. The lips and chin protruded farther than its nose. But the hair that covered it, leaving bare only the parts of its face around the mouth, nose and ears, made it resemble an animal as much as a human. None of this hair, even on the back of its head, was longer than an inch, and that on its face was much shorter. Its ears were shaped like a human's ears. But its eyes were small and black like a bear's. And its neck also was unhuman. Thicker and shorter than any man's I had ever seen."

After a few minutes the creature became aware of Roe's presence in the bushes and departed. It was not exactly afraid but was apparently unwilling to have contact with a human being (Green 1978, p. 55).

In 1967, in the Bluff Creek region of Northern California, Roger Patterson and Bob Gimlin managed to shoot a short color film of a female Sasquatch. They also made casts of her footprints. These prints, which were 14 inches long, were 5.5 inches wide at the ball and 4 inches wide at the heel (Green 1978, p. 118).

Several opinions have been expressed about the film. While some authorities have said it is an outright fake, others have said they think it provides good evidence in favor of the reality of the Sasquatch. Mixed opinions have also been put forward. Dr. D. W. Grieve, an anatomist specializing in human walking, studied the film and had this to say: "My subjective impressions have oscillated between total acceptance of the Sasquatch on the grounds that the film would be difficult to fake, to one of irrational rejection based on an emotional response to the possibility that the Sasquatch actually exists. This seems worth stating because others have reacted similarly to the film. The possibility of a very clever fake cannot be ruled out on the evidence of the film. A man could have sufficient height and suitable proportions to mimic the longitudinal dimensions of the Sasquatch. The shoulder breadth however would be difficult to achieve without giving an unnatural appearance to the arm swing and shoulder contours" (Napier 1973, p. 220).

From his study of the film, Grieve estimated the length of the Sasquatch's foot to be 13.3 inches, which is consistent with the length of 14 inches reported for the footprints. John R. Napier (1973), however, believed that a 14-inch foot length was not consistent with the estimated body height of 6 feet 5 inches. In his computations, Napier, a respected British anatomist, used the ratio of foot

length to body height in modern humans. He did not, however, explain why the physical proportions of the Sasquatch must be the same as those of modern humans.

Anthropologist Myra Shackley of the University of Leicester observed (1983, p. 43) that the majority view seems to be "that the film could be a hoax, but if so an incredibly clever one." Reacting similarly, Napier (1973, p. 95) stated: 'Perhaps it was a man dressed up in a monkey-skin; if so it was a brilliantly executed hoax and the unknown perpetrator will take his place with the great hoaxers of the world.' But then he added: "Perhaps it was the first film of a new type of hominid, quite unknown to science" (Napier 1973, p. 95). Concerning the charge of incredibly clever hoaxing, this explanation could be used to dismiss almost any kind of scientific evidence whatsoever. All one has to do is posit a sufficiently expert hoaxer. Therefore the hoax hypothesis should be applied only when there is actual evidence of hoaxing, as at Piltdown, for example. Ideally, one should be able to produce the hoaxer. Furthermore, even a demonstrated case of hoaxing cannot be used to dismiss entire categories of similar evidence.

## 10.5 More Footprints

As far as Sasquatch footprints are concerned, independent witnesses have examined and reported hundreds of sets, and of these more than 100 have been preserved in photographs and casts (Green 1978, p. 348). Napier stated: "if any of them is real then as scientists we have a lot to explain. Among other things we shall have to re-write the story of human evolution . . . and we shall have to admit that there are still major mysteries to be solved in the world we thought we knew so well" (1973, p. 204).

Critics, however, assert that all these footprints have been faked. Undoubtedly, some footprints have been faked, a fact the staunchest supporters of the Sasquatch will readily admit. But could every single one of them be a hoax? Napier (1973, p. 124) stated that if all the prints are fakes "then we must be prepared to accept the existence of a conspiracy of Mafia-like ramifications with cells in practically every major township from San Francisco to Vancouver."

Grover S. Krantz, an anthropologist at Washington State University, was initially skeptical of Sasquatch reports. In order to determine whether or not the creature really existed, Krantz studied in detail some prints found in 1970 in northeast Washington State. In reconstructing the skeletal structure of the foot from the print, he noted that the ankle was positioned more forward than in a human foot. Taking into consideration the reported height and weight of an adult Sasquatch, Krantz, using his knowledge of physical anthropology, calculated just how far forward the ankle would have to be set. Returning to the prints, he found that the position of the ankle exactly matched his theoretical calculations. 'That's when I decided the thing is real,' said Krantz. "There is no way a faker could have known how far forward to set that ankle. It took me a couple of months to work it out with the casts in hand, so you have to figure how much smarter a faker would've had to be" (Huyghe 1984, p. 94).

Krantz (1983) and wildman expert John Green (1978, pp. 349-356) have written extensive reports on the North American footprint evidence. Typically the prints are 14 to 18 inches long and 5 to 9 inches wide, giving a surface roughly 3 to 4 times larger than that of an average human foot. Hence the popular name Bigfoot. To make a Sasquatch footprint as deep as an average human footprint would require a weight 3 to 4 times greater than that of an average-sized man. In all cases, however, whether the prints are in snow, mud, dirt, or wet sand, the Sasquatch prints are much deeper than those made by a man walking right next to them in the same material. Thus a weight of more than 3 or 4 times that of a man is required to make the Sasquatch prints. Green, wearing large fake feet and carrying 250 pounds on his back (for a total of 450 pounds), was unable to make a deep enough impression in firm wet sand. Moreover, Green's fake feet were only 14.5 inches long, small for a Sasquatch. Larger feet would have produced impressions of even smaller depth in the sand. Krantz (1983) estimated that to make typical Sasquatch prints a total weight of at least 700 pounds is required. Thus a 200-pound man would have to be carrying at least 500 pounds to make a good print.

But that is only the beginning. There are reports of series of prints extending from three-quarters of a mile up to several miles, in deserted regions far away from the nearest roads. The stride length of a Sasquatch varies from 4 to 6 feet (the stride length of an average man is about 3 feet). Try walking a mile with at least 500 pounds on your back and taking strides 5 feet long.

"A footprint machine, a kind of mechanical stamp, has been suggested," stated Napier (1973, p. 125), "but an apparatus capable of delivering a thrust of approximately 800 lb per square foot that can be manhandled over rough and mountainous country puts a strain on one's credulity." In addition, said Napier (1973, p. 125), careful studies of Sasquatch prints by Dr. Maurice Tripp, a geologist, revealed that impact ridges, which a footprint machine would be expected to leave, were not present.

Some of the reported series of tracks were in fresh snow, enabling observers to verify that no other marks were made at the ground level by some machine paralleling the prints. In several cases, the Sasquatch footprints indicated the maker strode over large logs, which a human of normal size could not have gotten over without disturbing the fresh snow clearly visible on their tops. Sometimes the Sasquatch prints went up or down embankments. In some cases, the distance between the toes of the footprints varied from one print to the next in a single series of prints. This means that besides all the other problems facing a hoaxer, he would have had to incorporate moving parts into his artificial feet.

Furthermore, in order to insure that some of his fake prints would be found, any hoaxer would probably have had to make more trails of footprints than were actually discovered—and that means a lot of work,

What about a device operated from a hovering craft? Such a device would undoubtedly be very expensive. A helicopter alone is not a cheap item, and a custom device for making the footprints would also cost a bit. Also, footprints have been found at the same time that a Sasquatch was actually seen or soon thereafter, as, for example, in the Patterson sighting in 1967 and the



Chapman sighting in 1941, In other cases, people sleeping at campsites or work sites have gotten up in the morning and found newly made footprints nearby, In one case, the footprints went right alongside a man's camper truck (Green 1978, p, 352), If the prints had been made by a stamping machine, operated on the ground or from a helicopter, the people reporting the prints almost certainly would have been awakened,

In conclusion, critics have failed to explain all the footprints as the work of hoaxers, It would seem, therefore, that the footprints argue strongly for the reality of the Sasquatch, as demonstrated by the following case,

On June 10, 1982, Paul Freeman, a U.S. Forest Service patrolman tracking elk in the Walla Walla district of Washington State, observed a hairy biped around 8 feet tall, standing about 60 yards from him, After 30 seconds, the large animal walked away (Huyghe 1984, p. 94), Krantz (1983) studied casts of the creature's footprints and found dermal ridges, sweat pores, and other features in the proper places for large primate feet. Detailed skin impressions on the side walls of the prints indicated the presence of a flexible sole pad,

Krantz solicited opinions from other scholars and fingerprint experts, Tatyana Gladkova, a specialist in dermatoglyphics from the USSR Institute of Anthropology, said: "I see dermal ridges of the arch type distally directed, I see sweat pores, If it's a fake, it's a brilliant fake, on the level of counterfeiting, and by someone well versed in dermatoglyphics" (Krantz 1983, p, 78),

Douglas M, Monsoor, a master police fingerprint examiner from Lakewood, Colorado, stated: "I see the presence of ridge structure in these casts, which, in my examination, appears consistent with that type of ridge structure you would find in a human, Under magnification, they evidence all the minute characteristics similar to human dermal ridges, ... If hoaxing were involved, I can conceive of no way in which it could have been done, They appear to be casts of impressions of a primate foot—of a creature different from any of which I am aware" (Krantz 1983, p, 79),

Ten years earlier, John R. Napier (1973, p, 125) declared that he found the prints he himself studied "biologically convincing," Napier (1973, pp, 204 -205) stated: "The evidence that I have examined persuades me that some of the tracks are real, and that they are manlike in form, . . , But when the size of the tracks is taken into account, and the conclusion is reached that the man-like creature in question has a stature of at least 8 ft and weighs upward of 800 lb. , the mind starts to boggle at such a preposterous idea, The vision of such creatures stomping barefoot through the forests of north-west America, unknown to science, is beyond common sense, Yet reason argues this is the case. . . , Thomas Huxley's aphorism that 'logical consequences are the scarecrows of fools and the beacons of wise men' puts steel into my soul. , , , I am convinced that the Sasquatch exists, " Coming from a scientist who headed the primate program at the Smithsonian Institution, this is a very strong statement. Napier is also one of Great Britain's leading experts in the field of primate anatomy, His name will come up often in our discussion of hominid fossil discoveries in Africa,

In the face of much good evidence, why do almost all anthropologists and zoologists remain silent about Sasquatch? Krantz observed, "They are scared for their reputations and their Jobs" (Huyghe 1984, p, 96), Napier similarly noted: "One of the problems, perhaps the greatest problem, in investigating Sasquatch sightings is the suspicion with which people who claim to have seen a Sasquatch are treated by their neighbours and employers, To admit such an experience is, in some areas, to risk personal reputation, social status and professional credibility" (1973, p, 88), In particular, he told of "the case of a highly qualified oil company geologist who told his story but insisted that his name should not be mentioned for fear of dismissal by his company" (Napier 1973, p, 88), In this regard, Roderick Sprague, an anthropologist from the University of Idaho, said of Krantz: "It is Krantz's willingness to openly investigate the unknown that has cost him the respect of many colleagues as well as timely academic promotion" (1986, p, 103),

The majority of the Sasquatch reports come from the northwestern United States and British Columbia, However, there are also numerous reports from the eastern parts of the United States and Canada, For example, Green (1978) stated that there were, as of 1977, 11 reports from New York, more than 24 reports from Pennsylvania, 19 reports from Ohio, 18 from Michigan, 9 from Tennessee, more than 36 from New Jersey, 19 from Arkansas, 23 from Illinois, 30 from Texas, and 104 (maybe more) from Florida, Moving out west, Green recorded 74 reports from Montana, 32 from Idaho, 176 from Oregon, 281 from Washington, 225 from British Columbia, and 343 from California,

The volume of reports from the Pacific Northwest caused John R. Napier (1973, p. 96) to state: "The North American Bigfoot or Sasquatch has a lot going for it. It is impossible on the evidence ... to say that it does not exist. Too many people claim to have seen it or at least to have seen footprints to dismiss its reality out of hand. To suggest that hundreds of people at worst are lying or, at best, deluding themselves is neither proper nor realistic."

"One is forced to conclude," said Napier. "that a man-like life-form of gigantic proportions is living at the present time in the wild areas of the northwestern United States and British Columbia. . . . That such a creature should be alive and kicking in our midst, unrecognized and unclassifiable, is a profound blow to the credibility of modern anthropology" (Green 1978, p. 12). It might also be said that the existence of living ape-men in North America, from Washington and Oregon to Florida and New Jersey, is a blow not only to anthropology, but to biology, zoology, and science in general.

## 10.6 Central And South America

Apelike wildmen are also reported in southern Mexico and throughout Central America. In White Indians of Darien. Richard Oglesby Marsh said a man told him that in 1920 he had killed a humanlike animal in Central America. In Buckskin Joe. Edward Jonathan Hoyt reported an encounter he had in 1898 in Honduras. A Large. apelike creature. about 5 feet tall, crawled over the end of his bunk. Hoyt killed the animal. which resembled a human (Green 1978, p. 133).

From southern Mexico's tropical forests come accounts of beings called the Sisimite. Wendell Skousen, a geologist, said the people of Cubulco in Baja Verapaz reported: 'There live in the mountains very big, wild men, completely clothed in short, thick, brown, hairy fur, with no necks, small eyes, long arms and huge hands. They leave footprints twice the length of a man's.' Several persons said that they had been chased down mountainsides by the Sisimite. Skousen thought the creatures, which were said to travel sometimes on two legs and sometimes on all four, may have been bears. However, upon questioning the natives carefully, he wrote: "it looked like a bear, but it wasn't from the description that they gave—no conspicuous ears, no 'snout'" (Sanderson 1961, p. 159). Similar creatures are reported in Guatemala, where, it has been said, they kidnap women and children (Sanderson 1961, pp. 161-162).

People in Belize (formerly British Honduras) speak of semi-human creatures called Dwendis, which inhabit the jungles in the southern part of their country. The name Dwendi comes from the Spanish word Duende, meaning "goblin." Ivan Sanderson, who conducted research in Belize, wrote (1961, pp. 164 -165): 'Dozens told me of having seen them, and these were mostly men of substance who had worked for responsible organizations like the Forestry Department and who had, in several cases, been schooled or trained either in Europe or the United States. One, a junior forestry officer born locally, described in great detail two of these little creatures that he had suddenly noticed quietly watching him on several occasions at the edge of the forestry reserve near the foot of the Maya Mountains. . . . These little folk were described as being between three foot six and four foot six, well proportioned but with very heavy shoulders and rather long arms, clothed in thick, tight, close brown hair looking like that of a short-coated dog; having very flat yellowish faces but head-hair no longer than the body hair except down the back of the neck and midback.' The Dwendis appear to represent a species different from the large Sasquatch of the Pacific Northwest of North America.

Most of Sanderson's informants told him that the Dwendis carried what appeared to be dried palm leaves or some kind of large hatlike object over their heads. Sanderson (1961, p. 165) observed: "This at first sounds like the silliest thing, but when one has heard it from highly educated men as well as from simple peasants, and all over an area as great as that from Peten [southern Mexico] to Nicaragua, one begins to wonder." He then pointed out: 'There are many Mayan bas-reliefs that show pairs of tiny little men with big hats but no clothes, standing among trees and amid the vast legs of demi-gods, priests, and warriors. They are also much smaller than the peasants bearing gifts to the temples' (Sanderson 1961, p. 166).

From the Guianas region of South America come accounts of wildmen called Didis. Early explorers heard reports about them from the Indians, who said they were about five feet tall, walked erect, and were covered with thick black hair.

In 1931, Nello Beccari, an anthropologist from Italy, heard an account of the Didi from Mr. Haines, the Resident Magistrate in British Guiana. Heuvelmans gave this summary of what Haines related to Beccari: "Haines told him that he had come upon a couple of di-di many years before

when he was prospecting for gold. In 1910 he was going through the forest along the Konawaruk, a tributary which joins the Essequibo Just above its Junction with the Potaro. when he suddenly came upon two strange creatures, which stood up on their hind feet when they saw him. They had human features but were entirely covered with reddish brown fur. . . . the two creatures retreated slowly and disappeared into the forest" (Sanderson 1961, pp. 179-180).

After giving many similar accounts in his book about wildmen, Sanderson (1961, p. 181) stated: "The most significant single fact about these reports from Guiana is that never once has any local person—nor any person reporting what a local person says—so much as indicated that these creatures are Just 'monkeys.' In all cases they have specified that they are tailless, erect, and have human attributes."

From the eastern slopes of the Andes in Ecuador come reports of the Shim, a small fur-covered hominidlike creature, about 4 to 5 feet tall (Sanderson 1961, p, 166), In Brazil, people tell of the large apelike Mapinguary, which leaves giant humanlike footprints and is said to kill cattle (Sanderson 1961, p, 174),

#### 10.7 Yeti: Wildmen of The Himalayas

Diaries and other papers of British officials residing in the Himalayan region of the Indian subcontinent during the nineteenth century contain sporadic references to sightings and footprints of wildmen called Yeti. The Yeti were first mentioned by B. H, Hodgson, who from 1820 to 1843 served as British resident at the Nepalese court, Hodgson reported that in the course of a Journey through northern Nepal his bearers were frightened by the sight of a hairy, tailless, humanlike creature,

Many will suggest, on hearing a report like this (and hundreds have been recorded since Hodgson's time), that the Nepalese mistook an ordinary animal for a Yeti, The usual candidates for mistaken identity are bears and the langur monkey, But it is hard to imagine that lifelong residents of the Himalayas, intimately familiar with the wildlife, would have made such mistakes, Myra Shackley observed that Yeti are found in Nepalese and Tibetan religious paintings depicting hierarchies of living beings, "Here," said Shackley (1983, p, 60), "bears, apes, and langurs are depicted separate from the wildman, suggesting there is no confusion (at least in the minds of the artists) between these forms,"

After reviewing the available reports, Ivan Sanderson (1961, p, 358) compiled the following composite description of the Yeti: "Somewhat larger than man-sized and much more sturdy, with short legs and long arms: clothed in long rather shaggy fur or hair, same length all over and not differentiated. Naked face and other parts jet black; bull-neck and small conical head with heavy browridges; fanged canine teeth; can drop hands to ground and stand on knuckles like gorilla, , .. heel very wide and foot almost square and very large, second toe longer and larger than first, and both these separated and semi-opposed to the remaining three which are very small and webbed,"

During the nineteenth century, at least one European reported personally seeing a captured animal that resembled a Yeti. A South African man told Myra Shackley (1983, p. 67): "Many years ago in India, my late wife's mother told me how her mother had actually seen what might have been one of these creatures at Mussorie, in the Himalayan foothills. This semi-human was walking upright, but was obviously more animal than human with hair covering its whole body. It was reportedly caught up in the snows. ... his captors had it in chains. "

During the twentieth century, sightings by Europeans of wildmen and their footprints continued, increasing during the Himalayan mountain-climbing expeditions of the 1930s. In 1938, H. W. Tilman followed a trail of footprints for a mile on a glacier, at an elevation of 19,000 feet. Speaking of one of his Sherpa guides, Tilman stated: "Sen Tensing, who had no doubt whatever that the creatures . . . that made the tracks were 'Yetis' or wild men, told me that two years before, he and a number of other Sherpas had seen one of them at a distance of about 25 yards at Thyangbochi. He described it as half man and half beast, standing about five feet six inches, with a tall pointed head, its body covered with reddish brown hair, but with a hairless face. . . . Whatever it was that he had seen, he was convinced that it was neither a bear nor a monkey, with both of which animals he was, of course, very familiar" (Heuvelmans 1962, pp. 136 -137).

During the Second World War, a man named Slavomir Rawicz escaped from a Siberian prisoner-of-war camp and made his way by foot to India. In *The Long March* (1956), a book describing his experiences, Rawicz stated that while traveling across the Himalayas, he encountered two wildmen, six feet in height and covered with long reddish hair. However, explorers familiar with the region traversed by Rawicz have pointed out some inconsistencies in his account of his journey. For example, he took an inordinate amount of time to travel a certain section of the route—even at his own stated rate of progress. Critics thus insinuated that Rawicz's book, including the story of the wildmen, was largely if not completely fictional (Shackley 1983, pp. 54-55).

In November of 1951, Eric Shipton, while reconnoitering the approaches to Mt. Everest, found footprints on the Menlung glacier, near the border between Tibet and Nepal, at an elevation of 18,000 feet. Shipton followed the trail for a mile. Already well known as a mountaineer, Shipton could not easily be accused of publicity-seeking. A close-up photograph of one of the prints has proved convincing to many. Myra Shackley (1983, pp. 55-56) wrote: "Indeed, even the doubters admit that Shipton's famous footprints, seen on the Menlung Glacier in 1951, cannot readily be explained away."

The footprints were quite large. John R. Napier considered the possibility that the particular size and shape of the best Shipton footprint could have been caused by melting of the snow. Napier, however, noted (1973, p. 140): "Eric Shipton agrees that melting and sublimation might be responsible for the appearance, but he points out quite correctly that it would be reasonable to expect the narrow ridges behind and between the little toes to be the first features to disappear in these circumstances." For Napier, Shipton's observation appeared to rule out the snow-melting

explanation, or at least make it far less likely. Napier proposed another possibility: "that the footprint is double—two tracks superimposed. But a double— what? I don't know.'

Napier (1973, p. 141) concluded: "Something must have made the Shipton footprint. Like Mount Everest, it is there, and needs explaining. I only wish I could solve the puzzle; it would help me sleep better at night. Of course, it would settle a lot of problems if one could simply assume that the Yeti is alive. . . . The trouble is that such an assumption conflicts with the principles of biology as we know them." In the end, Napier suggested that the Shipton footprint was the result of superimposed human feet, one shod and the other unshod. In general, Napier, who was fully convinced of the existence of the North American Sasquatch, was highly skeptical of the evidence for the Yeti. But, as we shall see later in this section, new evidence would cause Napier to become more inclined to accept the Himalayan wildman.

In the course of his expeditions to the Himalaya Mountains in the 1950s and 1960s, Sir Edmund Hillary gave attention to evidence for the Yeti, including footprints in snow. He concluded that in every case the large footprints attributed to the Yeti had been produced by the merging of smaller tracks of known animals, by superimposition and melting. To this Napier (1973, pp. 57- 58), himself a skeptic, replied: "The signs of melting are so obvious that no one with any experience would confuse a melted footprint with a fresh one. Not all the prints seen over the years by reputable observers can be explained away in these terms; there must be other explanations for footprints, including, of course, the possibility that they were made by an animal unknown to science."

But although Napier was unwilling to completely reject the existence of an unknown hominid, he was nevertheless inclined to regard this as the least probable or desirable alternative. In 1956, Professor E. S. Williams photographed some prints on the Biafo glacier in the Karakoram mountains. Napier, who thought it likely that they were the superimposed prints of the front and rear paws of a bear, said (1973, p. 130): "It is impossible to state categorically that Williams's prints are those of a bear and not of a Yeti, but in the spirit of Bishop of Ockham it seems more reasonable to explain a phenomenon in terms of the known rather than the unknown."

Of course, in avoiding the relatively straightforward explanation that a peculiar set of tracks in snow was made by an unknown animal, one is forced to come up with all kinds of speculative hypotheses about the superimposition of prints of various animals and humans, or the transformation of such prints by melting, in a manner not clearly understood. And this would also appear to be a violation of a key aspect of Ockham's razor—namely, that the simplest of competing theories is preferable to the more complex.

In addition to Westerners, native informants also gave a continuous stream of reports on the Yeti. Lord Hunt, who headed a Mount Everest expedition in 1953, told of an incident recounted by the Tibetan Buddhist abbot of the Thyangboche monastery: "he gave a most graphic description of how a Yeti had appeared from the surrounding thickets, a few years back in the winter when the snow lay on the ground. This beast, loping along sometimes on its hind legs and sometimes on all fours, stood about five feet high and was covered with gray hair" (Shackley 1983, p. 62).

In 1958, Tibetan villagers from Tharbaleh, near the Rongbuk glacier, came upon a drowned Yeti, said Myra Shackley in her book on wildmen. The villagers described the creature as being like a small man with a pointed head and covered with reddish-brown fur (Shackley 1983, p. 1983).

Some Buddhist monasteries claim to have physical remains of the Yeti. One category of such relics is Yeti scalps, but the ones studied by Western scientists are thought to have been made from the skins of known animals (Shackley 1983, pp. 65-66). In 1960, Sir Edmund Hillary mounted an expedition to collect and evaluate evidence for the Yeti and sent a Yeti scalp from the Khumjung monastery to the West for testing. The results indicated that the scalp had been manufactured from the skin of the serow, a goatlike Himalayan antelope. But some disagreed with this analysis. Shackley (1983, p. 66) said they "pointed out that hairs from the scalp look distinctly monkey-like, and that it contains parasitic mites of a species different from that recovered from the serow."

In the 1950s, Western explorers sponsored by American businessman Tom Slick obtained samples from a mummified Yeti hand kept at Pangboche. Shackley (1983, p. 66) stated: "detailed investigation of small skin samples back in European laboratories failed to reach a diagnosis. Local rumour maintains that the hand comes from a rather poorly mummified lama, but it has some curiously anthropoid features. "

In May of 1957, the Kathmandu Commoner carried a story about a Yeti head that had been kept for 25 years in the village of Chilunka, about 50 miles northeast of Kathmandu. The head reportedly had been severed from the corpse of a Yeti slain by Nepalese soldiers, who had hunted down the creature after it had killed many of their comrades (Shackley 1983, p. 66). Concerning another specimen. Shackley noted that Chemed Rigdzin DorJe, a Tibetan lama, spoke of the existence of a complete mummified Yeti.

Over the years, sightings continued. In 1970, mountaineer Don Willans was researching an approach to Annapurna, a high peak in northern Nepal. He found some tracks and at night saw an apelike creature bounding across the snow. Napier (1973, p. 135), still skeptical, said it could have been a langur monkey.

In 1978, Lord Hunt, who headed the British Mt. Everest expedition of 1953, saw Yeti tracks and heard the high-pitched cry the Yeti is said to make. Lord Hunt, described by Shackley as "a vigorous champion of the Yeti," had come upon similar tracks in 1953. In both 1953 and 1978, the tracks were found at altitudes of 15,000 to 20,000 feet, too high for either the black or red bears of the Himalayas. Shackley (1983, p. 56) stated: 'The tracks seen by Lord Hunt in 1978 were very fresh, and it was possible to see the impression of the toes, convincing him that the footprint represented the actual size and shape of the feet, about 13½ in. long and 6½ in. broad. . . . This is especially interesting since it has, of course, been frequently contended that such tracks are made either by other animals (bears or langurs being the most favoured), or by the impressions of human feet which have become exaggerated in the melting snow. "

It is interesting to note that science has recognized the existence of many fossil species on the strength of their footprints alone. Heuvelmans (1982, p. 3) stated: "The hypotheses and

reconstructions of cryptozoology (regarding animals actually alive) are no more daring, questionable, fantastic, or illegitimate than those upon which paleontology has based its reconstructions of the fauna of past ages. ... It seemed perfectly legitimate to give the scientific name *Chirodierium* to a fossil genus known only by its tracks, found in Germany, England, France, Spain, Italy, and the United States, and of which some 20 species have been described. Yet, at the same time, it seemed ridiculous, premature, and absurd to describe scientifically the Himalayan Yeti, known not only by many tracks not identifiable with any known animal, but also by morphology and behavior as related by numerous eyewitnesses. ”

In 1986, Marc E. Miller and William Caccioli, of the New World Explorers Society, retraced the route of Hillary's 1960 Yeti expedition, visiting the Buddhist monasteries at Khunijung, Thyangboche, and Pangboche. At Khumjung, Miller and Caccioli interviewed Khonjo Khumbi, the village elder who accompanied Hillary to the United States with the famous Yeti scalp. Khonjo told Miller and Caccioli that in the course of his travels through Tibet he had seen whole Yeti furs. The High Lama of the Thyangboche monastery also said he had seen such furs in the homes of great hunters.

Miller and Caccioli (1986, p. 82) reported that they received possible Yeti chest hairs from an elderly woman of Khumjung village in Tibet: "We were told that her son was carrying potatoes along a trail in 1978, and was allegedly attacked by a Yeti. The Yeti was described as a large male, nearly 7 feet tall, and covered with dark and reddish hair. During the course of the attack, the young man took his potato hoe and struck the Yeti across the chest. The Yeti fled into the higher mountain region. The young man struggled back to Khumjung village to his mother, and described his encounter with the Yeti. His wounds were serious, and he later died."

KAZAKHSTAN

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Figure 10.1. Areas where Yetis have been sighted in Central Asia and the Himalayas are shaded with vertical black bars (after Shackley 1983, pp. 78-79).

Figure 10.2. Drawing of a Mongolian Almas from a 19th-century Tibetan book (Shackley 1983, p. 97).

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## 10.8 The Almas of Central Asia

The Sasquatch and the Yeti, from the descriptions available, are large and very apelike. But there is another wildman, the Almas, which seems smaller and more human. Reports of the Almas are concentrated in an area extending from Mongolia in the north, south through the Pamirs, and then westward into the Caucasus region. Similar reports come from Siberia and the far northeast parts of the Russian republic.

Early in the fifteenth century, Hans Schiltener was captured by the Turks and sent to the court of Tamerlane, who placed him in the retinue of a Mongol prince named Egi. After returning to Europe in 1427, Schiltener wrote about his experiences. In his book, he described some mountains, apparently the Tien Shan range in Mongolia: 'The inhabitants say that beyond the mountains is the beginning of a wasteland which lies at the edge of the earth, No one can survive there because the desert is populated by so many snakes and tigers, In the mountains themselves live wild people, who have nothing in common with other human beings, A pelt covers the entire body of these creatures, Only the hands and face are free of hair. They run around in the hills like animals and eat foliage and grass and whatever else they can find, The lord of the territory made Egi a present of a couple of forest people, a man and a woman. They had been caught in the wilderness. together with three untamed horses the size of asses and all sorts of other animals which are not found in German lands and which I cannot therefore put a name to" (Shackley 1983, p. 93).

Myra Shackley (1983, pp. 93 - 94) found Schiltener's account especially credible for two reasons: "First, Schiltener reports that he saw the creatures with his own eyes Secondly. he refers to Przewalski horses. which were only rediscovered by Nikolai Przewalski In 1881. . . Przewalski himself saw

'wildmen' In Mongolia in 1871."

A drawing of an Almas is found in a nineteenth-century Mongol compendium of medicines derived from various plants and animals. The text next to the picture reads: 'The wildman lives In the mountains, his origins close to that of the bear, his body resembles that of man, and he has enormous strength. His meat may be eaten to treat mental diseases and his gall cures Jaundice" (Shackley 1983. p. 98).

Shackley (1983, p. 98) noted: "The book contains thousands of illustrations of various classes of animals (reptiles, mammals and amphibia), but not one single mythological animal such as are known from similar medieval European books. All the creatures are living and observable today. There seems no reason at all to suggest that the Almas did not exist also and illustrations seem to suggest that it was found among rocky habitats, In the mountains. "

In 1937, Dordji Melren, a member of the Mongolian Academy of Sciences. saw the skin of an Almas in a monastery in the Gobi desert. The lamas were using it as a carpet in some of their rituals. Shackley (1983, pp. 103 -104) stated: 'The hairs on the skin were reddish and curly. .. The features [of the face] were hairless, the face had eyebrows, and the head still had long disordered hair. Fingers and toes were in a good state of preservation and the nails were similar to human nails. "

A report of a more recent sighting of live wildmen was related to Myra Shackley by Dmitri Bayanov, of the Darwin Museum in Moscow. In 1963, Ivan Ivlov, a Russian pediatrician, was traveling through the Altai mountains in the southern part of Mongolia. Ivlov saw several humanlike creatures standing on a mountain slope. They appeared to be a family group, composed of a male, female, and child. Ivlov observed the creatures through his binoculars from a distance of half a mile until they moved out of his field of vision. His Mongolian driver also saw them and said they were common in that area. Shackley (1983, p. 91) stated: "So we are not dealing with folktales or local legends, but with an event that was recorded by a trained scientist and transmitted to the proper authorities. There is no reason to doubt Ivlov's word, partly because of his Impeccable scientific reputation and partly because, although he had heard local stories about these creatures he had remained sceptical about their existence."

After his encounter with the Almas family, Ivlov interviewed many Mongolian children, believing they would be more candid than adults. The children provided many additional reports about the Almas. For example, one child told Ivlov that while he and some other children were swimming in a stream, he saw a male Almas carry a child Almas across it (Shackley 1983, pp. 91-92).

In 1980, a worker at an experimental agricultural station, operated by the Mongolian Academy of Sciences at Bulgan, encountered the dead body of a wildman: "I approached and saw a hairy corpse of a robust humanlike creature dried and half-buried by sand. I had never seen such a humanlike being before covered by camel-colour brownish-yellow short hairs and I recoiled, although in my native land in Sinkiang I had seen many dead men killed in battle. ... The dead thing was not a bear or ape and at the same time it was not a man like Mongol or Kazakh or Chinese and Russian. The hairs of its head were longer than on its body" (Shackley 1983, p. 107).

The Pamir mountains, lying in a remote region where the borders of Tadzhikistan, China, Kashmir, and Afghanistan meet, have been the scene of many Almas sightings. In 1925, Mikhail Stephanovitch Topilski, a major-general in the Soviet army, led his unit in an assault on an anti-Soviet guerilla force hiding in a cave in the Pamirs. One of the surviving guerillas said that while in the cave he and his comrades were attacked by several apelike creatures. Topilski ordered the rubble of the cave searched, and the body of one such creature was found. Topilski reported (Shackley 1983, pp. 118-119): "At first glance I thought the body was that of an ape. It was covered with hair all over. But I knew there were no apes in the Pamirs. Also, the body itself looked very much like that of a man. We tried pulling the hair, to see if it was just a hide used for disguise, but found that it was the creature's own natural hair. We turned the body over several times on its back and its front, and measured it. Our doctor made a long and thorough inspection of the body, and it was clear that it was not a human being."

"The body," continued Topilski, "belonged to a male creature 165-170 cm [about 5 ft 4 in] tall, elderly or even old, judging by the greyish colour of the hair in several places. The chest was covered with brownish hair and the belly with greyish hair. The hair was longer but sparser on the chest and close-cropped and thick on the belly. In general the hair was very thick, without any underfur. There was least hair on the buttocks, from which fact our doctor deduced that the

creature sat like a human being. There was most hair on the hips. The knees were completely bare of hair and had callous growths on them. The whole foot including the sole was quite hairless and was covered by hard brown skin. The hair got thinner near the hand, and the palms had none at all but only callous skin."

Topilski added: "The colour of the face was dark, and the creature had neither beard nor moustache. The temples were bald and the back of the head was covered by thick, matted hair. The dead creature lay with its eyes open and its teeth bared. The eyes were dark and the teeth were large and even and shaped like human teeth. The forehead was slanting and the eyebrows were very powerful. The protruding jawbones made the face resemble the Mongol type of face. The nose was flat, with a deeply sunk bridge. The ears were hairless and looked a little more pointed than a human being's with a longer lobe. The lower jaw was very massive. The creature had a very powerful chest and well developed muscles. . . . The arms were of normal length, the hands were slightly wider and the feet much wider and shorter than man's."

In 1957, Alexander Georgievitch Pronin, a hydrologist at the Geographical Research Institute of Leningrad University, participated in an expedition to the Pamirs, for the purpose of mapping glaciers. On August 2, 1957, while his team was investigating the Fedchenko glacier, Pronin hiked into the valley of the Balyandkiik River. Shackley (1983, p. 120) stated: "at noon he noticed a figure standing on a rocky cliff about 500 yards above him and the same distance away. His first reaction was surprise, since this area was known to be uninhabited, and his second was that the creature was not human. It resembled a man but was very stooped. He watched the stocky figure move across the snow, keeping its feet wide apart, and he noted that its forearms were longer than a human's and it was covered with reddish grey hair. " Pronin saw the creature again three days later, walking upright. Since this incident, there have been numerous wildman sightings in the Pamirs, and members of various expeditions have photographed and taken casts of footprints (Shackley 1983, pp. 122-126).

We shall now consider reports about the Almas from the Caucasus region. According to testimony from villagers of Tkhina, on the Mokvi River, a female Almas was captured there during the nineteenth century, in the forests of Mt. Zaadan. For three years, she was kept imprisoned, but then became domesticated and was allowed to live in a house. She was called Zana. Shackley (1983, p. 112) stated: "Her skin was a greyish-black colour, covered with reddish hair, longer on her head than elsewhere. She was capable of inarticulate cries but never developed a language. She had a large face with big cheek bones, muzzle-like prognathous jaw and large eyebrows, big white teeth and a 'fierce expression. Eventually Zana, through sexual relations with a villager, had children. Some of Zana's grandchildren were seen by Boris Porshnev in 1964. In her account of Porshnev's investigations, Shackley (1983, p. 113) noted: "The grandchildren, Chalikoua and Taia, had darkish skin of rather negroid appearance, with very prominent chewing muscles and extra strong jaws." Porshnev also interviewed villagers who as children had been present at Zana's funeral in the 1880s.

In the Caucasus region, the Almas is sometimes called Biaban-gull. In 1899, K. A. Satunin, a Russian zoologist, spotted a female Biaban-guli in the Talysh hills of the southern Caucasus. He stated that the creature had "fully human movements" (Shackley 1983, p. 109). The fact that Satunin was a well-known zoologist makes his report particularly significant.

In 1941, V. S. Karapetyan, a lieutenant colonel of the medical service of the Soviet army, performed a direct physical examination of a living wildman captured in the Dagestan autonomous republic, just north of the Caucasus mountains. Karapetyan said: "I entered a shed with two members of the local authorities. When I asked why I had to examine the man in a cold shed and not in a warm room, I was told that the prisoner could not be kept in a warm room. He had sweated in the house so profusely that they had had to keep him in the shed. I can still see the creature as it stood before me, a male, naked and bare-footed. And it was doubtlessly a man, because its entire shape was human. The chest, back, and shoulders, however, were covered with shaggy hair of a dark brown colour. This fur of his was much like that of a bear, and 2 to 3 centimeters [1 inch] long. The fur was thinner and softer below the chest. His wrists were crude and sparsely covered with hair. The palms of his hands and soles of his feet were free of hair. But the hair on his head reached to his shoulders partly covering his forehead. The hair on his head, moreover, felt very rough to the hand. He had no beard or moustache, though his face was completely covered with a light growth of hair. The hair around his mouth was also short and sparse. The man stood absolutely straight with his arms hanging, and his height was above the average—about 180 cm [almost 5 feet 11 inches]. He stood before me like a giant, his mighty chest thrust forward. His fingers were thick, strong and exceptionally large. On the whole, he was considerably bigger than any of the local inhabitants. His eyes told me nothing. They were dull and empty—the eyes of an animal. And he seemed to me like an animal and nothing more" (Sanderson 1961, pp. 295-296). Significantly, the creature had lice of a kind different from those that infect humans. It is reports like this that have led scientists such as British anthropologist Myra Shackley and Soviet anatomist Dr. Zh. I. Kofman to conclude that the Almas may represent a relict population of Neanderthals or perhaps even *Homo erectus* (Shackley 1983, p. 114). What happened to the wildman of Dagestan? According to published accounts, he was shot by his Soviet military captors as they retreated before the advancing German army.

In the 1950s, Yu. I. Merezhinski, senior lecturer in the department of ethnography and anthropology at Kiev University, was doing research in Azerbaijan, in the northern part of the Caucasus region. From local people, Merezhinski heard reports of an Almas-like wildman called the Kaptar. Khadzi Magoma, an expert hunter, told Merezhinski that he would take him to a stream where the Kaptar sometimes bathed at night. In exchange, the hunter asked Merezhinski to take a flash photo of the creature for him. Merezhinski agreed, and they went to the stream, near which a few albino Kaptars were said to live. Shackley (1983, p. 110) stated: "sure enough Merezhinski saw one from a distance of only a few yards, clearly discernible on the river bank through the bushes. It was damp, lean and covered from head to foot with white hair. Unfortunately the reality of the creature was too much for Merezhinski, who instead of

photographing It shot at It with his revolver but missed in his excitement. The old hunter, furious at the deception, refused to repeat the experiment."

Here once more we have a report by a professional scientist who directly observed a wildman. As an anthropologist, Merezhinski was particularly well qualified to evaluate what he saw. It is reports like this that tend to dispel the charge that the Almas is a creature that exists only in folklore.

And as far as folklore is concerned, accounts of the Almas and other wildmen are not necessarily a sign that the Almas is imaginary. Dmitri Bayanov, of the Darwin Museum in Moscow, asked (1982, p. 47): "Is the abundant folklore, say, about the wolf or the bear not a consequence of the existence of these animals and man's knowledge of them?" Bayanov (1982, p. 47) added: "Therefore we say that, if relic hominoids were not reflected in folklore and mythology, then their reality can be called into question."

#### 10.9 Wildmen of China

"Chinese historical documents, and many city and town annals, contain abundant records of Wildman, which are given various names," states Zhou Guoxing of the Beijing Museum of Natural History (Zhou, G. 1982, p. 13).

Two thousand years ago, the poet-statesman Qu Yuan made many references to Shangui (mountain ogres) in his verses. Li Yanshou, a historian who lived during the T'ang Dynasty (a.d. 618-907), stated that the forests of Hubei province sheltered a band of wildmen. Wildmen also appeared in the writings of Li Shizhen, a pharmacologist of the Ming Dynasty (a.d. 1368-1644). In the fifty-first volume of his massive work on medical ingredients, he described several species of humanoid creatures, including one named Fei-fei.

Li wrote: "'Feifei,' which are called 'manbear,' are also found in the mountainous areas in west Shu (part of Sichuan Province today) and Chu division, where people skin them and eat their palms. The You mountain of Sha county, Fujian province, sees the same ones, standing about one zhang (equal to 3.1 meters [Just over 10 feet]) in height and smiling to the people they come across, and are called 'shandaren' (men as big as mountains), 'wildmen,' or 'shanxiao'" (Zhou, G. 1982, p. 13).

In the eighteenth century, the Chinese poet Yuan Mei made reference to strange creatures inhabiting the wild regions of Shanxi province, calling them "monkeylike, yet not monkeylike" (Yuan and Huang 1979, p. 57).

According to Zhou: "Even today, in the area of Fang County, Hubei Province, there are still legends about 'maoren' (hairy men) or 'wildmen.' A local chronicle, about 200 years old, says that the Fang mountain lying 40 li (2 li equals one kilometer [.62 mile]) south to the county town is precipitous and full of holes, where live many maoren, about one zhang high and hair-coated. They often come down to eat human beings and chickens and dogs, and seize those who fight with them.' A lantern on which there is an ornament of a 'maoren' figure was unearthed in this area during an archaeological excavation. It has been dated at 2,000 years" (Zhou, G. 1982, pp. 13-14).

There have been many other reports of wildmen from the Hubei province in central China. In 1922, a militiaman is said to have captured a wildman, but there are no further records of this incident (Poirier et al. 1983, p. 32).

In 1940, Wang Zelin, a graduate of the biology department of Northwestern University in Chicago, was able to directly see a wildman shortly after it was shot to death by hunters. Wang was driving from Baoji, in Shanxi Province, to Tianshui, in Gansu Province, when he heard gunfire ahead of him. He got out of the car to satisfy his curiosity and saw a corpse. It was a female creature, six and a half feet tall and covered with a coat of thick greyish-red hair about one and a quarter inches long.

The hair on its face was shorter. The cheek bones were prominent, and the lips jutted out. The hair on the head was about one foot long. According to Wang, the creature looked like a reconstruction of the Chinese *Homo erectus* (Yuan and Huang 1979, p. 57; Shackley 1983, pp. 79-82).

Ten years later, another scientist, Fan Jinquan, a geologist, saw some living wildmen. Zhou Guoxing stated: 'With the help of local guides, he watched, at a safe distance, two local Wildmen in the mountain forest near Baoji County, Shanxi Province, in the spring of 1950. They were mother and son, the smaller one being 1.6 meters [5.25 feet] in height. Both looked human' (Zhou, G. 1982, p. 14).

In 1957, a middle-school teacher of biology in Zhejiang province obtained the hands and feet of a "manbear" killed by local peasants. Zhou Guoxing wrote: "In December 1980, I went to Sui Chang to study these hand and foot specimens. I concluded beyond any doubt, that they belong to a higher primate, and have morphological traits of both ape and monkey. The eyewitnesses thought that they had belonged to a Wildman, or of a manlike 'strange animal,' but after examining the specimens, I determined that they were not the hands and feet of a Wildman. They might possibly belong to an enormous monkey (perhaps of a species of macaque not previously recorded in this area). . . . There is no denying the possibility that they came from an unknown primate in the Jiulong Mountain area" (Zhou, G. 1982, p. 18).

Talk of the existence of an enormous monkey, previously unknown, raises interesting questions about the Beijing *Homo erectus* finds. Beijing man, as generally portrayed in textbooks and films, was quite human and almost civilized, a Middle Pleistocene hunter, fire maker, and cave dweller. But as several dissenting scientists noted, most of the Beijing man fossils were thick, big-browed partial skulls with smashed braincases. They appeared to represent not a relatively advanced protohuman but rather the unfortunate animal-like prey of some more intelligent hominid. Perhaps the large, hitherto unknown species of macaque posited by Zhou in order to explain away some modern wildman evidence also inhabited China during the Middle Pleistocene, and the smashed skulls in the Zhoukoudian cave belonged to it. Or perhaps the broken skulls of Zhoukoudian belonged to the *Homo erectus*-like creature described above by Wang Zelin.

In 1961, workers building a road through the heavily forested Xishuang Banna region of Yunnan province in southernmost China reported killing a humanlike female primate. The creature was 1.2-1.3 meters (about 4 feet) tall and covered with hair. It walked upright, and according to the eyewitness reports, its hands, ears, and breasts were like those of a female human. The Chinese Academy of Sciences sent a team to investigate, but they were not able to obtain any physical evidence. Some suggested that the workers had come upon a gibbon. But Zhou Guoxing stated: "The present author recently visited a newsman who took part in that investigation. He stated that the animal which had been killed was not a gibbon, but an unknown animal of human shape. It is worth noting that, over the past 2 years or so, some people in the western border areas of Yunnan Province say that the above-mentioned kind of Wildman still move about, and that another one has since been killed" (Zhou, G. 1982, pp. 15-16).

In 1976, six cadres from the Shennongjia forestry region in Hubei province were driving at night down the highway near the village of Chunshuya, between Fangxian county and Shennongjia. On the way, they encountered a "strange tailless creature with reddish fur" (Yuan and Huang 1979, p. 56). Fortunately, it stood still long enough for five of the people to get out of the car and look at it from a distance of only a few feet, while the driver kept his headlights trained on it. The observers were certain that it was not a bear or any other creature with which they were familiar. They reported the incident in a telegram to the Chinese Academy of Sciences in Peking.

Over the years, Academy officials had received many similar reports from the same region of Hubei province. So when they heard about this incident, they decided to thoroughly investigate the matter. A scientific expedition consisting of more than 100 members proceeded to Hubei province. They collected physical evidence, in the form of hair, footprints, and feces, and recorded sightings by the local inhabitants (Yuan and Huang 1979). Subsequent research has added to these results.

Altogether, more than a thousand footprints have been found in Hubei province, some more than 19 inches long (Poirier et al. 1983, p. 34). Over 100 hairs have been collected, the longest measuring 21 inches. Some of the hairs were supplied by persons who claimed to have seen wildmen; others were taken from trees against which wildmen were said to have rubbed. Frank E. Poirier, an anthropologist at Ohio State University, reported (Poirier et al. 1983, p. 33): "The hair was studied by the Hubei Provincial Medical College and the Institute of Vertebrate Paleontology and Paleoanthropology in Beijing. The general consensus is that the hair belongs to a higher primate (monkey, ape, or human)."

Some have sought to explain sightings of wildmen in the Shennongjia region of Hubei province as encounters with the rare golden monkey, which inhabits the same area. The golden monkey might very well account for reports of creatures glimpsed for a moment at a great distance. But consider the case of Pang Gensheng, a local commune leader, who was confronted in the forest by a wildman.

Pang, who stood face to face with the creature, at a distance of five feet for about an hour, said: "He was about seven feet tall, with shoulders wider than a man's, a sloping forehead, deep-set



eyes and a bulbous nose with slightly upturned nostrils. He had sunken cheeks, ears like a man's but bigger, and round eyes. also bigger than a man's. His jaw jutted out and he had protruding lips. His front teeth were as broad as a horse's. His eyes were black. His hair was dark brown, more than a foot long and hung loosely over his shoulders. His whole face, except for the nose and ears, was covered with short hairs. His arms hung down to below his knees. He had big hands with fingers about six inches long and thumbs only slightly separated from the fingers. He didn't have a tail and the hair on his body was short. He had thick thighs, shorter than the lower part of his leg. He walked upright with his legs apart. His feet were each about 12 inches long and half that broad—broader in front and narrow behind, with splayed toes" (Yuan and Huang 1979, pp. 58-59).

Zhou Guoxing has suggested that the wildman of Hubei province might be a relict population of *Gigantopithecus*, a large apelike hominid that inhabited southern China during the Middle Pleistocene. Zhou noted that in the forests of Hubei province some types of trees from the Tertiary have survived, as have the panda and other mammals from the Middle Pleistocene (Zhou, G. 1982, p. 22).

#### 10.10 Wildmen of Malaysia And Indonesia

In 1969, John McKinnon, who journeyed to Borneo to observe orangutans, came across some humanlike footprints. McKinnon asked his Malay boatman what made them. "Without a moment's hesitation he replied 'Batutut,'" wrote McKinnon, "but when I asked him to describe the beast he said it was not an animal but a type of ghost. . . Batutut, he told me, is about four feet tall, walks upright like a man and has a long black mane. . . . Like other spirits of the forest the creature is very shy of light and fire" (Green 1978, p. 134).

Later, in Malaya, McKinnon saw some casts of footprints even bigger than those he had seen in Borneo, but he recognized them as definitely having been made by the same kind of creature. The Malaysians called it *Orangpendek* (short fellow). McKinnon stated: "Again natives spoke of a creature with long hair, who walks upright like a man. Drawings and even photographs of similar footprints found in Sumatra are attributed to the *Sedapa* or *Umang*, a small, shy, long-haired, bipedal being living deep in the forest" (Green 1978, pp. 134 -135). According to Ivan Sanderson, these footprints differ from those of the anthropoid apes inhabiting the Indonesian forests (the gibbon, siamang, and orangutan). They are also distinct from those of the sun bear (Sanderson 1961, p. 219).

Early in the twentieth century, L. C. Westenek, a governor of Sumatra, received a written report about an encounter with a *Sedapa* wildman. The overseer of an estate in the Barisan Mountains, along with some workers, observed the *Sedapa* from a distance of 15 yards. The overseer said he saw "a large creature, low on its feet, which ran like a man, and was about to cross my path; it was very hairy and it was not an orang-utan: but its face was not like an ordinary man's. It silently and gravely gave the men a disagreeable stare and then ran calmly away" (Sanderson 1961, pp. 216-217).

In a Journal article about wildmen published in 1918, Westenek recorded a report from a Mr. Oostingh, who lived in Sumatra. Once while proceeding through the forest, he came upon a man sitting on a log and facing away from him. Oostingh stated: "I saw that he had short hair, cut short, I thought; and I suddenly realised that his neck was oddly leathery and extremely filthy. 'That chap's got a very dirty and wrinkled neck!' I said to myself. His body was as large as a medium-sized native's and he had thick square shoulders, not sloping at all. ... he seemed to be quite as tall as I (about 5 feet 9 inches). Then I saw that it was not a man."

"It was not an orang-utan," declared Oostingh. "I had seen one of these large apes a short time before." What was the creature if not an orangutan? Oostingh could not say for sure: "It was more like a monstrously large slamang, but a siamang has long hair, and there was no doubt that it had short hair" (Sanderson 1961, p. 220).

In 1918, Mr. Van Heerwarden, a hunter, began finding tracks of the Sedapa in Sumatra. The footprints he saw were shaped like those of a small human being. Van Heerwarden also heard reports about the Sedapa from natives. In October of 1923, he himself spotted one in a tree: "I discovered a dark and hairy creature on a branch. . . . The sedapa was also hairy on the front of its body; the colour there was a little lighter than on the back. The very dark hair on its head fell to just below the shoulder-blades or even almost to the waist. . . . Had it been standing, its arms would have reached to a little above its knees; they were therefore long, but its legs seemed to me rather short. I did not see its feet, but I did see some toes which were shaped in a very normal manner. . . . There was nothing repulsive or ugly about its face, nor was it at all ape-like" (Sanderson 1961, pp. 222-223). After observing it for a while, Van Heerwarden allowed the creature to run away.

The presence of large humanlike creatures in the forests of the Indonesian archipelago is relevant to the dating of fossils of *Homo erectus* found in Java (Chapter 7). Paleoanthropologists assume that fossils displaying *Homo erectus* morphology must be 800,000 or more years old, even when they are found on the surface. In fact, almost all the fossils of *Homo erectus* from Java have been surface finds. But if creatures resembling *Homo erectus* are still roaming the forests of Indonesia, then the general practice of dating a fossil by its morphology is not secure. On January 20, 1986, Bernard Heuvelmans wrote in response to a letter from our researcher, Stephen Bernath: "I am convinced myself that fossils, especially remains of Hominoids, are dated not after the strata they have been found in but after a prejudiced idea of the strata they should have been found in according to the classical scheme of human evolution, which is completely wrong. "

In practice, morphological dating has worked like this. During the 1930s, many fossil skull fragments were found lying on the surface at various locations in Java. The formations in the region have since been dated by the potassium-argon method to the Middle Pleistocene. The potassium-argon method, it may be recalled, is used to date volcanic materials, not the bones themselves. Ideally, for a bone to be assigned a Middle Pleistocene date using this method, it should have been found lying beneath an undisturbed layer of volcanic material. But in Java this was not the case, for almost without exception the fossils labeled *Homo erectus* were found lying

on the surface or in unspecified locations. Scientists have simply assumed that the bones eroded from Middle Pleistocene formations, in which they had supposedly been deposited hundreds of thousands of years ago. The reason scientists feel comfortable in making this assumption is that they are certain that hominids with erect/s morphology have been extinct since the Middle Pleistocene. But perhaps not. Heuvelmans stated in his letter of January 20, 1986: 'small hairy hominoids, with long straight hair on the head, the Nittawo, were exterminated at the end of the 18th century in Sri Lanka. According to British leading primatologist Osman Hill (1945), these dwarfs could be modern representatives of *Homo erectus*. "

Such evidence makes possible another explanation for erectus-like fossils found on the surface of Middle Pleistocene formations, in Java and elsewhere. As far as Java is concerned, perhaps as little as 10,000 years ago, a Sedapalike creature died by a stream bed or lake shore, and its bones became fossilized in the sediments. In very recent times, a piece of the fragmented skull reappeared on the surface, where it was discovered by native collectors, who turned it over to a paleoanthropologist. Upon seeing its primitive erectus-like morphology, the paleoanthropologist assigned it to the Middle Pleistocene, giving it a date of 800,000 years or more. The fossil was then described in textbooks and cited as more proof for the hypothesis that modern human beings evolved over the past several hundred thousand years from more apelike ancestors. But the fossil may not actually belong to the Middle Pleistocene. It could in fact be much more recent.

Whether the Java *Homo erectus* fossils are recent or ancient, the existence of living erectus-like creatures (or recently living ones) in Java shows the coexistence of such creatures with humans in modern times. And, as we have seen in previous chapters, there is much evidence such creatures coexisted with humans in the distant past. This throws the accepted pattern of human evolution into complete confusion.

Paleoanthropologists, in the face of such evidence, or in ignorance of it, will insist that human beings of modern type could not have existed any earlier than one hundred thousand years ago, and certainly not in the Early Pleistocene, the Pliocene, or the Miocene.

But if there is uncertainty about what kinds of hominids may be around today, how can we be so sure about what kinds of hominids may or may not have been around in the distant past?

Empiric investigation of the fossil record may not be a sure guide. As Heuvelmans stated in a letter (April 15, 1986) to our researcher Stephen Bernath: "do not overestimate the importance of the fossil record. Fossilization is a very rare, exceptional phenomenon, and the fossil record cannot thus give us an exact image of life on earth during the past geological periods. The fossil record of primates is particularly poor because very intelligent and cautious animals can avoid more easily the very conditions of fossilization—such as sinking in mud or peat, for instance."

The empiric method undoubtedly has its limitations, and the fossil record is incomplete and imperfect. But when all the fossil evidence, including that for very ancient humans and living apemen, is objectively evaluated, the pattern that emerges is one of coexistence rather than sequential evolution.

## 10.11 Africa

Native informants from several countries in the western part of the African continent, such as the Ivory Coast, have given accounts of a race of pygmylike creatures covered with reddish hair. Europeans have also encountered them: "During one of his expeditions in the course of 1947, the great elephant-hunter Dunckel killed a peculiar primate unknown to him; it was small with reddishbrown hair and was shot in the great forest . . . between the Sassandra and Cavally rivers" (Sanderson 1961, p. 189). Natives are said to have bartered with these red-haired pygmies, called Sehites, leaving various trinkets in exchange for fruits (Sanderson 1961, p. 190).

Wildman reports also come from East Africa. Capt. William Hitchens reported in the December 1937 issue of *Discovery*: "Some years ago I was sent on an official lion-hunt in this area (the Ussure and Simibit forests on the western side of the Wembare plains) and, while waiting in a forest glade for a man-eater, I saw two small, brown, furry creatures come from dense forest on one side of the glade and disappear into the thickets on the other. They were like little men, about 4 feet high, walking upright, but clad in russet hair. The native hunter with me gazed in mingled fear and amazement. They were, he said, agogwe, the little furry men whom one does not see once in a lifetime" (Sanderson 1961, p. 191). Were they just apes or monkeys? It does not seem that either Hitchens or the native hunter accompanying him would have been unable to recognize an ape or monkey. Many reports of the Agogwe emanate from Tanzania and Mozambique (Green 1978, p. 133).

From the Congo region come reports of the Kakundakari and Kilomba. About 5.5 feet tall and covered with hair, they are said to walk upright like humans. Charles Cordier, a professional animal collector who worked for many zoos and museums, followed tracks of the Kakundakari in Zaire in the late 1950s and early 1960s. Once, said Cordier, a Kakundakali had become entangled in one of his bird snares. "It fell on its face," said Cordier, 'turned over, sat up, took the noose off its feet, and walked away before the nearby African could do anything" (Green 1978, p. 133).

Reports of such creatures also come from southern Africa. Pascal Tassy (1983, pp. 132-133), of the Laboratory of Vertebrate and Human Paleontology, wrote in a review of Heuvelmans's *Les Bêtes Humaines d'Afrique* (which has a chapter on relict australopithecines): "Philip V. Tobias, now on the Board of Directors of the International Society of Cryptozoology, once told Heuvelmans that one of his colleagues had set traps to capture living australopithecines." Tobias, from South Africa, is a recognized authority on *Australopithecus*.

According to standard views, the last australopithecines perished approximately 750,000 years ago, and *Homo erectus* died out around 200,000 years ago.

The Neanderthals, it is said, vanished about 35,000 years ago, and since then fully modern humans alone have existed throughout the entire world. Yet many sightings of different kinds of wildmen in various parts of the world strongly challenge the standard view. Also, recent fossil skulls reportedly display anomalously primitive features. For example, *Nature* (1908, vol. 77, p. 587) published a report by Dr. K. Stolyhwo on a recent Neanderthal skull, found as part of a skeleton in

a tomb that also contained a suit of chain armor and iron spearheads. Stolyhwo said the skull was similar to the Spy Neanderthal skull.

#### 10.12 Mainstream Science and Wildman Reports

Despite all the evidence we have presented, most recognized authorities in anthropology and zoology decline to discuss the existence of wildmen. If they mention wildmen at all, they rarely present the really strong evidence for their existence, focusing instead on the reports least likely to challenge their disbelief.

Skeptical scientists say that no one has found any bones of wildmen; nor, they say, has anyone produced a single body, dead or alive. But as we have seen, hand and foot bones of wildmen, and even a head, have been collected. Competent persons report having examined bodies of wildmen. And there are also a number of accounts of capture. That none of this physical evidence has made its way into museums and other scientific institutions may be taken as a failure of the process for gathering and preserving evidence. The operation of what we could call a knowledge filter tends to keep evidence tinged with disrepute outside official channels.

However, some scientists with solid reputations, such as Krantz, Napier, Shackley, Porshnev, and others, have found in the available evidence enough reason to conclude that wildmen do in fact exist, or, at least, that the question of their existence is worthy of serious study.

Myra Shackley wrote to our researcher Steve Bernath on December 4, 1984: "As you know, this whole question is highly topical, and there has been an awful lot of correspondence and publication flying around on the scene. Opinions vary, but I guess that the commonest would be that there is indeed sufficient evidence to suggest at least the possibility of the existence of various unclassified manlike creatures, but that in the present state of our knowledge it is impossible to comment on their significance in any more detail. The position is further complicated by misquotes, hoaxing, and lunatic fringe activities, but a surprising number of hardcore anthropologists seem to be of the opinion that the matter is veiy worthwhile investigating."

So there is some scientific recognition of the wildman evidence, but it seems to be largely a matter of privately expressed views, with little or no official recognition.

Always Something New Out of Africa

Always something new out of Africa. -Pliny the Elder (a.d. 27-79)

The controversies surrounding Java man and Beijing man, what to speak of Castenedolo man and the European eoliths, have long since subsided. As for the disputing scientists, most of them are in their graves, their bones on the way to disintegration or fossilization. But today Africa, the land of Australopithecus and Homo habilis, remains an active battlefield, with scientists skirmishing to establish their views on human origins.

Only In the later decades of the twentieth century did paleoanthropologists shift the main focus of their discipline from Europe and Asia to Africa. But the importance of Africa was foreseen by Darwin (1871, p. 199), who wrote In *The Descent of Man* "In each great region of the world the living mammals are closely related to the extinct species of the same region. It is, therefore, probable that Africa was formerly Inhabited by extinct apes closely allied to the gorilla and chimpanzee; and as these two species are man's nearest allies, It is somewhat more probable that our early progenitors lived on the African continent than elsewhere."

In this chapter, we survey the history of paleoanthropological discoveries in Africa. The finds from the early part of the twentieth century, such as Reck's skeleton (Section 11.1) and the Kanjera skulls and Kanam Jaw (Section 11.2), were controversial. According to their discoverers, these fossils represented evidence for anatomically modern humans In the Early Pleistocene. Anomalous finds continued to occur even in the latter part of the twentieth century. Among these we may number the Kanapoi humerus (Section 11.5.1), the ER 1481 femur from Lake Turkana (Section 11.6.3), and the Laetoli footprints (Section 11.10). Scientists have said all of them are morphologically within the modern human range. But Instead of taking these fossils as evidence for anatomically modern humans In unexpectedly ancient times, scientists have generally said they show that protohuman creatures such as *Australopithecus* and *Homo habilis* had skeletal features resembling those of modern humans. Indeed, most scientists have consistently depicted *Australopithecus* and *Homo habilis* as essentially human below their apelike heads. They also say that these creatures were exclusively terrestrial and bipedal In the human fashion. But there Is much evidence that this view Is mistaken, and that the australopithecines and habilines were very well adapted for life In the trees (Section 11.8).

### 11.1 Reck's skeleton

The first significant African discovery related to human origins and antiquity occurred in 1913. In that year, Professor Hans Reck, of Berlin University, conducted Investigations at Olduvai Gorge In Tanzania, then German East Africa. During his stay at Olduvai Gorge, Reck found a human skeleton that would remain a source of controversy for decades.

#### 11.1.1 The Discovery

While one of Reck's African collectors was searching for fossils on the northern slope of Olduvai Gorge, he spotted a piece of bone sticking up from the earth near a bush (Wendt 1955, p. 418). After clearing away the surface rubble, the collector saw parts of a complete and fully human skeleton embedded In the rock. He summoned Reck, who then had the skeleton taken out in a solid block of hardened sediment. The human skeletal remains, including a complete skull (Figure 11.1), were strongly cemented In the surrounding matrix, which had to be chipped with hammers and chisels (MacCurdy 1924a, p. 423).

Figure 11.1. This skull is from a fully human skeleton found in 1913 by H. Reck at Olduvai Gorge, Tanzania (Reck 1933, plate 31).

Reck identified a sequence of five beds at Olduvai Gorge. The first four beds are water-laid volcanic tuffs of various colors. Bed I is grey and yellow. Bed II is generally of a buff color, although the upper portion has a reddish tint. Bed III is bright red, while Bed IV is grey or brownish. Bed V, a loesslike deposit, is brownish (Hopwood 1932, p. 192).

At the top and base of Bed V are hard whitish layers of a limestone-like deposit of calcrete, or steppe-lime. The sequence of beds (Table 11.1) outlined by Reck is still in use today, except that upper Bed IV is now referred to as the Masek formation and Bed V has been divided into several distinct formations (M. Leakey 1978, p. 3). From oldest to youngest they are the Lower Ndutu, Upper Ndutu, and Naisiusiu formations (Oakley et al. 1977, p. 169).

TABLE 11.1 Stratigraphy of Olduvai Gorge, Tanzania

| Est Age (Years b.p.) |
|----------------------|
| 32,000               |

Upper Nddutu

Lower Nddutu

40,00 600,000 70000 1,15000

1,700,00

The skeleton was from the upper part of Bed II. Just below the skeleton were fossils of *Elephas antiquus recki* (Hopwood 1932, p. 192). To Reck, the faunal evidence indicated the human skeleton was of Middle Pleistocene age, roughly contemporary with Dubois's Java man, now thought to be about 800,000 years old. Modern dating methods, however, give the uppermost part of Bed II a late Early Pleistocene date of around 1.15 million years (Oakley et al. 1977, p. 166).

The overlying layers were not, however, intact. The skeleton had been found on the side of Olduvai Gorge, about 3 or 4 meters (10 to 13 feet) below the level of the plain (Protsch 1974, p. 379). Here (Figure 11.2) the overlying layers (Beds III, IV, and V) had been worn by erosion. Bed II was, however, still covered by rubble from bright red Bed III and from Bed V (Hopwood 1932, p. 194). It was clear to Louis Leakey (1932b) that perhaps as little as 50 years ago, the site would have been covered by "a small relic of Bed 3 overlain by Bed 5," the latter containing hard layers of calcrete. Beds III and V were present on the slope just above the spot where the skeleton was found. Bed IV was missing in the immediate area, apparently removed by erosion before the deposition of Bed V.

Reck, understanding the significance of his find, carefully considered the possibility that the human skeleton had arrived in Bed II through burial or earth movements. He determined this was not the case. Reck (1914b) said: "The bed in which the human remains were found, without any



accompanying cultural objects, showed no sign of disturbance. The spot appeared exactly like any other in the horizon. There was no evidence of any refilled hole or grave" (Hopwood 1932, p. 193).

Figure 11.2. This section of the northern slope of Olduvai Gorge (after Hopwood 1932, p. 192) shows the location where H. Reck found a fully human skeleton in 1913 in upper Bed II. Bed II is 1.15-1.7 million years old (Oakley et al. 1977, p. 166).

Later, Reck (1926) provided this account: "In some graves, the existence of refilling can be obscured by tamping down the ground and so forth. But artificial disturbance of the strata also results in the mixing together of different kinds of excavated earth. This should be quite evident in the present case. . . . But there was no sign of mixing of earth of different colors, nor were there any fragments of calcrete found mixed into the earth by the skeleton. Nothing of the sort was observed during the inspection of the original site and its surroundings at Olduvai nor in later examination of the matrix in which the skeleton was encased during transport to Germany" (Reck 1926, pp. 85-86; Hopwood 1932, p. 193). According to Reck, the strata at the site had not undergone any geological resorting by which, as Wendt (1955, p. 420) suggested, a recent layer, containing the skeleton, might have been forced into an older one.

In an unpublished manuscript, Reck observed: "The sediment. . . is so constituted that the artificial breaking of the bed with its visible layering by the digging of a grave would necessarily be recognizable.

The wall of the grave would have a definite border, an edge that would show in profile a division from the undisturbed stone. The grave filling would show an abnormal structure and heterogeneous mixture of excavated materials, including easily recognizable pieces of calcrete. Neither of these signs were to be found despite the most attentive inspection. Rather the stone directly around the skeleton was not distinguishable from the neighboring stone in terms of color, hardness, thickness of layers, structure, or order" (Hopwood 1932, pp. 193-194).

In his first report, Reck (1914a) observed: "The skeleton in the grave was complete, though somewhat shifted and compressed [Figure 11.3]. It lay in horizontal position, exactly parallel to the layers of stone in which it was embedded, just

as were all the faunal remains" (Hopwood 1932, p. 193).

Figure 11.3. The skull found by H. Reck in Bed II of Olduvai Gorge was distorted (Reck 1933, plate 30). W. O. Dietrich (1933) believed this distortion argued against the skeleton being a recent shallow burial.

This sheds light on the question of burial. Beds I through IV at Olduvai were laid down by water in a lake bed, producing a distinct sequence of thin layers. Before the deposition of the overlying Nduvu Beds, starting 400,000 years ago (Oakley et al. 1977, p. 166), faulting tilted the Olduvai strata in an east-west direction. If the skeleton had been buried in Bed II fairly recently, it probably would have intersected at an angle the layers of Bed II, here tilted about 7 degrees (Hopwood 1932, p. 192).

The skeleton's distortion by compression was also significant (Figure 11.3, p. 629). W. O. Dietrich, writing in 1933, stated that this feature of the skeleton argued against its being a recent, shallow burial in the top of Bed II. Its condition indicated a substantial accumulation of sediments had once covered it (Dietrich 1933, pp. 299-303). According to Reck, the deposition of the skeleton took place during the formation of Bed II. Later, the full weight of Beds III and IV would have covered the skeleton. Still later, after the erosion of Bed IV, Beds III and V would have covered the skeleton. All in all, the skeleton's condition and stratigraphic position appeared to rule out recent burial.

Reck returned to Germany, carrying Olduvai man's skull with him. He left the rest of the skeleton, encased in a block of Bed II sediment, to be shipped with the expedition's baggage.

Upon Reck's return to Germany, his African discovery attracted immediate attention, both in the popular press and in scientific circles. A leading American anthropologist, George Grant MacCurdy (1924a, p. 423) of Yale University, considered Reck's discovery to be genuine: "The human skeleton . . . came from the next to the lowest horizon (No. 2). . . . The skeleton was found some 3 or 4 meters (10 to 13 feet) below the rim of the Olduvai gorge, which here is about 40 meters [131 feet] deep. The skeleton bore the same relation to the stratified beds as did the other mammalian remains, and was dug out of the hard clay tufa with hammer and chisel just as these were. In other words, the conditions of the find were such as to exclude the possibility of an interment. The human bones are therefore as old as the deposit (No. 2)." He also agreed that the skeleton was of modern type: "Judging from the photograph of the skeleton still in situ, the man of Olduvai gorge did not belong to the Neandertal, but rather to the Aurignacian type" (MacCurdy 1924a, p. 423). Aurignacian refers to Cro-Magnon man, the first representative of *Homo sapiens sapiens* in Europe.

### 11.1.2 Leakey's conversion

Louis Leakey (1928, p. 499) examined Reck's skeleton in Berlin, but he initially judged it more recent than Reck had claimed. Other scientists agreed.

In 1931, Leakey and Reck, attempting to settle the issue, visited the site where the skeleton had been found. Along with them were A. T. Hopwood of the British Museum of Natural History, Donald MacInnes, and geologist E. V. Fuchs. After studying the geology, Leakey and Hopwood were won over to Reck's point of view. Leakey was also influenced by new discoveries of stone implements in Beds I and II of Olduvai Gorge. As we have seen, Reck originally reported that no cultural remains were found in Bed II, a fact that had caused Leakey to judge the skeleton not very old (Goodman 1983, p. 107).

In a letter published in *Nature*, the prestigious British science review, Leakey, Hopwood, and Reck confirmed that the skeleton was not buried from Bed IV, as Leakey had suggested in his book *The Stone Age Cultures of Kenya Colony* (1931), but was native to Bed II, as originally reported by Reck. They concluded that the skeletal remains belonged to an anatomically modern *Homo sapiens* who had lived during Africa's Upper Kamasian pluvial (rainy) period (L. Leakey et al. 1931), equivalent to the Mindel glacial period of the European middle Middle Pleistocene. This made Reck's skeleton roughly contemporary with Beijing man and Java man, both from the Middle Pleistocene. But, as previously mentioned, uppermost Bed II at Olduvai Gorge is now given a late Early Pleistocene age of 1.15 million years. By modern accounts, *Homo sapiens sapiens* is not thought to be more than 100,000 years old, although some specimens regarded as early *Homo sapiens* are dated at around 300,000 years.

In an article published in the *Times of London*, Leakey stated that his firsthand research in Africa had established "almost beyond question that the skeleton of a human being found by Professor Reck in 1913 is the oldest authentic skeleton of *Homo sapiens*" (Goodman 1983, p. 107). This led Leakey to announce that Beijing man and Java man were not direct human ancestors. How could they be, when Reck's skeleton, fully human, was just as old as they were?

Hopwood later published his own account of the 1931 expedition to Olduvai. Hopwood (1932, p. 193) stated: "Examination of the site in 1931 confirmed the observation that the bed in which the skeleton lay was undoubtedly, Bed II." Hopwood (1932, p. 194) added: "The slope is covered by rubble from Beds III and V in such a manner that it is difficult to see how a shallow grave could be dug and filled again without including some of this rubble."

From his study of the stratigraphy and the rate of erosion, Hopwood concluded that as little as 250 years ago "the place where the skeleton lay would certainly have been covered by the lower hard layer [of Bed V calcrete], which is ten to twelve inches thick." Hopwood pointed out that the calcrete layers at the site were extremely hard. He once saw laborers working with heavy crow bars take two full days to dig a hole just 2 feet square and 3 feet deep through similar material. The nearly impenetrable character of the calcrete appeared to rule out burial (Hopwood 1932, p. 194). Furthermore, the Bed

II sediments themselves were quite hard at that point. The skeleton found by Reck in 1913 had to be extracted with hammers and chisels.

After reproducing statements from Reck's original reports, Hopwood (1932, p. 194) stated: "It is clear that Professor Reck, when he found the skeleton, thought it possible that he might be dealing with an intrusive burial. that he was careful to look for evidence for this, and that he failed to find it."

Hopwood (1932, p. 195) concluded: "it seems to follow from the original evidence of Professor Reck that the skeleton lay in undisturbed sediment without trace of foreign matter. The ethnological evidence appears to show, that despite physical resemblances, the skeleton is not of the Masai, who inhabit the country today, and that in pre-Masai days the actual part of the bed was in such a position that it was inaccessible to a tribe only with native tools. Hence the conclusion of my colleagues and myself that the skeleton was enclosed in Bed II before that bed was covered by later deposits; and in that sense we regard the skeleton as contemporary with Bed II."

Around this time, Sir Arthur Keith, who initially thought Reck's skeleton recent, also adopted the Bed II date. But not everyone agreed with the conclusion that Leakey and Hopwood reached after their 1931 expedition.

#### 11.1.3 Cooper and Watson launch their Attack

In February of 1932, *Nature* printed a letter by zoologists C. Forster Cooper of Cambridge and D. M. S. Watson of the University of London. They suggested that the completeness of the skeleton found by Reck clearly indicated it was a recent burial (Cooper and Watson 1932a, p. 312).

Cooper and Watson (1932a, p. 312) stated: "Complete mammalian skeletons of any age are, as field palaeontologists know, of great rarity. When they occur, their perfection can usually be explained as the result of sudden death and immediate covering by volcanic dust." Even here, Cooper and Watson admitted that examples of complete, naturally-deposited skeletons, although rare, do in fact occur. They gave one circumstance for such an occurrence and indicated there might be others.

Cooper and Watson, casting further doubt on the claimed age for Reck's skeleton, contended that no one had yet found anatomically modern human skeletal remains anywhere near as old. They dismissed the Galley Hill skeleton, claiming it was 'never seen in situ by any trained observer' (Cooper and Watson 1932a, p. 312). This is not unreasonable. As far as we can tell, the Galley Hill skeleton could be recent, but there is also evidence suggesting it could be from the middle Middle Pleistocene (Section 6.1.2.1). Cooper and Watson then mentioned the Ipswich skeleton, observing that it had "been withdrawn by its discoverer." While it is true that J. Reid Moir did change his mind about the age of the Ipswich skeleton, our own study (Section 6.1.3) shows that there is still reason to think it might be from the middle Middle Pleistocene.

Cooper and Watson (1932a, p. 312) then referred obliquely to "other fragments, found long ago . . . entirely without satisfactory evidence as to their mode of occurrence." They ignored (or were ignorant of) the finds at Castenedolo, Italy (Section 6.2.2). There G. Ragazzoni, a professional geologist, found in situ, in a Pliocene formation, a fairly complete and anatomically modern human skeleton, as well as parts of others.

In May 1932, Leakey replied to Cooper and Watson. In a letter to *Nature*, he argued that no more than 50 years ago the reddish-yellow upper part of Bed II would have been covered by an intact layer of bright red Bed III. If the skeleton had been buried in recent times (50 or more years ago), there should have been a mixture of bright red and reddish-yellow sediments in the grave filling. Such was not the case. "I was lucky enough personally to examine the skeleton at Munich while it was still intact in its original matrix," wrote Leakey, "and could detect no trace whatever of such admixture or disturbance." He added: 'The bones of the skeleton . . . are, as far as I know, every bit as mineralized as most of the bones from Bed No. 2 itself' (L. Leakey 1932a, p. 721). This would argue against their being very recent.

Leakey, however, agreed with Cooper and Watson that Reck's skeleton had arrived in its position in Bed II by burial, but he did not think the burial was recent. "My own personal belief..," wrote Leakey, 'is that contemporary man, living on the edge of the then existing Oldoway lake, buried the skeleton into the muddy, clayey edge of the lake whilst Bed No. 2 was in the process of being deposited, for Bed No. 2 is essentially a shallow water deposit at the place where the skeleton was found' (L. Leakey 1932a, p. 721). Reck, on the other hand, believed that the individual had drowned and been covered by sedimentation.

Some scientists had called attention to apparent filing of the teeth of Reck's skeleton, suggesting this was characteristic of the tribal people inhabiting the region during recent historical times. To this Leakey replied: "I have personally examined the so-called 'filing' of the teeth of the Oldoway man on the original specimen at Munich, and this 'filing' has no resemblance to any filing done by native tribes to-day, and it is, to my mind, exceedingly doubtful if it can be called filing at all" (L. Leakey 1932a, p. 721).

Leakey then referred to his own finds at Kanam and Kanjera (Section 11.2), which he believed supported the Middle Pleistocene antiquity of Reck's skeleton. "Actually in situ at a place called Kanam," stated Leakey, "in the same horizon as the Pre-Chellean tools and the *Deinotherium*, we found a fragment of a mandible of *Homo sapiens* type, thus putting *Homo sapiens* in East Africa back one stage further than Oldoway Man—in fact, in deposits of the same age as Bed No. 1 at Oldoway" (L. Leakey 1932 a, p. 722). The upper part of Bed 1 at Olduvai is now thought to be about 1.7-1.8 million years old (Oakley et al. 1977, p. 166).

About the Kanjera finds, Leakey reported: "We have . . . found fragments of the skulls of three different individuals of *Homo sapiens* type completely mineralised and just washed out of the exposures by the rains. They are in the same state of complete mineralisation as the remains of *Elephas antiquus*,

Hipparion, etc., from the same beds, and I have personally no doubt whatever that they were in situ a month or two ago, before the beginning of the present rainy season. These later remains are probably, then, the contemporary of the Oldoway skeleton, and since we have fragments which make up the greater part of the skull cap of one of the [KanJera] individuals, an interesting comparison will be possible later on" (L. Leakey 1932a, p. 722).

C. Forster Cooper and D. M. S. Watson were still not satisfied. In June 1932, they said in a letter to *Nature* that red pebbles from Bed III may perhaps have been discolored. "Mere proximity to a large decaying body often alters the character of a matrix," said Cooper and Watson (1932b). This would explain why Reck and Leakey did not see the Bed III pebbles in the matrix surrounding the skeleton. Hopwood, however, disagreed that Bed III pebbles would have lost their bright red color. He pointed out that the top of Bed II, in which the skeleton was found, was also reddish and stated: "The reddish colour of the matrix is against the theory that any inclusions of Bed III would have been decolorised by decomposition products" (Hopwood 1932, p. 194).

In support of their post-Bed II burial hypothesis, Cooper and Watson offered additional explanations for the absence of Bed III materials in the supposed grave filling. According to Cooper and Watson (1932b), the grave diggers would have taken the red Bed III materials out first and thrown them back in last, on the top. This would explain why no Bed III materials were present in the matrix immediately surrounding the skeleton in Bed II. But this hypothesis depends on a fairly deep grave, with lots of Bed II materials being thrown out of the grave upon the previously removed Bed III materials. This would insure little mixing when the materials were placed back into the grave. But the hardness of the Bed II materials argues against a deep burial. When Reck found the skeleton, it had to be removed with chisels. So if there were a burial, it would most likely have been a shallow one. And in a shallow grave, dug through the rubble of Beds III and V a short distance into Bed II, mixing of materials from Beds II, III, and V would have been hard to avoid in the grave refilling. Since no mixing was visible, there was, all things considered, probably no post-Bed II burial.

Another suggestion—the skeleton was buried horizontally into Bed II, from the side of Olduvai Gorge. Therefore, no Bed

III materials were found in the skeleton's matrix. But Hopwood (1932, p. 194) said: "It would appear that the onus of proof lies on those who might wish to make such a suggestion." The hardness of Bed II poses a substantial obstacle to horizontal burial.

Furthermore, in an October 1932 letter to *Nature*, Leakey (1932b) pointed out that the side of the cliff had receded about 2 feet since 1913. At this rate, a few centuries ago the side of the cliff would have been many yards past the present position of the skeleton. So any burial by horizontal tunneling must have taken place fairly recently. And Hopwood (1932, p. 194) noted: "The present inhabitants of the country, the Masai, rarely bury their dead." And if they did, they did not dig tunnels. Hopwood, describing current Masai burial practices, said: "the shallow grave (about one metre [3 feet] deep) is filled with stones and earth." The stones are meant "to keep hyaenas from abstracting the body." Reck's skeleton was not surrounded by stones.

Leakey's measurements also showed that since 1913 erosion had lowered the land surface near the skeleton's resting place by about 6 inches. Repeating a conclusion he had expressed in his May 1932 letter, Leakey said: "my own estimate is that a time less than fifty years before Prof. Reck came to Oldoway, the site where he found the skeleton was covered by a deposit consisting of a very small relic of Bed 3 overlain by Bed 5 and the steppe lime" (L. Leakey 1932b). Therefore, if a burial took place 50 or more years ago, workers would have had to dig through bright red layers of Bed III materials and the hard calcrete layers of Bed V. And neither Leakey nor Reck had seen any materials from Bed III or Bed V present in the skeleton's matrix.

But Cooper and Watson (1932b) called Leakey's 50-year estimate "a guess." They thought it was possible that Bed II could have been exposed for a very much longer time, allowing the skeleton to be buried without the difficulty of digging through the bright red Bed III materials or the hard calcrete layers of Bed V. The longer period of time would also allow for the subsequent fossilization of the skeleton. But the high rate of erosion observed by Leakey did not support the view maintained by Cooper and Watson.

Also, Hopwood (1932, p. 194) observed that Bed II was, at the time the skeleton was excavated, covered with a rubble of Bed III and Bed V materials, along with pieces of steppe lime, or calcrete. So even if the overlying beds were not intact, a very recent burial should nevertheless have caused their loose materials to be mixed in the grave filling.

In his October letter, Leakey responded to criticism of his proposal that Reck's skeleton had been buried during the formation of Bed II, a shallow Middle Pleistocene lake bottom. Leakey suggested that the deposit might have been dry during parts of the year, as often occurs with African lakes. He also remarked that burial in shallow water is not unknown. "Even to-day in certain circumstances," he wrote, 'some native tribes dispose of the bodies of undesirables, such as suicides, in just such a way, 'so as to prevent the spirit from escaping'" (L. Leakey 1932b).

Leakey also replied to a suggestion by Cooper and Watson that the Kanam and Kanjera discoveries were irrelevant to the solution of the question of the age of Reck's skeleton. "I must, however, add," he wrote, 'that I do regard the discovery of the Kanam mandible and Kanjera skulls as relevant to the Oldoway problem, in that they at least show that Homo sapiens was in existence at the time when Bed 2 at Oldoway was being formed" (L. Leakey 1932b, p. 578).

#### 11.1.4 Reck and Leakey change their Minds

Despite the broadsides from Cooper and Watson, Reck and Leakey seemed to be holding their own. But in August 1932, P. G. H. Boswell, a geologist from the Imperial College in England, gave a perplexing report in the pages of Nature.

Professor Mollison had sent to Boswell from Munich a sample of what Mollison said was the matrix surrounding Reck's skeleton. Mollison, it may be noted, was not a completely neutral party. As early as 1929, he had expressed his belief that the skeleton was that of a Masai tribesman, buried in the not too distant past (Protsch 1974, p. 380).

Boswell (1932, p. 237) stated that the sample supplied by Mollison contained "(a) pea-sized bright red pebbles like those of Bed 3, and (b) chips of concretionary limestone indistinguishable from that of Bed 5 and enclosing at least one mineral (an amphibole), in relative abundance, not found in Beds 2 and 3, but present in Bed 4." Boswell took all this to mean that the skeleton had been buried after the deposition of Bed V, which is topped by a hard layer of steppelime, or calcrete. At the time he wrote his report, he was unaware that there was also a layer of calcrete at the bottom of Bed V.

The presence of the bright red Bed III pebbles and Bed V limestone chips in the sample sent by Mollison certainly calls for some explanation. Reck and Leakey had both carefully examined the matrix at different times over a period of

20 years. They did not report any mixture of Bed III materials or chips of limestonelike calcrete, even though they were specifically looking for such evidence. So it is remarkable that the presence of red pebbles and limestone chips should suddenly become apparent.

In short, we are faced with contradictory testimony. It would appear that at least one of the participants in the discovery and the subsequent polemics was guilty of extremely careless observation—or cheating.

Reck had studied the matrix at the site. And both Reck and Leakey had studied the matrix directly in contact with the skeleton in Munich. Did they fail to see the red pebbles and chips of limestone, or make false statements about their absence in the matrix? Neither possibility seems likely.

Later, Boswell and other scientists in England studied a sample sent from Munich, in isolation from any of the bones. Mollison, we have already noted, had for years expressed his own view that the skeleton was a recent burial. His statement assuring Boswell that the sample was part "of the material in which the Oldoway skeleton had been embedded" is thus open to question.

Cooper and Watson (1932b) had pointed out in one of their letters to *Nature*: "The photographs published by Prof. Reck show that the whole of the upper and a good deal of the lateral surfaces of the skeleton were exposed during the excavation made for its removal. ... It need scarcely be pointed out that the only material certainly of the grave infilling carried to Munich in this way is that which is contained within the ribs and between the limbs and the trunk." Did Mollison carefully take his sample from within the ribs or between the legs of Reck's skeleton? Or did he take it from matrix materials that may have come from elsewhere on the block of sediment that contained the skeleton? None of the reports we have seen give any information that would allow these questions to be answered.

Even if the matrix sample supplied by Mollison was suitable for analysis, the presence of limestone chips (containing amphibole) is of ambiguous significance. E. J. Wayland (1932), head of the Geological Survey of Uganda, wrote in a letter to *Nature*: "The fact that the matrix . . . contained bits of concretionary limestone containing a mineral characteristic of Bed 4 does not prove the burial



to be post-Bed 5, for Bed 4 contains concretionary limestone, and for that matter so do the other beds, not excluding Bed 2."

It seems that Boswell's mineral test, if accepted at face value, most strongly supports a Bed IV burial. During such a burial, Bed IV limestone chips and bright red Bed III pebbles could have been mixed into Bed II sediments. But a Bed IV burial would still give the anatomically modern skeleton an unexpectedly great age (Table 11.1, p. 629) of 400,000 to 700,000 years.

Keep in mind, however, that Reck, who examined the skeleton in situ, saw no signs of limestone chips or bright red pebbles, although he looked carefully for them. This suggests that no burial activity disturbed any layers of limestonelike calcrete in Beds II, III, IV, or V.

The debate about the age of Reck's skeleton became more complicated when Leakey brought new soil samples from Olduvai. Boswell and J. D. Solomon studied them at the Imperial College of Science and Technology. They reported their findings in the March 18, 1933 issue of *Nature*, in a letter signed also by Leakey, Reck, and Hopwood.

The letter contained this very intriguing statement: "Samples of Bed II, actually collected at the 'man site,' at the same level and in the immediate vicinity of the place where the skeleton was found consist of pure and wholly typical Bed II material, and differ very markedly from the samples of matrix of the skeleton which were supplied by Prof. Mollison from Munich" (L. Leakey et al. 1933, p. 397). This adds to our suspicion that the matrix sample supplied by Mollison to Boswell may not have been representative of the material closely surrounding Reck's skeleton.

Reck and Leakey, however, apparently concluded from the new observations that the matrix sample from Reck's skeleton was in fact some kind of grave filling, different from pure Bed II material. As far as we can tell, they offered no satisfactory explanation for their previous opinion that the skeleton had been found in pure, unmistakable Bed II materials.

Instead, both Reck and Leakey joined Boswell, Hopwood, and Solomon in concluding that "it seems highly probable that the skeleton was intrusive into Bed II and that the date of the intrusion is not earlier than the great unconformity which separates Bed V from the lower series" (L. Leakey et al. 1933, p. 397).

It remains somewhat of a mystery why both Reck and Leakey changed their minds about a Bed II date for Reck's skeleton. Perhaps Reck was simply tired of fighting an old battle against odds that seemed more and more overwhelming. At the time Reck had discovered his skeleton, many scientists were still somewhat uncertain about the evolutionary status of Dubois's Middle Pleistocene Java man. This left some room for controversial discoveries such as Reck's. But by the 1930s, after Black's discovery of Beijing man, the scientific community had become more uniformly committed to the idea that a transitional ape-man was the only proper inhabitant of the Middle Pleistocene. An anatomically modern *Homo sapiens* skeleton in Bed II of Olduvai Gorge did not make sense except as a fairly recent burial.

Leakey, almost alone, remained very much opposed to the idea that Java man (*Pithecanthropus*) and Beijing man (*Sinanthropus*) were human ancestors. In his discoveries at Kanam and Kanjera, he believed he had indisputable evidence for the presence of *Homo sapiens* in the same period as *Pithecanthropus* and *Sinanthropus* (and Reck's skeleton). So perhaps he abandoned the fight over Reck's highly controversial skeleton in order to strengthen support for his own recent finds at Kanam and Kanjera.

There is substantial circumstantial evidence in support of this hypothesis. In the issue of *Nature* (March 25, 1933) immediately following the one carrying Leakey's reversal on Reck's skeleton (March 18, 1933), there appeared the following notice in *Nature*'s "News and Views" section: "On March 18-19 a conference summoned by the Royal Anthropological Institute met at St. John's College, Cambridge, under the presidency of Sir Arthur Smith Woodward, to receive reports on the human skeletal remains discovered by Dr. Leakey's archaeological expedition to East Africa in the autumn of last year." We shall discuss the conference report later in this chapter (Section 11.2.3). For now, we simply note that Leakey's statement abandoning his previous position on the antiquity of Reck's skeleton appeared in *Nature* on the same day as the opening of a conference that would bear heavily on his reputation as a scientist.

In the March 18 issue of *Nature*, C. Stanton Hicks, of the University of Adelaide, Australia, complained about the disadvantages of practicing science in the outlying regions of the British Empire: "the old-established scientific societies with all their tradition and prestige, their facilities for publication and criticism of original work, and their influence in paving the way to higher posts, are in Great Britain." Leakey was a colonial, born and raised in British East Africa. In the conference convened to review his discoveries at Kanam and Kanjera, this promising young scientist's fate hung in the balance. He would perhaps be accepted into the elite circles of British science, and given a post at Cambridge, or perhaps be banished into obscurity, lucky to occupy a professorship in an outlying university. It is quite possible Leakey thought it best to withdraw his reputation from somebody else's controversial fossil and thus pave the way for acceptance of his own better-dated finds at Kanam and Kanjera. After all, some of the most vocal opponents of Reck's skeleton, such as Boswell, Solomon, Cooper, Watson, and Mollison, would be sitting on the committee that would review the Kanam Jaw and Kanjera skulls. As we shall see, the committee accepted the Kanam and Kanjera finds.

In his memoirs, Louis Leakey (1972, pp. 37-38) gave a brief and somewhat confusing review of his involvement with Reck's skeleton. He said the debate about the skeleton's age was resolved by a mineral analysis conducted by Boswell after a 1935 visit to Olduvai Gorge. This version is repeated, almost verbatim, in Cole's 1975 biography of Leakey. But, as far as we can tell, Boswell's mineral analysis was performed in 1933, and the results were reported in the March 18, 1933 letter to *Nature*, signed by Boswell, Leakey, Reck, Hopwood, and Solomon.

According to the new view outlined in the March 1933 letter, intact Beds III and IV at Olduvai Gorge were stripped away by erosion at the location of the skeleton. Bed II, thus exposed, would have probably still been covered by some remnants of Bed III and perhaps a thin layer of calcrete,

or steppe-lime. The burial supposedly took place at this time. Subsequent to the burial, the layers of Bed V, including thick, hard layers of calcrete, were deposited. The authors of the March 1933 letter said: "It seems certain that the skeleton was deposited where it was found before the main mass of Bed V, and the overlying steppe-lime were formed, that is, the skeleton appears to have been buried at the time of the existence of the old land surface connected with the steppe-lime at the base of Bed V" (L. Leakey et al. 1933, pp. 397-398).

This still gives a potentially anomalous age for the fully human Reck's skeleton. The base of Bed V is about 400,000 years old, according to current estimates (Table 11.1, p. 629). Therefore, even according to the revised position taken by Reck and Leakey, the skeleton could be at least 400,000 years old. This is true even if, as Boswell claimed in his August 1932 letter, the matrix sample supplied by Mollison contained deep red pebbles like those of Bed III and pieces of steppe-lime with a mineral characteristic of Bed IV. Today, however, most scientists believe that *Homo sapiens sapiens* first appeared about

100,000 years ago, as shown by the Border Cave discoveries in South Africa.

The March 1933 letter to *Nature* concluded with some interesting observations about stone tools found "in the basal deposits of Bed V" and on an "old land surface" at the same level as the steppe-lime just below Bed V. These tools, said the authors, had "very close affinities with the phase C of the Upper Kenyan Aurignacian" (Leakey et al. 1933, p. 398). Archeologists first used the term Aurignacian in connection with the finely-made artifacts of Cro-Magnon man (*Homo sapiens sapiens*) found at Aurignac, France. According to standard opinion, tools of the Aurignacian type did not appear before 30,000 years ago.

The Kenyan Aurignacian is now called the Kenyan Capsian, and the industry referred to above is called Upper Kenyan Capsian C. An Upper Kenyan Capsian C industry is found at Gamble's Cave, Kenya. Gamble's Cave is considered Holocene, or less than 10,000 years old (Oakley et al. 1977, pp. 36-37).

The presence of tools characteristic of anatomically modern humans just below Bed V and in the basal layers of Bed V at Olduvai Gorge is significant. The tools lend support to the idea that anatomically modern humans, as represented by Reck's skeleton, were present in this part of Africa at least 400,000 years ago. Alternatively, one could attribute the tools to *Homo erectus*. But this would mean granting to *Homo erectus* toolmaking abilities substantially greater than scientists currently accept.

In *The Stone Age Races of Kenya* (1935), Leakey repeated his view that Reck's skeleton had been buried into Bed II from a land surface that existed during the formation of Bed V. But now he favored a time much later in that period, contemporary with the Upper Kenyan Capsian C industry at Gamble's Cave. Rainer Protsch (1974, p. 382) wrote: "the contemporaneity was not based on association of the hominids in both localities with that culture, but on the association of one with that culture [at Gamble's Cave] and similar physical types of the hominids in both sites." In our discussion of discoveries made in China, we examined the practice of morphological dating. Here

again we see the primal)' role that the morphology of a skeleton plays in assigning it a date. And Leakey was not alone. Concerning the dating of Reck's skeleton, Protsch (1974, p. 382) noted: "Weinert [1934] argued against an early age of these Homo sapiens remains from a purely theoretical evolutionary point of view. "

In 1971, Mary Leakey repeated the position taken by her husband in *The Stone Age Races of Kenya*: "The skull is of Homo sapiens type and resembles those of the Kenya Capsian from Gamble's Cave II and Naivasha Railway Rockshelter in Kenya. A living site with a microlithic industry dated about 10,000 b.p. is known to exist within a short distance of the Olduvai burial and it is possible that the two are associated" (M. Leakey 1971, p. 225).

But even if the hypothesis that the skeleton was buried into Bed II during the deposition of Bed V is accepted, the skeleton could still be up to 400,000 years old. As mentioned above, that is when the post-Bed IV sediments began to accumulate at Olduvai. Other than its anatomically modern character, the Leakeys had little justification for assigning Reck's skeleton to recent rather than earlier Bed V times.

#### 11.1.5 The Radiocarbon Dating of Reck's skeleton

Reiner Protsch later attempted to remedy this situation by dating Reck's skeleton itself. Without such a determination, all that could truthfully be said (granting the Bed V burial hypothesis) was that the skeleton could be anywhere from 400,000 to perhaps a few thousand years old.

In 1929, Mollison had measured the organic content of Reck's skeleton under ultraviolet light, hoping to gain insight into its age. Sonia Cole (1975, p. 93), Leakey's biographer, said: "he found a great contrast between it and very recent bones on the one hand and the fossil fauna from Olduvai Bed II on the other." According to Cole, the differences in organic content indicated to Mollison that the bones were of different ages. In particular, Reck's skeleton would have to be younger than the other fossils found in Bed II. Mollison's ultraviolet measurements are said to have substantially influenced Leakey to change his mind about the antiquity of Reck's skeleton (Cole 1975, p. 93).

But Protsch contradicted Cole's statement, quoted above, that Mollison had found the organic content of Reck's skeleton to be different from that of the fauna from Bed II. Protsch (1974, p. 380) said Mollison had found "identical results for the organic content of the hominid and the fauna of Bed II." This demonstrates the difficulties one encounters in trying to unravel the truth about a case like this.

According to Protsch (1974, p. 380), Mollison obtained an organic content of 4.8 percent for the Olduvai human skeleton and 5.3 percent for a skull, only a few thousand years old, from the Ofnet Cave in Bavaria. Mollison used this determination to assign a date of approximately five thousand years to the Olduvai skeleton. Protsch later ran his own tests, using modern microanalytical methods to measure the amount of collagen, the main organic constituent of bone. He obtained an organic content of 2.7 percent for Reck's skeleton and

16.56 percent for the Bavarian skull. This invalidated the earlier determination by Mollison. Not much can be read into either set of results, because bones from different locations can lose their organic content at greatly different rates (Appendix 1.1).

Eventually, fragments of bone thought to belong to the original skeleton were dated by the radiocarbon method. Protsch (1974) obtained for his sample an age of 16,920 years.

The skull was considered too valuable to use for testing, and the rest of the skeleton had disappeared from a Munich museum during the Second World War. Protsch (1974, p. 383) stated: 'Through the courtesy of G. Glowatzki, director of the Staatssammlung, some very fragmentary postcranial material still imbedded in earth was found and used for radiocarbon dating. This material consisted mainly of rib fragments, long bone fragments, and pieces of vertebrae. Some of the bones were covered by the preservative Sapon, a lacquer, which was easily flaked off and removed. Many parts were not covered by this preservative, but nevertheless the same chemical pretreatment was given to all bone material. This postcranial material most likely belongs to the Olduvai Hominid I since it was marked as such. '

But if the bones were "still embedded in earth," how did they become fragmented? The original skeleton was said to be intact. Also, the hard Bed II material in which the skeleton was encased upon arrival in Germany was not exactly "earth." Reports of the discovery say the skeleton had to be removed with hammers and chisels, indicating the matrix was stonelike in hardness.

From the bone fragments available to him, Protsch was able to gather a sample of only 224 grams, about one third the normal size of a test sample for the method he used. Although he obtained an age of 16,920 years for the human bone, he apparently got very much different dates from other materials from the same site. "Several other radiocarbon dates were run, but could be contaminated by either recent or old radiocarbon, since these sample materials were mostly calcrete or fresh water shells," said Protsch (1974, p. 384). But the human fossil material may also have been contaminated by recent radiocarbon.

In Appendix 1. we discuss in detail the difficulties involved in radiocarbon dating of bones that have been exposed to contamination. By 1974, the remaining bone fragments from Reck's skeleton, if they in fact belonged to Reck's skeleton, had been lying around in a museum for over 60 years and had been soaked in an organic preservative (Sapon).

Protsch did not describe what chemical treatment he used to eliminate recent carbon 14 contributed by the Sapon. Thus we have no way of knowing to what degree the contamination from this source was eliminated.

In Appendix 1.3.2, we also describe other sources of contamination, including: (1) saprophytes growing in and feeding on bone, (2) humic and fulvic acids, (3) exogenous amino acids. (4) Improper collection procedures. Protsch (1974) did not discuss any of these sources of contamination or what procedures he used to try to eliminate them. All of these sources of

contamination, if not properly dealt with, would cause the carbon 14 test to yield a falsely young age.

The procedures employed today (Appendix 1.3.2.1) are much more exacting than those used by the radiocarbon laboratories that dated Reck's skeleton in the early 1970s.

The radiocarbon method is applied only to the collagen, or protein, fraction of the bone. This protein must be extracted from the rest of the bone by an extremely rigorous purification process.

Scientists then determine whether a sample's amino acids (the building blocks of proteins) correspond to those found in collagen. If they do not correspond, this suggests that amino acids may have entered the bone from outside. According to Jeffrey Bada (1985a, pp. 256-257), who conducted extensive research at Olduvai Gorge, bones can absorb amino acids from groundwater. These amino acids, being of a different age than the bone, could yield a falsely young radiocarbon date.

Even though a bone has a noncollagen profile, the amino acids could still be original to the bone (the collagen could have decayed, leaving only some of its constituent amino acids). In all cases, one should therefore date each amino acid separately. If any of the amino acids yield dates different from any of the others, this suggests the bone is contaminated and not suitable for carbon 14 dating.

Concerning the radiocarbon tests on Reck's skeleton reported by Protsch, the laboratories that performed them could not have dated each amino acid separately. This requires a dating technique (accelerator mass spectrometry) that was not in use in the early 1970s. Neither could these labs have been aware of the stringent protein purification techniques now deemed necessary.

Is it fair to subject Protsch's dating of Reck's skeleton to such retrospective criticism? After all, the requirements we are talking about were not in effect then. But if modern authorities are correct, and the rigorous purification and dating procedures outlined in Appendix 1.3.2.1 are actually necessary, then it is not unfair to measure Protsch's study against these standards. And when we do so, we can only conclude that the radiocarbon date Protsch gave for Reck's skeleton is unreliable. In particular, the date could very well be falsely young.

There are documented cases of bones from Olduvai Gorge giving falsely young radiocarbon dates. For example, a bone from the Upper Ndutu beds yielded an age of 3,340 years. The Upper Ndutu beds, part of what used to be called Bed V, are from 32,000 to 60,000 years old. A date of 3,340 years would thus be too young by at least a factor of ten. Bada (1985 a, p. 255) attributed the unexpectedly young radiocarbon date to deterioration of the bone's original collagen and contamination by secondary carbon compounds from the ground. The radiocarbon dating of Reck's skeleton is thus questionable.

From his radiocarbon date of 16,920 years, Protsch came to the conclusion that the skeleton had been buried in Bed II during the deposition of the upper part of Bed V, which formed after Beds III

and IV had been eroded (Protsch 1974, p. 384). Uppermost Bed V, now called the Naisiuslu formation, also yielded fossil material (an ostrich egg shell) with a radiocarbon date of approximately 17,000 years.

Nevertheless, burial from upper Bed V times still seems somewhat problematic. From the reports of Leakey, Hopwood, and others, it is apparent that as little as a few hundred years ago, the spot where Reck's skeleton was found would have been covered by intact Bed V. How much of Bed V is hard to tell, but Louis Leakey (1932b) reported that between 1913 and 1931 the land surface at the site had eroded 6 inches. And at the base of Bed V there would have been a hard layer of calcrete, 10 to 12 inches thick.

Furthermore, Bed II itself was quite hard. According to the original reports of the excavation, Reck's skeleton had to be taken out with the aid of hammers and chisels. It hardly seems likely that primitive tribal people would have engaged in the arduous efforts necessary to dig a grave in such resistant rock. One way around this difficulty is to suppose, contrary to the geological evidence, that 17,000 years ago the Bed V calcrete was not present and that the Bed II sediments were still soft a million years after they were deposited—a highly improbable scenario.

Protsch also reported uranium content test results of 3 parts per million for both some middle Bed V faunal remains and Reck's skeleton (now called Olduvai Hominid 1). But he correctly pointed out that the specimens were from different localities, which reduces the value of the comparison. Uranium isotopes may accumulate at vastly different rates in different localities (Protsch 1974, p. 384).

Protsch (1974, pp. 382-383) said stone tools had been discovered in the Naisiuslu Beds, corresponding to the upper part of the old Bed V (Table 11.1, p. 629). These were somewhat like those found at Gamble's Cave (Upper Kenyan Capsian C), some distance away from Olduvai. At Gamble's Cave, skeletons of *Homo sapiens sapiens* had been found, like Reck's skeleton, in a contracted burial position (Protsch 1974, p. 381). Protsch thought these facts lent support to a Naisiuslu Bed origin for Reck's skeleton. But in 1933, Louis Leakey had said that stone tools of the Upper Kenyan Capsian C type had been found just below and in the basal layers of Bed V (Leakey et al. 1933, p. 398). This could be taken as evidence that men like those of Gamble's Cave (*Homo sapiens sapiens*) existed at least 400,000 years ago in this part of Africa.

But Protsch (1974, p. 382) said about Reck's skeleton: "Theoretically, several facts speak against an early age of the hominid, such as its morphology." This suggests that the skeleton's modern morphology was one of the main reasons Protsch doubted it was as old as Bed II or even the base of Bed V.

If Reck's skeleton were classified as *Homo erectus*, it is hard to imagine that anyone would now be raising any serious objections to its presence in Bed II. In 1960, a *Homo erectus* cranium (OH 9) was found on the surface at Olduvai Gorge (Poirier 1977, p. 223). It was nevertheless assigned to upper Bed II, giving it an age of over 1 million years. Adhering to the base of the cranium was a

matrix matching that of Bed II. Yet a determined debunker could always attribute this to "secondary cementation. "

What about the observations by Leakey (1932a, p. 721) and Mollison (Protsch 1974, p. 380) that the human bones are fossilized to the same degree as the animal bones found nearby in Bed II? Protsch (1974, p. 382) said that "a relative age determined by the state of fossilization of bones is Invalid for a positive chronological diagnosis." Here, we agree with Protsch.

Yet scientists have used such determinations of relative age, as measured by differences in fluorine, uranium, or nitrogen content, to discredit many of the anomalously old *Homo sapiens* fossils we have discussed (such as Galley Hill). They also have used relative age determinations to date many accepted finds. But in the case of Reck's skeleton, Protsch said such results have no value.

Curiously enough, Protsch himself used a relative age determination to confirm that the bone fragments he had tested were actually from Reck's skeleton. Protsch (1974, p. 383) reported: "to check whether the skull and the fragmentary bones belonged together, two separate microanalytical tests were made on the skull and some post cranial primary bone. The [nitrogen] values of 0.45% (British Museum) and 0.43% (UCLA) are remarkably similar and give support to a positive association of the bones. " So to be fair, if the Bed II faunal remains and Reck's skeleton were fossilized to a similar degree (Leakey 1932a, p. 721), could not this also be taken as supporting (although not proving) a 'positive association of the bones?'"

All in all, Protsch appears to have done a needed service—the cleaning up of a problem discovery, fitting it nicely into the accepted evolutionary sequence. By 1974, it is clear, no one in the mainstream of human evolutionary thought was prepared to accept a fully modern human being existing at least 400,000 years ago, contemporary with *Homo erectus*. Protsch himself admitted that his theoretical expectations ruled this out. By giving a plausible carbon 14 date and identifying the skeleton with tools found nearby in upper Bed V and with skeletal remains found at Gamble's Cave along with similar tools, Protsch put Reck's skeleton in an appropriate paleoanthropological niche. The case was closed.

But the case made by Protsch in favor of a Late Pleistocene burial was very weak. First of all, it is not at all certain that the bone sample he tested actually belonged to the original Reck's skeleton, which, except for the skull, disappeared during the Second World War. Furthermore, the carbon 14 method is not infallible, especially when applied to bones that were exposed to contamination for over

60 years. It is also possible that the bones were contaminated with recent carbon while they were buried in the ground at Olduvai Gorge. And, as we have seen, the radiocarbon dating methods employed by Protsch have been superseded by more rigorous procedures.

#### 11.1.6 Probable Date Range of Reck's skeleton



We are now left with several alternative explanations, which we shall now summarize, for the age of Reck's skeleton. First we have the original determination by Reck that it was deposited naturally during the formation of Bed II. Reck carefully searched for signs of Intrusive burial (especially chips of limestone and other materials from the overlying beds) and found none "despite the most attentive inspection" (Hopwood 1932, pp. 193-194). This gives a date of over 1.15 million years for the skeleton, which is fully human. Second, we have Leakey's view that the skeleton was deliberately buried during the deposition of upper Bed II, which also gives a date of over 1.15 million years. Third, we have the revised position, taken by Reck, Leakey, and others, that the skeleton was buried into Bed II during the time Bed V was being deposited. In adopting their revised position, Reck and Leakey in effect reversed their previous statements that they had observed no mixture of materials from overlying beds in the matrix of the skeleton. It is significant that Leakey recanted his position on Reck's skeleton just before a commission of scientists, including the critics of his prior views on Reck's skeleton, was to pass judgement on his own discoveries at Kanam and Kanjera. The new position adopted by Reck, Leakey, and others yields a date range of from 400,000 to perhaps 10,000 years for the skeleton. Primarily on the basis of its modern morphology, the skeleton was assigned a very recent date within this range. During the Second World War, much of the skeleton was lost. Finally, in 1974, in an attempt to confirm an uppermost Bed V date, Protsch published a radiocarbon test result of about 17,000 years for a bone sample that may not have been from the original Reck skeleton. Even if the sample was from Reck's skeleton, the dating techniques that were used are now considered unreliable.

In our discussion of China, we introduced the concept of a probable date range (Section 9.2.1) as the fairest age indicator for controversial discoveries. The available evidence suggests that Reck's skeleton (OH I) should be assigned a probable date range extending from the late Early Pleistocene (1.15 million years) to the late Upper Pleistocene (10,000 years). There is much evidence that argues in favor of the original Bed II date proposed by Reck. Particularly strong is Reck's observation that the thin layers of Bed II sediment directly around the skeleton were undisturbed. Also arguing against later burial is the rocklike hardness of Bed II. Reports favoring a Bed V date seem to be founded upon purely theoretical objections, dubious testimony, inconclusive test results, and highly speculative geological reasoning. But even these reports yield dates of up to 400,000 years for the skeleton.

A skeleton of *Homo sapiens sapiens* type with an age of 1.15 million years, or even .4 million years, does not fit the current evolutionary scenario. But Reck's skeleton does not seem out of place when seen in the context of the evidence documented in this book. This evidence demonstrates the presence of anatomically modern humans throughout the Early Pleistocene, Pliocene, Miocene, and even earlier. Only the radiocarbon date reported by Protsch suggests Reck's skeleton might be fairly recent, but as we have seen, this date has its problems.

It would undoubtedly take a time-traveling detective with supersensory powers to give us the real story of Reck's skeleton and its age. And Reck's skeleton is not exceptional. Most of the discoveries

scientists have used to build up their picture of human evolution are similarly ambiguous, their significance obscured by professional rivalries and Imperfect Investigative methods.

## 11.2 The Kanjera Skulls and Kanam Jaw

In 1932, Louis Leakey announced discoveries at Kanam and Kanjera, near Lake Victoria In western Kenya. The Kanam Jaw and Kanjera skulls, he believed, provided good evidence of Homo sapiens In the Early and Middle Pleistocene.

### 11.2.1 Discovery of the kanjera skulls

Kanjera lies on the south shore of Lake Victoria's Kavirondo Gulf. When Leakey visited Kanjera In 1932 with Donald MacInnes, they found stone hand axes and fragments of five human skulls. designated Kanjera 1-5. Leakey (1960d, p. 204) said: "I found part of No.3 specimen insitu myself, and I have no doubt about Its genuineness." The expedition also found a human femur.

According to Leakey, the fossil-bearing beds at Kanjera were equivalent to Bed IV at Olduvai Gorge. The faunal studies of

H. B. S. Cooke (1963) confirmed this, which means the Kanjera beds range from 400,000 to 700,000 years old (Table 11.1, p. 629). But the morphology of the Kanjera skull pieces was quite modern. Leakey (1960d, p. 203) wrote: "The front part of the skull Is preserved, In a damaged condition, In two of the specimens, and from this we can see that there was no trace of a bony brow-ridge above the eyes. Instead we find a very small and simple form much as In a child, but certainly of Homo sapiens type." Scientists now think modern Homo sapiens appeared about 100,000 years ago In Africa. An age of 400,000 years for the Kanjera skulls would, however, be acceptable for the oldest African early Homo sapiens (Brauer 1984, p. 394). But the author of a recent survey attributed the Kanjera skulls to Homo sapiens cf. sapiens (Groves 1989, p. 291), Indicating they are anatomically modern.

### 11.2.2 Discovery of the kanamjaw

At Kanam, Leakey Initially found teeth of Mastodon and a single tooth of Deinotherium (an extinct elephantlike mammal), as well as some crude stone Implements. Because Deinotherium was the marker fossil for Bed I at Olduvai Gorge, Leakey believed the Kanam formations were of the same Early Pleistocene age—about 1.7-2.0 million years old, according to current estimates (Oakley et al. 1977, pp. 166. 169).

On March 29, 1932, Leakey's collector, Juma Gitau, brought him a second In the same spot. Working a few yards from Leakey, Gitau hacked out a block of travertine (a hard calcium carbonate deposit) and broke It open with a pick. He saw a tooth protruding from a piece of travertine and showed It to MacInnes, who Identified the tooth as human. MacInnes summoned Leakey. Together they searched for more human fossils, but none turned up (L. Leakey 1960d, p. 202).

Upon chipping away the travertine surrounding Gitau's find, they saw the front part of a human lower Jaw with two premolars. Leakey thought the Jaw from the Early Pleistocene Kanam formation was much like that of *Homo sapiens*. and he announced its discovery in a letter to *Nature* (Cole 1975, p. 91). According to Cooke (1963), the Kanam fauna is older than that of Bed I at Olduvai Gorge, making the Kanam beds at least 2.0 million years old.

Almost without exception, today's scientists believe that the human lineage extends from *Australopithecus* in the Late Pliocene and Early Pleistocene, to *Homo erectus* in the Early and Middle Pleistocene, and thence to *Homo sapiens sapiens* in the Late Pleistocene. Against this background, a *Homo sapiens* Jaw in the earliest Pleistocene seems strangely out of place. But in the early 1930s, the now dominant view of human origins, although held by some, was still a somewhat tentative hypothesis. A good many British scientists regarded *Australopithecus*, discovered in 1925 by Raymond Dart in South Africa, as a variety of ape, with no direct connection to the line of human descent. Similarly, many scientists never accepted Java man, and Beijing man had only newly arrived on the scene. Although some scientists did believe Java man and Beijing man, now classified as *Homo erectus*, were genuine human ancestors, the picture was somewhat clouded by Piltdown man. Whereas Java man and Beijing man had humanlike Jaws and apelike skulls, Piltdown man, roughly the same age, featured an apelike Jaw and humanlike skull. The Neanderthals also had to be fit into the picture. Some thought they were direct human ancestors, others thought they were on an evolutionary side branch. In short, scientists of the 1930s held varying views about the progress of human evolution and the antiquity of the modern human type. Therefore, not all of them would automatically reject finds such as Leakey made at Kanam and Kanjera.

For Leakey, the Kanam and Kanjera fossils showed that a hominid close to the modern human type had existed at the time of Java man and Beijing man, or even earlier. If he was correct, Java man and Beijing man (now *Homo erectus*) could not be direct human ancestors, nor could Piltdown man with his apelike Jaw.

### 11.2.3 A commission of scientists Decides on Kanam and Kanjera

On March 18 and March 19, 1933, the human biology section of the Royal Anthropological Institute met to consider Leakey's discoveries at Kanam and Kanjera. Chaired by Sir Arthur Smith Woodward, 28 scientists issued reports on four categories of evidence: geological, paleontological, anatomical, and archeological (Woodward et al. 1933, pp. 477-478). The geology committee concluded that the Kanjera and Kanam human fossils were native to the beds in which they were found. The paleontology committee said the Kanam beds were Early Pleistocene, while the Kanjera beds were no more recent than Middle Pleistocene. The archeology committee noted the presence at both Kanam and Kanjera of stone tools in the same beds where the human fossils had been found.

The anatomical committee said the Kanjera skulls exhibited "no characteristics inconsistent with the reference to the type *Homo sapiens* (Woodward et al. 1933, p. 477). The same was true of the Kanjera femur.

About the Kanam Jaw, the anatomy experts said: "With the possible exceptions of the thickness of the symphysis, the conformation of the anterior Internal surface, and what seems to be a large pulp-cavity of the first right molar tooth, the Committee is not able to point to any detail of the specimen that is incompatible with its inclusion in the type of the *Homo sapiens*" (Woodward et al. 1933, p. 478). The symphysis, the joint between the two halves of the lower jaw, runs down the middle of front part of the jaw.

The species designation *Homo sapiens*, as employed today by most, although not all, paleoanthropologists, includes early

*Homo sapiens*, *Homo sapiens neanderthalensis*, and *Homo sapiens sapiens* (fully modern humans). But in 1933, the Neanderthals were generally considered distinct from *Homo sapiens*, and the first representatives of early *Homo sapiens*, as presently conceived by many workers, either had not been discovered or had not been reported to the scientific world. The first report on the Steinheim skull, discovered in 1933, came out in 1935. And the Swanscombe skull fragments were not found until 1935 and 1936. So when the members of the anatomical committee classed the Kanjara skulls and Kanam Jaw as *Homo sapiens*, they presumably meant they were within the range of anatomically modern humans.

Although the committee stated that the remains could be classified as *Homo sapiens*. Leakey assigned the jaw to a new species, *Homo kanamensis*, which he considered the immediate ancestor of *Homo sapiens*. According to Cole (1975, pp. 103-104), Leakey later dropped the name *kanamensis* in favor of *sapiens*.

#### 11.2.4 Boswell strikes Again

Shortly after the 1933 conference gave Leakey its vote of confidence, geologist Percy Boswell began to question the age of the Kanam and Kanjara fossils. Leakey, who had experienced Boswell's attacks on the age of Reck's skeleton, decided to bring Boswell to Africa, hoping this would resolve his doubts. But it did not go well.

Upon returning to England, Boswell (1935) submitted to *Nature* a negative report on Kanam and Kanjara: "Unfortunately, it has not proved possible to find the exact site of either discovery, since the earlier expedition (of

1931-32) neither marked the localities on the ground nor recorded the sites on a map. Moreover, the photograph of the site where the mandible was found, exhibited with the jaw fragment at the Royal College of Surgeons, was, through some error, that of a different locality." Having examined Leakey's original field notes, Boswell (1935) said "it is regrettable that the records are not more precise."

Boswell found the geological conditions at the sites confused. He said that "the clayey beds found there had frequently suffered much disturbance by slumping." From this Boswell (1935) concluded: "The date of entombment of human remains found in such beds would be inherently doubtful."

But what about the committee that had given Leakey its endorsement? "It seems likely," said Boswell (1935) "that if the facts now brought forward had been available to the Committee, a different report would have been submitted." Boswell concluded that the "uncertain conditions of discovery . . . force me to place Kanam and KanJera man in a 'suspense account.'"

#### 11.2.5 Leakey Responds

Replying to Boswell's charge that he had not properly marked the sites, Leakey (1936) stated in a letter to Nature that he had in fact done so. Unfortunately, the iron pegs he used had disappeared, perhaps taken by natives for spearheads or fishhooks. He had not marked the sites on a map, but only because no maps of sufficient detail existed. He had considered hiring a surveyor to make maps, but had not done so because of lack of money. Instead, he had taken photographs to identify the sites, but these had been spoiled by a malfunction in his camera.

His own photographs of Kanam and KanJera ruined, Leakey had selected some by Miss Kendrick, a member of his expedition, to display with his fossils in England. In his letter to Nature, Leakey explained he had misinterpreted the label on one of Kendrick's photographs and had mistakenly used it to show the site where the Kanam Jaw had been found. But he pointed out: "I carefully refrained from using any photographs as evidence in connexion with my claim for the antiquity of the Kanam mandible, and only used them to show the general nature of the sites" (L. Leakey 1936).

Furthermore, Leakey felt he had been able to show Boswell the locations where he had found his fossils. Leakey (1936) wrote: "At KanJera I showed him the exact spot where the residual mound of deposits had stood which yielded the KanJera No. 3 skull in situ. . . . the fact that I did show Prof. Boswell the site is proved by a small fragment of bone picked up there in 1935 which fits one of the 1932 pieces.'

Regarding the Kanam Jaw, Leakey stated in his memoirs: "It had been found in direct association with Lower Pleistocene fossils such as Deinotherium and Mastodon, and the matrix adhering to it was entirely similar to that which Boswell had now seen in the Kanam West gullies" (L. Leakey 1972, p. 35). Boswell did not mention the matrix adhering to the Jaw in his letter to Nature.

Leakey (1972, p. 35) added: "Boswell, however, remained doubtful because no scientist had seen the Jaw in situ. He would not agree to accept Juma's statement that it had been dug out while he was working on the Deinotherium tooth." Of course, if this standard were to be applied across the board, then many thoroughly accepted discoveries would also have to be thrown out. The Heidelberg (Mauer) Jaw, for example, was discovered by a German sand pit worker. And almost all of the Java man discoveries reported by von Koenigswald were found by native collectors.

Regarding the location of the Kanam jaw, Leakey (1972, p. 35) said: "we had originally taken a level section right across the Kanam West gullies, using a Zeiss-Watts level, and could therefore locate the position to within a very few feet—and, in fact, we did so. I had brought with me a copy of the cross section, taken from a tree that could still be located on one side of the gully to another tree

on the other side. On this cross section was a mark showing the point where the Jaw had been recovered. I had, therefore, no doubt at all that I was showing Boswell and Wayland the right place within a few feet."

Boswell suggested that even if the jaw was found in the Early Pleistocene formation at Kanam, it had entered somehow from above—by "slumping" of the strata or through a fissure. To this Leakey (1960d, pp. 202-203) later replied: "I cannot accept this interpretation, for which there is no evidence. The state of preservation of the fossil is in every respect identical to that of the Lower Pleistocene fossils found with it. Had the Kanam mandible been a specimen representing some specialized extinct type of man (such as used to be called 'primitive') no one would have suggested that it was not contemporary with the other fossils of the same horizon. ... the fact that the Kanam mandible has a distinct chin eminence certainly influenced some people against accepting its authenticity."

Boswell's preconceptions about the morphology of hominids in the Early Pleistocene apparently motivated his attacks on the age of the Kanam jaw, and of Reck's skeleton (Section 11.1.4). Leakey (1972, pp. 35-36) said in his memoirs: "he actually told us that were it not for the counterindication provided by the Piltdown jaw, which showed that man in the Lower Pleistocene had a simian shelf and extremely apelike characteristics, he would be inclined to accept the Kanam evidence, since the mineralization of the specimen compared closely with that of other fossils from the same deposits." Of course, British scientists later declared the Piltdown jaw to be a fake (Chapter 8).

#### 11.2.6 Kanam and Kanjera after Boswell

Despite Boswell's attacks on the Kanam and Kanjera finds, a few well-known scientists continued to keep open minds about Leakey's original claims. Robert Broom, who in the 1930s found the first adult specimens of *Australopithecus*, wrote (1951, p. 13): "I have looked into this controversy very carefully and have no hesitation in saying that I have the fullest confidence in Leakey's work: I am quite satisfied that Leakey found these remains where he says he found them, and that they prove modern man is far older than a few English scientists had thought—perhaps even as old as the Lowest Pleistocene." Broom's use of the words "modern man" to describe the Kanam and Kanjera fossils suggests he regarded them as similar to *Homo sapiens sapiens*. Broom (1951, pp. 11-12) characterized the Kanjera fossils as "skulls of early man with a large brain, and without any of the characters of Neanderthal man." Standard texts give a lot of attention to Broom's *Australopithecus* finds, but usually fail to mention his unorthodox views on Kanam and Kanjera.

Philip V. Tobias of South Africa said about Kanjera (1968, p. 182): "Boswell did not disprove the claim that human fragments were found in a Middle Pleistocene deposit; he only failed to find additional evidence confirmatory of Leakey's claim. Thus there is a good *prima facie* case to reopen the question of Kanjera."

And the Kanjera case was in fact reopened. Leakey's biographer Sonia Cole (1975, p. 358) wrote: "In September 1969 Louis attended a conference in Paris sponsored by UNESCO on the theme

of the origins of *Homo sapiens*. . . the 300 or so delegates unanimously accepted that the Kanjera skulls were Middle Pleistocene."

Leakey originally suggested that the fossil-bearing formation at Kanjera was equivalent to Olduvai Bed IV, which is approximately 400,000 to 700,000 years old (early to middle Middle Pleistocene). By 1960, however, Leakey had modified his position. He said the Kanjera skulls were the same age as the Swanscombe skull (L. Leakey 1960d, p. 204), which is about 300,000 years old. In the paper Leakey presented at the UNESCO conference, he maintained his view that the deposits at Kanjera and Swanscombe were "of comparable age." ' But as we have seen, H. B. S. Cooke (1963), a leading authority on African mammals, confirmed Leakey's original view that the Kanjera beds were the same age as Olduvai Bed IV. In his Paris paper, Leakey (1971, p. 26) also asserted that the Kanjera skulls had "brow-ridges of modern *Homo sapiens* appearance

Tobias (1962, p. 344) said about the Kanamjaw: "Nothing that Boswell said really discredited or even weakened the claim of Leakey that the mandible belonged to the stratum in question, nor did Boswell deny the faunal and cultural associations previously attributed to this stratum. . . a number of subsequent writers have gratuitously assumed that Boswell's report invalidated all Leakey's claims. Although Leakey answered some of Boswell's specific criticisms, the reply has seldom been quoted and little cognizance has seemingly been taken of it." ' But, as we shall see below, Tobias had his own ideas about the age and evolutionary status of the Kanamjaw.

#### 11.2.7 Morphology of the Kanamjaw

Scientists have described the Kanam Jaw in a multiplicity of ways. In 1932, a committee of English anatomists proclaimed it *Homo sapiens* (Woodward et al.

1933). Louis Leakey initially attributed the Jaw to a new species, *Homo kanamensis*, a direct ancestor of *Homo sapiens*. But his biographer Sonia Cole (1975, pp. 103-104) said he soon gave up that designation in favor of *Homo sapiens*. Sir Arthur Keith (1935, p. 163), the dean of British anthropologists, also considered the Kanam Jaw *Homo sapiens*. But in the 1940s Keith decided the Jaw was most likely from an australopithecine (Tobias 1968, p. 180).

Tobias, an expert on the Australopithecinae, disagreed. After comparing the Kanam Jaw with available Australopithecus Jaws, Tobias (1968, p. 181) found it was, among other things, "much less robust" and different in the "general conformation and orientation" ' of the front part of the Jaw.

Tobias (1962, p. 341) suggested that some of the sapiens-like features of the front part of the Kanam Jaw might be, at least partially, the result of bone growth in response to a tumor on the inner surface of the front part of the Jaw. Tobias was, however, not the first to notice the tumor.

Almost 20 years earlier, Sir Arthur Keith (1935, p. 163) wrote: "the chin of this representative of early humanity was the seat of a bony tumour of an exceedingly rare kind. The tumour, which grew from the deep aspect of the Jaw, just behind the chin, has spread over and obscured the normal features of this region. Enough remains, however, to make quite certain that in dimensions and in

its features, the chin region of this early being was shaped as in primitive types of living humanity— such as the aborigines of Australia." In other words, Keith, at that time, took the chin features to be within the range of anatomically modern humans, *Homo sapiens sapiens*.

Despite the effects of the bone tumor on the inner surface of the chin, Tobias (1962, p 349) thought the lower front part of the Kanam Jaw had some features like that of the modern human chin—although not as well developed. For example, the Kanam Jaw, like the human Jaw, has a pronounced incurvation below the level of the teeth and an outward swelling of the bone at the base of the front part of the Jaw (Figures 11.4g-h).

But Tobias also called attention to the depth and thickness of the Jaw, the relatively large size of some of the teeth, and other features that he regarded as primitive. Tobias (1962, p. 355) observed: "Several, though not all, of these features might be encountered individually as exceptional variants among modern African mandibles." He thought the Kanam Jaw most closely resembled the late Middle Pleistocene mandible from Rabat in Morocco, and Upper Pleistocene mandibles such as those from the Cave of Hearths in South Africa and Dire-Dawa in Ethiopia (Tobias 1968, p. 181).

Recent workers class Rabat and Cave of Hearths as "early archaic *Homo sapiens*" (Brauer 1984, pp. 380, 394). The Rabat mandible is said to have no true chin (Howell 1978, pp. 196, 204), while the Cave of Hearths mandible is said to have "a slight to moderate chin" (Tobias 1971, p. 338). The Dire-Dawa mandible, said to have no true chin, is nevertheless listed as *Homo sapiens sapiens* (Howell 1978, p. 214).

According to Tobias (1968, pp. 190-191), all of these mandibles displayed "neanderthaloid" features. He placed them, along with other neanderthaloid fossils, in the subspecies *Homo sapiens rhodesiensis*, which he regarded as transitional between *Homo erectus* and more developed African Neanderthals.

In 1960, Louis Leakey (1960d, p. xix), retreating from his earlier view that the Kanam Jaw was sapiens-like, wrote: "it becomes highly probable that the Kanam mandible represents, in fact, a female of *Zinjanthropus*."

Leakey had found *Zinjanthropus* in 1959, at Olduvai Gorge (Section 11.4.1). He briefly promoted this apelike creature as the first toolmaker, and thus the first truly humanlike being. Shortly thereafter, fossils of *Homo habilis* were found at Olduvai. Leakey quickly demoted *Zinjanthropus* from his status as toolmaker, placing him among the robust australopithecines (*A. boisei*).

In the early 1970s, Leakey's son Richard, working at Lake Turkana, Kenya, discovered fossil Jaws of *Homo habilis* that resembled the Kanam Jaw. Since the Lake Turkana *Homo habilis* Jaws were discovered with a fauna similar to that at Kanam, the elder Leakey changed his mind once more, suggesting that the Kanam Jaw could be assigned to *Homo habilis* (L. Leakey 1972, p. 36; Cole 1975, p. 362).



That scientists have attributed the Kanam Jaw to almost every known hominid (Australopithecus, Australopithecus boisei, Homo habilis, Neanderthal man, Early Homo sapiens, anatomically modern Homo sapiens) shows the difficulties involved in properly classifying hominid fossil remains.

Tobias's suggestion that the Kanam Jaw came from a variety of early Homo sapiens, with neanderthaloid features, has won wide acceptance. Yet as can be seen in Figure 11.4, which shows outlines of the Kanam mandible and other hominid

mandibles, the contour of the Kanam mandible's chin region is similar to that of the Border Cave specimen (f), recognized as Homo sapiens sapiens, and to that of a modern South African native (g). All three share two key features of the modern human chin, namely, an invagination toward the top and a swelling outward at the base.

Figure 11.4 The outlines of the mandibles shown here (not to scale) were traced from published photographs, except for (a) and (g), which were traced from a drawing. (a) *Australopithecus*, Omo, Ethiopia (Eckhardt 1972, p. 103); (b) *Homo erectus*, Heidelberg (Mauer), Germany (Osborn 1916, p. 98); (c) Early *Homo sapiens*, Arago, France (Stringer et 1984, p. 64); (d) Neanderthal, Shanidar, Iraq (Gowlett 1984, p. 104); (e) *Homo sapiens rhodesiensis* ("neanderthaloid" according to P. V. Tobias), Cave of Hearths, South Africa (Tobias 1971, p. 338); (f) *Homo sapiens sapiens*, Border Cave, South Africa (Brauer 1984, p. 381); (g) *Homo sapiens sapiens*, modern South African native (Zuckerman 1954, p. 308); (h) the Kanam mandible (Tobias 1962, p. 345).

But even if one were to accept Tobias's view that the Kanam Jaw was neanderthaloid, one would still not expect to discover Neanderthals in the Early Pleistocene, over 1.9 million years ago, Neanderthaloid hominids came into existence at most 400,000 years ago (Brauer 1984, p. 394) and persisted until about 30,000 or 40,000 years ago, according to most accounts, We note that some workers (Brauer 1984) confine the Neanderthal line to Eurasia and a small area of North Africa adjacent to Europe, These workers would not expect to find Neanderthals at Kanam in East Africa,

#### 11.2.8 Chemical Testing Of the Kanam And Kanjera Fossils

To ascertain the age of the Kanam Jaw and Kanjera skulls, K. P. Oakley of the British Museum performed fluorine, nitrogen, and uranium content tests,

The Kanam Jaw and the Kanjera skulls had about the same fluorine content as other bones from the Early and Middle Pleistocene formations where they were found (Oakley 1974, p. 257; 1975, p. 151), But Oakley (1974, p. 257) pointed out that "in volcanic areas (particularly under tropical conditions) fluorine analysis does not provide a reliable method of relative dating," If this is so, one wonders why he ran the tests. Nevertheless, the results he reported are consistent with the hypothesis that the human bones at Kanam and Kanjera are as old as the faunal remains at those sites, Of course, the agreement in fluorine content might, as Oakley suggested, be the result of uneven fluorine absorption in a volcanic, tropical environment. But then again, it might not.

Oakley (1974, p, 257) found that a KanJera 4 skull fragment showed Just a trace of nitrogen {0,01 percent), while a KanJera 3 skull fragment showed none, Neither of the two animal fossils tested showed any nitrogen, The presence of "measurable traces" of nitrogen in the KanJera 4 skull fragment meant, said Oakley (1974, p, 258), that all the human fossils were "considerably younger" than the KanJera fauna,

But certain deposits, such as clay, preserve nitrogen, sometimes for millions of years (Appendix 1.1.2), So perhaps the KanJera 4 fragment was protected from nitrogen loss by clay, In any case, the KanJera 3 fragment, like the animal samples, had no nitrogen, It is possible that this human bone was younger than the animal bones, and lost its nitrogen fairly quickly, But the test results do not dictate this interpretation—the bones could be the same age,

As shown in Table 11.2, the uranium content values for the KanJera human fossils (8-47 parts per million) overlapped the values for the KanJera fauna (26-216 parts per million), This could mean they were of the same age,

But the human bones averaged 22 parts per million while the mammalian fauna averaged 136 parts per million, To Oakley (1974, p, 257), the substantial difference between the averages meant that "the KanJera hominids, although fossilized (Upper Pleistocene?), are considerably younger than the KanJera faunal stage (Middle Pleistocene)," Similar uranium contents results were obtained at Kanam, The Kanam mandible had 4-12 parts per million eU 0 , while the Kanam fauna had 60-214 parts per million (Oakley 1975, p, 151), "The low radiometric values of the Kanam Jaw fragment strongly suggest that it is younger than the Kanam fauna," said Oakley (1975, p, 151),

TABLE 11.1.

#### T<sup>ra</sup>Dium Content of KanJera Hominid Fossils

Fossil

Identification

Description of Fragment Tested

Uranium (eU<sub>0</sub>:J (parts per 1<sup>oo</sup>)

Kanijera 3

oroitaJ. fragment, in situ

right pme^ fragment, in situ

2.1

fragments from

16,27, 27, 30, 42

fragment from

8,14

^Kanjera 4

frfrontal fragments from s^fcce

11, 21, 35

mandat fra^^te

2.6,131, 146. 159,216

The data in til table are OOakley (1974, p. IS7).

While the uranium content values, as reported, are consistent with the KanJeran and Kanam faunas being older than the human bones, there are reasons for caution. The values reported for the KanJeran fauna—26, 131, 146, 159, and 216 parts eU308 per million—vary widely. The highest value is 8.3 times greater than the lowest, although the bones are supposedly of the same general age. Also, the uranium content of the KanJera 3 human fossils ranged from 8 to 42 parts per million, differing by a factor of 5 in a single individual. The high and low values for the Kanam fauna vary by a factor of 3.5, and for the Kanamjaw itself by a factor of 3. This reinforces our observation (Appendix 1.2.4) that the rate at which a bone absorbs uranium depends on many highly variable conditions—such as the concentration of uranium in the groundwater, the rate of groundwater flow, and the nature of the surrounding sediment. Also, different kinds of bone (and, apparently, even different parts of the same bone) may absorb uranium at greatly different rates. All of this tends to reduce the value of uranium content as a relative age indicator.

Oakley himself pointed out: "the distribution of uranyl ions in ground-water, like that of fluorine ions is subject to very considerable variation from place to place ... It appears that fossil bones of Upper Pleistocene or early Holocene age in Kugata near Mount Homa [close to Kanam] not only

contain more fluorine than bones of Lower Pleistocene age at Kanam, but are more radioactive on account of adsorbed uranium" (Tobias 1968, p. 181).

Leakey reported that some of the KanJera human skull fragments, now classified *Homo sapiens cf. sapiens* (Groves 1989, p. 291), were found *insitu* in the Middle Pleistocene Kanjeran deposits. Oakley (1974, p. 257), however, explained: "When deposits such as the KanJera beds become waterlogged during the wet season, bones lying on the surface become readily incorporated, so that when subsequently discovered they can easily have the appearance of occurring *in situ*." Is this what actually happened? Maybe yes, maybe no.

Oakley had to stretch even further to account for the Kanam Jaw. After pointing out that Tobias had said the Kanam jaw was comparable to the Middle Pleistocene Rabat jaw, Oakley (1975, p. 152) said: "I suggest that during some interval in Middle Pleistocene times the jaw lay on a surface littered with fossils weathered out from the Kanam beds and it became embedded with these derived fossils in a block of surface limestone which was eventually down-faulted or trapped in a fissure penetrating the Kanam beds. This would explain the low uranium content and the high degree of calcification, and at the same time take into account L. S. B. Leakey's statement in his memoirs . . . that Juma Gitau discovered the Kanam jaw fragment while engaged on extracting a molar of tooth of *Deinotherium*. As he expressed it to me: 'the jaw was in the same

block as an undoubted Lower Pleistocene fossil.' "

Oakley had no trouble inventing special geological scenarios to explain away the stratigraphic evidence. But he offered no proof, such as positive signs of faulting, that these scenarios were correct. Operating as Oakley did, one can easily dispose of any unwanted stratigraphic evidence whatsoever.

But even if we do grant stratigraphic resorting, this does not necessarily show that the hominid fossils at Kanam and Kanjera were younger than the mammalian fossils at these sites. For example, Tobias (1968, p. 181) said: "The low radiometric values of the Kanam mandible do not necessarily bespeak a recent age for the jaw, but only a different history and probably a different age as compared with the other Kanam fauna." In fact, if the Kanam jaw had been washed in from a Late Pliocene deposit with a low uranium content, it could be older than the Early Pleistocene animal fossils in the Kanam bed.

Tobias, however, chose a more comfortable alternative. "Nothing in these results," he said "would rule out the possibility that the Kanam mandible was derived from Middle Pleistocene beds in the vicinity, such as those of Rawe close to Kanam West" (Tobias 1968, p. 181). A late Middle Pleistocene date would be favorable for his view that the jaw is Neanderthaloid.

Significantly, the uranium content values that Oakley reported in 1974 were apparently not the first he had obtained. In a paper published in 1958, Oakley said, Immediately after discussing the uranium content testing of the Kanam jaw: "Applied to the Kanjera bones our tests did not show any discrepancy between the human skulls and the associated fauna" (1958, p. 53). It would appear that Oakley was not satisfied with these early tests and later performed additional tests on the Kanjera bones, obtaining results that were more to his liking.

Our review of the chemical testing of the Kanam and Kanjera fossils leads us to the following conclusions. The fluorine and nitrogen content tests gave results consistent with the human bones being as old as their accompanying faunas. This interpretation can nevertheless be challenged. The uranium content test gave results consistent with the human bones being younger than their accompanying faunas. But here again, if one chooses to challenge this interpretation, one will find ample grounds to do so.

All in all, the results of chemical and radiometric tests do not eliminate the possibility that the Kanam and Kanjera human fossils are contemporary with their accompanying faunas. The Kanjera skulls, said to be anatomically modern (Groves 1989, p. 291), would thus be equivalent in age to Olduvai Bed IV, which is 400,000 to 700,000 years old. The taxonomic status of the Kanam jaw is uncertain. Recent workers hesitate to call it anatomically modern, although this designation cannot be ruled out completely. If it is as old as the Kanam fauna, which is older than Olduvai Gorge Bed I, then the Kanam mandible would be over 1.9 million years old. Also, crude pebble tools were found at Kanam, and more advanced Chellean tools were found at Kanjera.

### 11.3 The Birth of Australopithecus

In 1924, Josephine Salmons noticed a fossil baboon skull sitting above the fireplace in a friend's home. Salmons, a student of anatomy at the University of the Witwatersrand in Johannesburg, South Africa, took the specimen to her professor, Dr. Raymond A. Dart. She thus set off a train of events that would win Dart worldwide fame.

The baboon skull given to Dart by Salmons was from a limestone quarry at Buxton, near a town called Taung, about 200 miles southwest of Johannesburg. Upon learning this, Dart asked his friend Dr. R. B. Young, a geologist, to visit the quarry and see what else might be found. At the Buxton quarry, Young found a limestone wall, the surface of which showed signs of old caves, filled in with a hard mixture of sand and travertine (a deposit of calcium carbonate). It was this old cave filling that contained the fossils, including many baboon bones. In fact, baboons still inhabited caves on nearby cliffs. When the sections of the wall containing the ancient cave deposits were blasted, Young collected some fossilbearing chunks and sent them to Dart (Keith 1931, pp. 39- 46).

#### 11.3.1 The Taung Child

Two crates of fossils arrived at Dart's home on the very day a friend's wedding was to be held there. Dart's wife pleaded with him to leave the fossils alone until after the wedding, but Dart opened the crates. In the second crate, Dart saw something that astonished him: "I found the

virtually complete cast of the interior of a skull among them. This brain cast was as big as that of a large gorilla" (Wendt 1972, p. 208). Dart then found another piece of rock that appeared to contain the facial bones.

After the wedding guests departed, Dart began the arduous task of detaching the bones from their stony matrix. Without proper instruments, he used his wife's knitting needles to carefully chip away the stone without damaging the fossil remains. Dart wrote: "No diamond cutter ever worked more lovingly or with such care on a priceless Jewel—nor, I am sure, with such inadequate tools. But on the seventy-third day, December 23, the rock parted. I could view the face from the front... . The creature which had contained this massive brain was no giant anthropoid such as a gorilla. What emerged was a baby's face, an infant with a full set of milk teeth and its permanent molars just in the process of erupting. I doubt if there was any parent prouder of his offspring than I was of my Taung baby on that Christmas" (Fisher 1988, p. 27).

After freeing the bones, Dart reconstructed the skull (Figure 11.5). He characterized the Taung baby's brain as unexpectedly large, about 500 cubic centimeters. The average brain capacity of a large male adult gorilla is only about 600 cubic centimeters. Dart noted the absence of a brow ridge and suggested that the teeth displayed some humanlike features (Boule and Vallois 1957, pp. 87-88). The front teeth were smaller in relation to the back teeth than in the apes, the canines were not as pointed, and there was no diastema. The diastema is a gap between the teeth of the lower jaw in apes. The gap accommodates the tips of the large canines protruding downward from the upper jaw. The teeth of apes tend to be arranged in a U-shaped fashion, with the rows of back teeth on either side of the jaw running straight and parallel to each other. The teeth of the Taung specimen, like those of human beings, were arranged in a curved, parabolic dental arcade. The youthful age of the creature could be determined from the fact that among the 24 teeth, 20 were milk teeth and 4 were permanent molars (Keith 1931, p. 52).

Figure 11.5. Left: The infant Australopithecus skull from a quarry near Taung, South Africa, after a photograph by A. R. Hughes (Day 1989, p. 14). Right: The skull of an immature gorilla, after Eckhardt (1972, p. 95).

Dart also noted that the foramen magnum, the opening for the spinal cord, was set toward the center of the base of the skull, as in human beings, rather than toward the rear, as in adult apes.

Dart took this to indicate the creature had walked upright, which meant the Taung specimen was, in his eyes, clearly a human ancestor.

Dart prepared a report for *Nature*, the prestigious British science journal, and sent it off to England. He also told B. G. Paver, news editor for the *Johannesburg Star*: "Perhaps I may shortly have news for you that will not be merely a good local lead. I may have something of worldwide significance connected with man's origin to announce shortly" (Dart 1959, p. 23). Dart gave Paver, who was interested in anthropology, sufficient information to put together an article but made Paver promise not to print it until after his scientific report was published. *Nature*, however, held Dart's article for review by other scientists. Paver became impatient and jumped the gun. He published his own story on February 3, 1925. The *Nature* article appeared four days later (Dart 1959, p. 34). Despite the plan going somewhat awry, Dart's intuition was correct. He became an overnight celebrity, and letters of praise (and blame) began to pour in.

In his *Nature* article, Dart reported: "The specimen is of importance because it exhibits an extinct race of apes intermediate between living anthropoids and man" (Wendt 1972, p. 209). From the accompanying fossils, he estimated his find's age at 1 million years, and called it *Australopithecus africanus*—the southern ape of Africa. *Australopithecus*, he believed, was ancestral to all other hominid forms.

Although public interest and adulation flared up quickly, reaction from the scientific community was substantially more reserved. In England, Sir Arthur Keith and Sir Arthur Smith Woodward received the report from Dart with utmost caution.

Sir Arthur Keith's initial reaction was to give Dart the benefit of the doubt. Keith said: "Professor Dart is not likely to be led astray. If he has thoroughly examined the skull we are prepared to accept his decision" (Oohanson and Edey 1981, p. 45). But Keith's later pronouncements were negative: "one is inclined to place *Australopithecus* in the same group or sub-family as the chimpanzee and gorilla. ... It seems to be akin to both" (*Nature*, Feb. 14, 1924). Some German scientists, such as Hans Weinert, also thought the Taung specimen was nothing more than an anthropoid ape.

The dating of the find also figured into Keith's disapproval. "The Taung ape is much too late in the scale of time to have any place in man's ancestry," he wrote (Oohanson and Edey 1981, p. 45). Dart had estimated that the Taung specimen was about 1 million years old. Keith had consistently held that human beings of modern type had existed for well over 100,000 years. But Keith's ideas about the pace of evolution would not allow a transformation from a creature as apelike as the Taung specimen to modern *Homo sapiens* in so short a period of time.

Grafton Elliot Smith was even more critical. In his response to Dart's article in *Nature*, he noted: "Many of the features cited by Professor Dart as evidence of human affinity, especially the features of the jaw and teeth mentioned by him, are not unknown in the young of the giant anthropoids and even in the adult gibbon" (Dart 1959, p. 36).



As time went by, Smith became increasingly unfavorable. In May 1925, in a lecture delivered at University College, Smith stated, in remarks reported in the Times of London: 'It is unfortunate that Dart had no access to skulls of infant chimpanzees, gorillas, or orangs of an age corresponding to that of the Taung skull, for had such material been available he would have realized that the posture and poise of the head, the shape of the Jaws, and many details of the nose, face, and cranium upon which he relied for proof of his contention that Australopithecus was nearly akin to man, were essentially identical with the conditions met in the infant gorilla and chimpanzee' (Dart 1959, p. 38).

Grafton Elliot Smith's critique remains valid even today. As we shall see, despite the enshrinement of Australopithecus as an ancestor of human beings, several scientists remain doubtful. Anatomical features that to some scientists suggest incipient humanity fall for others within the ape family's range of variation.

The popular press, initially favorable, also began to adopt a different attitude toward Australopithecus, making Dart's baby, as it came to be called, a subject of jokes and ridicule. A popular journal, The Spectator, asked readers to submit epitaphs for Australopithecus. One entry selected for publication read (Dart 1959, p. 38):

Here lies a man, who was an ape.

Nature, grown weary of his shape,

Conceived and carried out the plan

By which the ape is now the man.

Dart also received trouble from another quarter—Biblical creationists angry with him for proposing the forbidden missing link between ape and man. Dart (1959, p. 40) wrote: "Letters from religious people all over the world poured into my office, warning me that I was 'sitting on the brink of the eternal abyss of flame' and would later 'roast in the general fires of Hell.'"

In 1931, Dart was invited to London to give a report about his Australopithecus find before the Zoological Society of London. At the same meeting, Davidson Black gave his report introducing Beijing man. Black's presentation was consummately professional, delivered confidently with well-prepared visual aids. Dart, Taung fossil in hand, apparently stumbled through a weak presentation, simply restating his old case, first made in 1924. He failed to change any minds (Dart 1959, pp. 57-58). Dart later submitted a lengthy monograph on Australopithecus to the Royal Society, which refused to publish the work in full. Dart therefore withdrew it.

### 11.3.2 Dart Retreats

Dart was dismayed by the cool reception he received from the British scientific establishment. "Perhaps like Davidson Black," he said, "I should have traveled overseas with my specimens to evoke support for my beliefs" (Dart 1959, p. 51). Instead, Dart remained quietly in South Africa,

teaching comparative anatomy at the University of the Witwatersrand in Johannesburg. For many years, he stopped hunting for fossils.

British scientists, led by Sir Arthur Keith, maintained their opposition to Dart's *Australopithecus* throughout the 1930s. Keith (1931, p. 82) said that he found the brain markings on the endocranial cast of the Taung specimen to be like those of the gorilla or chimpanzee, and not at all human. He recognized some differences between the brain of the Taung specimen and apes but concluded that "the difference is not such as to lead us to separate *Australopithecus* from the category of anthropoid apes and place it in a separate group—one intermediate to the highest ape and lowest form of humanity" (Keith 1931, p. 86).

The facial skeleton also appeared quite ape-like to Keith. He wrote: "Our comparison of the profile and full-face of the Taung specimen with corresponding views of human and anthropoid skulls leaves no doubt as to the true status of *Australopithecus*. viz. that in all its essential characters it is a true anthropoid ape" (Keith 1931, p. 103).

Here Piltdown man, believed to be similar in geological age to the Taung specimen, entered Keith's calculations. The skull of Piltdown man, as we saw in Chapter 10, was like that of *Homo sapiens*. This fact argued against *Australopithecus*, with its ape-like skull, being in the line of human ancestry.

Keith (1931, p. 109) also held that the Taung specimen's teeth, relatively bigger than those of a human child, were ape-like.

What about the position of the foramen magnum, the opening through which the spinal cord enters the bottom of the skull? Keith pointed out that Dart had wrongly compared the position of the foramen magnum in the juvenile Taung skull with that of adult human beings and adult chimpanzees.

In adult humans, the foramen magnum is located toward the center of the bottom of the skull. This indicates erect posture. In adult chimpanzees, the foramen magnum is located toward the back of the skull, indicating a quadrupedal posture. The Taung skull's foramen magnum was located in the adult human position, so Dart thought this was good evidence for erect posture in *Australopithecus*.

Keith, however, pointed out that Dart should have compared the position of the foramen magnum in the infant Taung specimen with that of an infant chimpanzee instead of that of an adult. The foramen magnum of a baby chimp lies in around the same position as that of either the Taung specimen (Keith 1931, p. 110).

Also, the foramen magnum of a human child is situated more forward than the foramen magnum of the Taung baby or a baby chimp.

That the foramen magnum in the Taung specimen was located near the base of the skull, rather than the rear, did not, therefore, allow scientists to draw any conclusions about the posture of an

adult *Australopithecus*. For that, they required an adult specimen of *Australopithecus*, complete with lower limbs, and no such specimen had yet been found.

Keith (1931, p. 115) concluded: "A close examination of all the features of the Taung skull—the size and configuration of the brain, the composition of the cranial walls, the features of face, the characters of jaws and teeth and the manner in which the head was hafted to the neck—leave me in no doubt as to the nature of the animal to which the skull formed part; *Australopithecus* was an anthropoid ape."

As for the few humanlike characteristics of the specimen, Keith (1931, p. 53) said: "The features wherein *Australopithecus* departs from living African anthropoids and makes an approach towards man cannot be permitted to outweigh the predominance of its anthropoid affinities." For Keith, the total evidence ruled out the possibility that *Australopithecus* was, as most modern paleoanthropologists finally believe, a human ancestor.

### 11.3.3 Broom and *Australopithecus*

When Dart retired from the world stage, his friend Dr. Robert Broom took up the battle to establish *Australopithecus* as a human ancestor. From the beginning, Broom displayed keen interest in Dart's discovery. Soon after the Taung baby made his appearance, Broom rushed to Dart's laboratory. According to Dart (1959, p. 35): "he strode over to the bench on which the skull reposed and dropped on his knees in adoration of our ancestor," as he put it." British science, however, demanded an adult specimen of *Australopithecus* before it would kneel in adoration. Early in 1936, Broom vowed to find one.

On August 17, 1936, G. W. Barlow, the supervisor of the Sterkfontein limestone quarry, gave Broom a brain cast of an adult australopithecine. Broom (1951, p. 44) later went to the spot where the brain cast had turned up and recovered several skull fragments. From these he reconstructed the skull of *Plesianthropus transvaalensis*. The deposits in which the fossil was discovered are thought to be between 2.2 and 30 million years old (Groves 1989, p. 198).

More discoveries followed, including the lower part of a femur (TM 1513). Broom and G. W. H. Schepers (1946) described this femur as essentially human (Zuckerman 1954, p. 310). W. E. Le Gros Clark, initially skeptical of this description, later admitted that the femur "shows a resemblance to the femur of *Homo* which is so close as to amount to practical identity." In 1949, W. L. Straus, Jr. (1949) said that the femur "resembles man and cercopithecoid monkey in about equal degree" (Zuckerman 1954, p. 311). But according to a modern worker, the key diagnostic features of the Sterkfontein femur (TM 1513) are distinct from those of cercopithecoid monkeys and African apes and are "characteristic of modern Man" (Tardieu 1981, pp. 77-79). Since the TM 1513 femur was found by itself, it is not clear that it belongs to a *Plesianthropus* individual. It is possible, therefore, that it could belong to a more advanced hominid, perhaps one resembling anatomically modern humans.

On June 8, 1938, Barlow gave Broom a fragment of a palate with a single molar attached. Broom, as usual, paid Barlow for the fossil, but when Broom asked from where it had come, Barlow was evasive. Broom noticed that the matrix was different from that in which the fossils from Sterkfontein were usually embedded. Some days later, he again visited Barlow and this time insisted that he reveal the source of the fossil.

Barlow told Broom that Gert Terblanche, a local schoolboy, had given him the fossil palate. Broom obtained some teeth from Gert, and together they went to the nearby Kromdraai farm, where the boy had gotten the teeth by pounding them from a fossil skull. Broom collected the skull fragments, and Gert also gave Broom a piece of lower Jaw and more teeth. After reconstructing the partial skull, Broom saw it was different from the Sterkfontein type. He called the new creature *Paranthropus robustus*. As the name *robustus* indicates, this australopithecine hominid had a larger Jaw and bigger teeth than *Australopithecus africanus*, represented by the Taung baby, and the gracile *Plesianthropus* specimens from Sterkfontein. The Kromdraal site is now considered to be approximately 1.0 to

1. 2 million years old (Groves 1989, p. 198), although some have suggested an age of up to 1.8 million years (Tobias 1978, p. 67).

Broom also found at Kromdraal a fragment of humerus (the bone of the upper arm) and a fragment of ulna (one of the bones of the lower arm). He said: "had they been found isolated probably every anatomist in the world would say that they were undoubtedly human" (Broom 1950, p. 57).

In 1947, Le Gros Clark wrote that the humerus fragment from *Paranthropus* (TM 1517) displayed "a very close resemblance to the humerus of *Homo sapiens* and none of the distinctive features found in the recent anthropoid apes" (Zuckerman 1954, p. 310).

As might be expected, not everyone accepted this assessment of the TM 1517 humerus. In 1949, Straus said that "it is in general more like the average chimpanzee than like the average man" But he added that "this probably should not be stressed since it consistently falls within the ranges of variation of both species" (Zuckerman 1954, p. 311). A subsequent morphometric analysis done by H. M. McHenry (1972, p. 95) puts the TM 1517 humerus from Kromdraal "within the human range." As we have seen, scientists attribute the TM 1517 humerus to *Paranthropus robustus*, a robust australopithecine. Significantly, a robust australopithecine humerus from Koobi Fora, Kenya (ER 739), fell outside the human range in McHenry's study (1972, p. 95). So perhaps the TM 1517 humerus belonged to something other than a robust australopithecine. It is not impossible that the Kromdraal humerus and ulna, like the Sterkfontein femur, belonged to more advanced hominids, perhaps resembling anatomically modern humans.

World War II interrupted Broom's excavation work in South Africa. During this interval, he began the task of fully describing his *Australopithecus* discoveries, including Dart's Taung specimen.

After the war, Broom found another australopithecine skull (St 5) at Sterkfontein (Figure 11.6). Later he discovered further

Figure 11.6. Left: The skull of a female chimpanzee (after Zuckerman 1954, p. 308). Right: The St 5 Plesianthropus (*Australopithecus*) *fransvadensis* skull discovered by Robert Broom at Sterkfontein, South Africa (Broom et al. 1950, plate 1).

#### 11.3.4

#### Paranthropus and Telanthropus

At Swartkrans, near Sterkfontein, Robert Broom and J. T. Robinson found, beginning in 1947, fossils of a robust australopithecine called *Paranthropus crassidens* (large-toothed near-man). This creature had large strong teeth and a bony crest on top of the skull. The crest served as the point of attachment for big jaw muscles.

In addition to the fossils of *Paranthropus crassidens*, Broom and Robinson found the jaw of another kind of hominid in the Swartkrans cave. They attributed the jaw (SK 15), smaller and more humanlike than that of *Paranthropus crassidens*, to a new hominid called *Telanthropus capensis*.

Member 1 at Swartkrans, where all of the *Paranthropus* bones were found, is now said to be 1.2 to 1.4 million years old (Groves 1989, p. 198) or 1.8 million years old (Susman 1988, p. 782). But ages of 2.0 million and 2.6 million years have also been proposed (Tobias 1978, p. 65). Member 2, where the SK 15 *Telanthropus* mandible was found, is said to be

300,000 to 500,000 years old. Member 2 is said to represent an erosion channel. This makes it hard to tell how old the SK 15 jaw really is. It could have been washed in with other bones in the Middle Pleistocene. Or perhaps it could have been eroded from Early Pleistocene Member 1. In general, dating fossils found in the South African caves is quite difficult. The caves have been periodically filled and refilled over the course of 1 to 2 million years, resulting in an exceedingly confused stratigraphy. Those who have a degree of faith in chemical dating methods may take note that K.

P. Oakley tested the fluorine content of the Telanthropus Jaw and found it to be the same as the Paranthropus fossils (Broom and Robinson 1952, p. 113).

In 1961, Robinson "sank" the genus Telanthropus and reclassified the Swartkrans Jaw as Homo erectus (Brain 1978, p. 140). Broom and Robinson (1952), however, had previously noted several differences between the SK 15 teeth and those of Beijing man and Java man, both of which are now classified as Homo erectus. In terms of these differences, the SK 15 teeth were more like those of modern humans. Broom and Robinson also described other ways in which the SK 15 teeth were similar to those of modern humans. But the lower front part of the Jaw was damaged, making it "impossible to be sure whether there was a trace of a chin or not" (Broom and Robinson 1952, p. 110). The affinities of this apparently somewhat humanlike Jaw remain a mystery.

Broom and Robinson found another humanlike lower Jaw at Swartkrans. This fragmentary mandible (SK 45) came not from

an erosion channel but from the main deposit containing the Paranthropus fossils. Broom and Robinson (1952, p. 112) said: "In shape it is more easily matched or approached by many modern Homo Jaws than by that of Telanthropus." Robinson later referred the SK 45 Jaw to Telanthropus and then to Homo erectus (Brain 1978, p. 140). But there are reasons, admittedly not unclouded, to consider other possibilities. Emphasizing the ambiguous nature of the Telanthropus fossils, a recent worker (Groves 1989, p. 275) assigned them to an unnamed species of Homo.

#### 11.3.5 Paranthropus a Toolmaker?

In the years 1979-1983, C. K. Brain of the Transvaal Museum recovered fossil bones of 130 hominid individuals, 30 crude bone tools, and some crude stone tools. The newly discovered Swartkrans fossils included a relatively small number of well-preserved hand and foot bones.

Speaking of the 8 hand bones from Member I at Swartkrans, Randall L. Susman (1988, p. 783) said they indicated "that the robust australopithecines had much the same morphological potential for refined precision grasping and for tool-behavior as do modern humans." Susman (1988, pp. 782-783) noted, however, that the hand bones retained an apelike overall morphology.

The bone tools found at Swartkrans, according to Susman (1988, p. 783), have wear patterns indicating they were used for digging. Susman (1988, p. 783) therefore proposed that Australopithecus (Paranthropus) robustus had used stone and bone implements "for vegetable procurement and processing."

Most workers believe the making of tools is an exclusive trait of the genus Homo, starting with Homo habilis. According to this view, big-Jawed Paranthropus, a robust australopithecine

unconnected to the Homo line, munched vegetable matter like the modern gorilla, without the aid of tools.

In a New York Times report (1988), Donald C. Johanson, discoverer of Lucy, the most famous representative of *Australopithecus afarensis*, said about the Swartkrans hand bones: 'The big question is. how can we be 100 percent sure these hands are not from a Homo individual.'

Susman admitted that "the attribution of individual fossils to *Paranthropus* is complicated by the presence of a second hominid taxon (*Homo c.f. erectus*) at Swartkrans." But he pointed out: "In Member 1, however, more than 95% of the cranio-dental remains are attributed to *Paranthropus*. This fact suggests that there is an overwhelming probability that any one specimen recovered from Member 1 samples [represents] *Paranthropus*" (Susman 1988 p. 782). But even Susman (1988, p. 782) admitted that a thumb metacarpal (SK 84) found in Member I at Swartkrans in 1949 probably belonged to a Homo individual rather than *Paranthropus*.

So any matching of hand bones with hominid species at Swartkrans is still uncertain. The only thing that could end this uncertainty would be the discovery of hand bones in undisputed connection with other *Paranthropus* fossils.

But even if the new hand bones do belong to *Paranthropus*. there is no guarantee that *Paranthropus*, rather than Homo, made any of the stone and bone tools found at Swartkrans. "Did the two species live side by side?" asks anthropologist Eric Delson of the City University of New York. "Did [*P. robustus*] use leftovers of *Homo erectus* tool kits? There is no way to test these questions adequately" (Bower 1988, p. 345).

#### 11.3.6 Makapansgat and Final Victory

In 1925. Raymond A. Dart investigated a tunnel at Makapansgat, South Africa. Noting the presence of blackened bones. Dart (1925) concluded hominids had used fire there. In 1945. Philip V. Tobias, then Dart's graduate student at the University of the Witwatersrand, found the skull of an extinct baboon in the cave deposits of Makapansgat and called it to Dart's attention. In 1947. Dart himself went back out into the field, after a lapse of two decades, to hunt for *Australopithecus* bones at Makapansgat.

At Makapansgat, Dart (1948) found australopithecine skull fragments (including an occipital) and other bones, along with more signs of fire. Dart therefore called the creature who lived there *Australopithecus prometheus*, after the Titan who stole fire from the gods. Today, *Australopithecus prometheus* is classified, along with the Taung and Sterkfontein specimens. as *Australopithecus africanus*. distinct from the robust australopithecines of Kromdraai and Swartkrans.

Most of the Makapansgat fossils came from dumps of broken rock in front of the quarry there. From the matrix surrounding the fossils, Dart said he was able to correlate them with identifiable fossil-bearing strata nearby. Had anatomically modern human fossils been recovered in such fashion, any claims for their great age would have been subjected to merciless criticism. This is

because the main hominid layers at Makapansgat have been dated at about 3 million years by paleomagnetic methods (K. Weaver 1985, p. 596).

Dart discovered 42 baboon skulls at Makapansgat, 27 of which had smashed fronts. Seven more showed blows on the left front side (Dart 1959, p. 106). Dart, suspecting that australopithecines had been the cause of the damage, requested that R.

H. Mackintosh, a specialist in forensic medicine at the University of the Witwatersrand, examine the skulls. Dart and Mackintosh concluded that the skulls showed signs of having been struck by a "powerful downward, forward, and inward blow, delivered from the rear upon the right parietal bone by a double-headed object" (Wendt 1972, p. 224). They believed the weapon was an antelope's humerus (the bone of the upper forelimb). The joint of an antelope humerus, noted Dart, exactly fit the double impressions on several broken baboon skulls.

From the evidence he gathered at Makapansgat, Dart created a lurid portrait of *Australopithecus prometheus* as a killer ape-man, bashing in the heads of baboons with primitive bone tools and cooking their flesh over fires in the Makapansgat cave. While his robust cousins had remained in the forest, peacefully munching vegetables, and becoming extinct, this more advanced hominid had, according to Dart, ventured into the dry savannahs, to survive by his ruthless wits, and begin the long Journey to humanity.

Dart said: "Man's predecessors differed from living apes in being confined killers; carnivorous creatures, that seized living quarries by violence, battered them to death, tore apart their broken bodies, dismembered them limb from limb, slaking their ravenous thirst with the hot blood of victims and greedily devouring their writhing flesh" (Johanson and Edey 1981, p. 40).

Dart and Mackintosh also ascertained that australopithecines were killed in the same way as the baboons (Wendt 1972, pp. 226-227). "*Australopithecus* lived a grim life," wrote Dart (1959, p. 191). "He ruthlessly killed fellow australopithecines and fed upon them as he would upon any other beast, young or old."

Today, however, paleoanthropologists characterize Dart's portrait of *Australopithecus* as somewhat exaggerated. Johanson and Edey (1981, p. 65) call the vision of the killer ape-man "something of an embarrassment to anthropologists, who honor Dart for his dazzling recognition of the first australopithecine, but shake their heads over this later aberration."

In addition to antelope bones, Dart collected at Makapansgat many other animal remains that he believed had been used as daggers, choppers, saws, clubs, and so forth. He grouped these into what he called an "osteodontokeratic" industry, comprising tools made from bones, teeth, and horns (Dart 1957). In 1954, C. K. Brain found pebble tools at Makapansgat, 25 feet above the main layers in which the australopithecine fossils were found. One possible conclusion: a hominid more advanced than *Australopithecus* was the maker of the tools. But Dart (1959, pp. 159-160) pointed out that *Australopithecus* skeletal fragments were also to be found in the same layer as the pebble tools.



Dart's views about *Australopithecus* hunting activity at Makapansgat aroused heavy opposition. Some scientists said that the combination of *Australopithecus* fossils, mammalian bones, and broken baboon skulls represented not hominid occupation sites but the lairs of hyenas or leopards.

To this Dart (1959, pp. 120-131) replied that hyenas, in particular, do not tend to leave such accumulations of bones in their lairs. However, C. K. Brain replied with a more sophisticated version of the carnivore hypothesis that eventually won the day. "Over a period of years," wrote Richard Leakey and Roger Lewin (1977, p. 96), "Brain observed that a combination of scavenging habits of local carnivores, and the differential resistance to weathering of various types of bone, produces a bone collection virtually identical to the one Dart found in the cave: the osteodontokeratic culture is apparently no more than the left overs from many leopard and hyena meals!"

Nevertheless, this version does not seem to account for some of the evidence reported by Dart. For example, Dart (1959, p. 166) told of finding a gazelle horn wedged solidly into the core of an antelope femur, clear evidence of an intentional act. Dart also noted that the bones of birds, turtles, and porcupines, not the normal prey of hyenas and leopards, were among those found in the cave.

Concerning the evidence for fire at Makapansgat, some researchers said the black deposits were not ash (Oakley 1954, 1956). Others claimed that although there might be signs of fire, the australopithecines were not the cause of them (Broom 1950, p. 74; Johanson and Edey 1981, p. 69).

But even though Dart's views were discredited, there was a positive result. According to Herbert Wendt (1972, p. 222), the controversy over the Makapansgat discoveries "brought the australopithecines into the news, and enhanced their status even in the eyes of their original critics."

Another key event was the publication, in 1946, of a monograph on the australopithecines by Broom and Schepers. The National Academy of Sciences of the United States gave Broom and his coauthor the Daniel Giraud Medal for the most

Important biological work published in that year.

Sir Arthur Keith wrote in 1947: "When Professor Dart of the University of the Witwatersrand, Johannesburg, announced in *Nature* the discovery of a juvenile *Australopithecus* and claimed for it a human kinship, I was one of those who took the point of view that when the adult form was discovered it would prove to be nearer akin to the living African anthropoids—the gorilla and the chimpanzee. Like Professor Le Gros Clark I am now convinced on the evidence submitted by Dr. Robert Broom that Professor Dart was right and I was wrong. The *Australopithecinae* are in or near the line which culminated in the human form" (Dart 1959, pp. 80-81). At long last, *Australopithecus* had won recognition in the power centers of British paleoanthropology.

### 11.3.7 Controversy Continues

With the new status of *Australopithecus* came a change in perception. Increasingly, the vast majority of scientists began to see *Australopithecus* as less and less apelike and more and more humanlike. Right up to the present, the place of *Australopithecus* in the direct line of human descent is taken as an indisputable fact by most paleoanthropologists. Pictures of australopithecines generally show them as essentially human from the neck down. Furthermore, the types of behavior displayed by the australopithecines in these pictures are such that figures of humans could be easily substituted. But even after mainstream English science changed its mind about *Australopithecus*, some scientists resisted. To these recalcitrant renegades, the undistorted facts continued to reveal a starkly apelike portrait of *Australopithecus*. According to their view, a picture of an *Australopithecus* individual should show it hanging by its arms from the branch of a tree rather than walking erect and humanlike on the ground.

The primary dissenter, in the early aftermath of English acceptance of *Australopithecus*, was Sir Solly Zuckerman, secretary of the Zoological Society of London and later a science adviser to the British government. In a comprehensive study, Zuckerman (1954) found that the teeth, skull, jaws, brain, and limbs of *Australopithecus* were essentially apelike. He therefore believed that attempts to identify australopithecines as human ancestors were misguided. Today, a new generation of dissident researchers is raising and sustaining the same objections to overly humanlike characterizations of *Australopithecus*. We shall give detailed attention to their views, and those of Zuckerman, in Section 11.8.

### 11.4 Leakey and His Luck

After the professional and personal disappointments he encountered in the late 1930s, Louis Leakey continued his work in East Africa, assisted by his second wife, Mary. They searched for fossils of Early Pleistocene human ancestors, which Leakey believed would be quite different from *Australopithecus* and *Homo erectus*. Eventually, the Leakeys would get lucky and make a series of important finds. But for decades they had to be content with stone tools.

#### 11.4.1 Zinjanthropus

A site of particular interest was Olduvai Gorge in Tanzania. There the Leakeys found crude pebble choppers in Bed I, said to be 1.7 to 2.0 million years old (Oakley et al. 1977, p. 169). They also found round stones that appeared to have been used as bolas (Section 5.3.2). Leakey even found a bone implement he believed had been used for working leather. The standard image of Early Pleistocene hominids is one of ape-men scavenging carcasses of lion kills, not of protohumans working leather and hunting with bolas.

The stone tools Leakey found at Olduvai were not enough to satisfy him. "The remains of the men themselves still elude us," he said (Goodman 1983, p. 111). Finally, on July 17, 1959, Mary Leakey came across the shattered skull of a young male hominid in Bed I at site FLK. The skull was designated OH 5.

By one account, Leakey came out, looked at the OH 5 skull, and instead of rejoicing said: "Why, it's nothing but a goddamned robust australopithecine" (Oohanson and Edey 1981, pp. 91-92). "When he saw the teeth he was disappointed since he had hoped we would find a Homo and not an Australopithecus," said Mary Leakey (Oohanson and Edey 1981, p. 92).

Mary Leakey eventually pieced together hundreds of fragments, comprising the facial region and the rear part of the hominid's braincase. The creature had a sagittal crest, a bony ridge running lengthwise along the top of the skull. In this respect, it was very much like *Australopithecus robustus*. Leakey nevertheless created a new species for OH 5, partly because its teeth were bigger than the South African robustus specimens. Leakey called the new find *Zinjanthropus boisei*. Zinj is a name for East Africa and boisei refers to Mr. Charles Boise, one of the Leakeys' early financial backers (Wendt

1972, p. 232).

Along with the skull, the cranial capacity of which was about 530 cc, Leakey (1960a, pp. 1050-1051) found bones of mammals, including antelope and pig: "An extensive and rich living floor . . . has been uncovered. . . . All the larger animal bones have been broken open to obtain the marrow; all jaws and skulls of animals are smashed. A high proportion of the bones represent immature animals. Many more stone tools of the Oldowan culture have also been found." This assemblage apparently caused Leakey to give up his initial reserve and proudly declare to the world that he had found the remains of the first stone tool maker, and hence the first "true man."

Why Leakey decided to attribute the tools found at the FLK site to *Zinjanthropus* is somewhat puzzling. Similar tools had been found along with australopithecine remains at Sterkfontein. But Leakey had then said this proved only that the australopithecines of Sterkfontein "were contemporary with a type of early man who made these stone tools, and that the australopithecines were probably the victims which he killed and ate" (Goodman 1983, p. 113).

The FLK site presented a similar situation, calling for a similar explanation. But in a *Nature* article on *Zinjanthropus* Leakey said: "There is no reason whatsoever, in this case, to believe that the skull represents the victim of a cannibalistic feast by some hypothetical more advanced type of man" (Goodman 1983, p. 113).

Leakey became the first superstar that paleoanthropology had seen in a while. Along with *Zinjanthropus*, Leakey flew from Africa to the University of Chicago late in 1959 to participate in the Darwin Centennial, marking the one hundredth anniversary of the publication of *The Origin of Species* (Goodman 1983, p. 115).

The National Geographic Society honored Leakey with funds, publication of lavishly illustrated articles, television specials, and worldwide speaking tours. In 1962, the Society awarded him its highest award, the gold Hubbard Medal, for "revolutionizing knowledge of prehistory by unearthing fossils of earliest man . . . in East Africa" (Goodman 1983, p. 117).

The National Geographic Society is somewhat different from the other foundations active in paleoanthropological research, such as the Carnegie and Rockefeller foundations. Its funds did not represent the fortune of a single individual or family. The Society started out small and grew on the strength of individual membership contributions, in exchange for which donors received the Society's now famous Journal.

Alexander Graham Bell did, however, play an instrumental role in getting the National Geographic Society started. Although Bell did not give large sums of money, he hired Gilbert Grosvenor to supervise the publication of the Society's magazine, and paid his salary from his own pocket for many years. When Grosvenor took over editorial duties in 1899, the magazine of the National Geographic Society was a dry technical Journal, intended mainly for specialists in geography. He quickly transformed it into a pictorial magazine with vast popular appeal among the middle and upper classes.

Much of the considerable social influence enjoyed by the National Geographic Society has derived from its carefully cultivated relationships with America's social and political elites. Its board of trustees has consistently represented a cross section of the aristocracies of money and merit. Gilbert Grosvenor himself, from an old New England family, was a cousin of William Howard Taft, who served as President of the United States and Chief Justice of the Supreme Court. Grosvenor married Alexander Graham Bell's daughter, and his son Melville Bell Grosvenor followed him into leadership of the National Geographic Society.

Through its Committee for Research and Exploration, the National Geographic Society expends funds for scientific work in geography and related fields. Results are publicized not only through the magazine of the Society but through school bulletins, newsreleases, lecture series, films, and television specials.

Until the Society backed Louis Leakey, it had not, the record of its grants shows, supported any work directly related to evolution. Since then, however, the National Geographic Society has been one of the most influential forces in educating the general public, at least in the United States, about the story of human evolution. Exactly why the Society suddenly became so active in this field, starting in 1959, is not explained in any of the accounts of its history we have thus far seen. We would welcome information about this.

In the September 1960 issue of National Geographic magazine, Louis Leakey (1960b, p. 433) wrote, in a big photo article about Zinjanthropus: "In some respects this new Stone Age skull more closely resembles that of present day man than it does the skulls of the gorilla or of the South African near-men . . . Zinjanthropus represents a stage of evolution nearer to man as we know him today than to the near-men of South Africa." The article, provocatively titled "Finding the World's Earliest Man," featured an artist's representation of Zinjanthropus. Notwithstanding his huge Jowls and low forehead, Zinjanthropus was depicted as blatantly humanlike—a shameless propaganda move.

But despite an outpouring of publicity, the reign of Zinjanthropus was all too brief. F. Clark Howell said: "It obviously was not a man. It was even less manlike than the least manlike of those two South African types " Oohanson and Edey 1981, p.

92). The two South African types were Australopithecus africanus (from Taung, Sterkfontein, and Makapansgat) and Australopithecus robustus (from Kromdraai and Swartkrans). Robustus was considered the least manlike.

Leakey's biographer, Sonia Cole (1975, pp. 239-240), wrote: "He must have wished he could have eaten his words. . . . Granted that Louis had to persuade the National Geographic Society that in Zinj he had a likely candidate for 'the first man' in order to ensure their continued support—but need he have stuck out his neck quite so far? Even a layman looking at the skull could not be fooled: Zinj, with his gorilla-like crest on the top of the cranium and his low brow, was quite obviously far more like the robust australopithecines of South Africa than he was like modern man—to whom, quite frankly, he bears no resemblance at all."

#### 11.4.2 Homo Habilis

In 1960, about a year after the discovery of Zinjanthropus, Leakey's son Jonathan found the skull of another hominid (OH 7) nearby in a slightly lower level of Bed I, Judged to be about 2 million years old. In addition to the skull, the OH 7 individual included the bones of a hand. Also in 1960, the bones of a hominid foot (OH 8) were found. In succeeding years, more discoveries followed, mostly teeth and fragments of jaw and skull. The fossil individuals were given colorful nicknames: Johnny's Child, George, Cindy, and Twiggy. Some of the bones were found in the lower part of Bed II

Philip Tobias, the South African anatomist, gave the first newly found skull a capacity of 680 cc, far larger than Zinjanthropus at 530 cc, and larger even than the biggest australopithecine skull, at roughly 600 cc. It was, however, around

100 cc less than the smallest Homo erectus skulls (Wendt 1972, pp. 245-246).

Leakey sent the OH 7 hand bones to Dr. John Napier of the Royal Free Hospital in England. The results of Napier's study were pleasing to Leakey. The bones, said Napier (1962, p. 409), were "strikingly human in one revealing and . . . critical character. The tips of the fingers and thumb were surmounted by broad, stout, flat, nail-bearing terminal phalanges, a condition that, as far as we know, is found only in man.'

The Leakeys sent the OH 8 foot bones to Michael Day for reconstruction. Day, recalling his impressions on completing his work, later said: "My hair stood on end. The foot was completely human" (Cole 1975, p. 253).

Like Zinjanthropus, the fossils of the new creatures were found along with broken animal bones and stone tools, scattered across a so-called living floor.

Some distance away from one of the new sites, but at the same level, a circle of large stones was found. The Leakeys interpreted this as the foundation for a windbreak made of brush, giving rise to speculation that the large-brained Olduvai hominid had made use of base camps.

Louis Leakey decided he had now come upon the real toolmaker of the lower levels of Olduvai, the real first true human. His bigger brain confirmed his status, although it was Darwin himself who had said that "one cannot measure intelligence in cubic centimeters" (Wendt 1972, p. 246). A full report on the new Olduvai hominid was published in 1964 by Louis Leakey, John Napier, and Philip Tobias. In this paper (Leakey et al. 1964), they called the creature *Homo habilis*. The name, suggested by Raymond Dart, means, "handy man." The designation *Homo* signified a close family relation to modern humans. As we shall see, however, many scientists doubted whether the honor was merited.

After the discovery of *Homo habilis*, *Zinjanthropus*, no longer the first true human, was demoted to *Australopithecus boisei*, a somewhat more robust variety of *Australopithecus robustus*. Both of these robust australopithecines had sagittal crests, and are regarded not as human ancestors but as evolutionary offshoots that eventually became extinct.

The whole business of sagittal crests complicates matters somewhat. Male gorillas and some male chimpanzees also have sagittal crests, whereas the females of these species do not (Fix 1984, p. 32). This leads to the possibility that creatures assigned to different australopithecine species, on the grounds that some have sagittal crests and others do not, may simply represent sexual variants within a single species. For example, Mary Leakey (1971, p. 281) said: "The possibility that *A. robustus* and *A. africanus* represent the male and female of a single species deserves serious consideration." If the possibility raised by Mary Leakey were found to be correct, this would mean that generations of experts have been wildly mistaken about the australopithecines.

#### 11.4.3 Leakey's Views on human evolution

With the discovery at Olduvai Gorge of *Homo habilis*, a creature contemporary with the early australopithecines but with a bigger brain, Louis Leakey believed he had excellent evidence supporting his view that neither *Australopithecus* nor *Homo erectus* were in the direct line of human ancestry (Figure 11.7). He later wrote: "For too long scientists have been confuted by earlier theories and in particular by those which derived *Homo sapiens* from classical forms of Neanderthal man, which in turn was supposed to have been derived from *Homo erectus*, that in turn was said to have been originated in the Australopithecines. . . . Today the vast amount of evidence that has been accumulated shows us clearly that the stock which was leading to ourselves—as distinct from *Homo erectus*—was already present some 2 million years ago in East Africa and that, at that time, it was contemporary with *Australopithecus*. We should therefore

expect to find evidence that true Homo, as well as primitive Australopithecus, was already present during the late stages of the Pliocene, about 4 million years ago " (L. Leakey 1971, p. 25).

Figure 11.7. According to Louis Leakey (1960d, pp. 210 -211; 1971, p. 27), neither Australopithecus nor Homo erectus was ancestral to modern humans. The Neanderthals, said Leakey (1971, p. 27), were probably the result of crossbreeding between Homo erectus and Homo sapiens. Today, the details of human evolution remain a subject of active debate. But most paleoanthropologists favor a progression from one of the australopithecines to Homo habilis. Homo erectus, early Homo sapiens, and then the Neanderthals and modern humans.

Although Leakey was now willing to settle for somewhat primitive Homo habilis as the representative of true humanity in the Early Pleistocene, he had earlier believed that the fully modern human type extended that far back in geological time. As we have seen Leakey initially supported Reck's anatomically modern skeleton, found in Bed II of Olduvai Gorge (Section 11.1). He also campaigned on behalf of his own finds of sapiens-like human fossils at Kanam and Kanjera (Section 11.2). These finds, all of which Leakey originally thought to be from the Middle and Early Pleistocene, would have been the contemporaries of Australopithecus and Homo erectus. Later, Leakey withdrew his support of a Middle Pleistocene date for Reck's skeleton when challenged by Boswell, and soon thereafter saw his own finds at Kanam and Kanjera discredited in the eyes of most scientists by the same persistent critic. But in reviewing the controversies over these fossils, we have found, despite some ambiguity, sufficient reason to keep them as evidence for sapiens-like beings in Africa 1-2 million years ago.

In Leakey's opinion, the major problem with the standard view of human origins was that it resulted in a progression that appeared to violate evolutionary principles. "Australopithecinae or 'near-men' show a number of characters which very strongly suggest over-specialization in directions which did not lead towards man," said Leakey (1960d, p. 184). "The very peculiar flattening of the face, the raising of the eye sockets high above the level of the root of the nose, and the shape of the external orbital angles are among such specializations, as is also the forward position of the root of the cheek-bone process."

Leakey (1960c, p. 212) also stated: "there are those who still hold that Peking man and Java man should be listed as direct

ancestors of *Homo sapiens*, with Neanderthal and Solo types as intermediate forms, but I cannot support this interpretation, which implies too great a measure of reversal of specialization." Some of Leakey's contemporaries assumed the earliest hominids would have features reminiscent of modern apes. According to this view, the path of human evolution, proceeding through the australopithecines and *Homo erectus*, involves a progressive diminution of these primitive apelike features. According to Leakey (1960d), this idea is incorrect.

Certain features of modern apes, such as large brow ridges, are not primitive, said Leakey, but are instead fairly recent specializations. *Proconsul*, an Early Miocene African ape thought to be at the very root of the human line, did not have large brow ridges. "There is no trace whatsoever of a ridge of bone over the eyes, separating the brain-case from the face," wrote Leakey (1960d, p. 175).

Modern humans, with their small brow ridges, according to Leakey, preserve the primitive condition found in the Miocene apes. *Australopithecus*, *Homo erectus*, and the Neanderthals, with their large brow ridges, depart, like the modern apes, from this primitive condition. The now-dominant evolutionary progression thus involves an evolutionary reversal that Leakey thought unlikely. Miocene apes with no brow ridges give rise to early hominids with heavy brow ridges, and these hominids in turn give rise to modern humans, with small brow ridges. Furthermore, the Miocene apes like *Proconsul* have thin skulls, while the australopithecines, *Homo erectus*, and the Neanderthals have relatively thick skulls. Modern humans have thin skulls, implying another evolutionary reversal.

The advocates of punctuated equilibrium in evolution have a response to Leakey, namely that such reversals can be expected (Stanley 1981, p. 155). One of the great advantages of the punctuated equilibrium theory, which holds that speciation occurs not gradually over long periods of time but in rapid bursts, is that it allows advocates of evolution to easily explain away all kinds of contradictions found in the fossil record.

Apart from size, the physical structure of modern human brow ridges is different from that of other hominids. "The brow-ridge over each eye is made up of two component parts in *Homo sapiens*," wrote Leakey (1960d, p. 164). "One part in each case starts just above the nose and extends sideways and slightly upwards to overlap the second part, which, on either side, starts at the extreme edge to right and left of the eye-socket respectively, and extends inwards and slightly downwards. Thus, above the center of each eye-socket, there is an overlap of the two elements." In Neanderthal, *Homo erectus*, and *Australopithecus*, the large brow ridges are most often composed of a single barlike mass of bone running horizontally over the eye sockets. To Leakey (1960d, p. 165), the presence of such barlike brow ridges "suggested not an ancestral stage in human evolution but a side branch that has become more specialized, in this respect, than any *Homo sapiens* type."

In addition to features found in the earliest presumed human ancestors (the Miocene apes such as *Proconsul*), modern humans also have, said Leakey, other specializations that distinguish them from *Homo erectus* and *Australopithecus*.



For example, the Jaw of modern *Homo sapiens* has a chin eminence, which Leakey (1960d, p. 168) described as a 'bony buttress on the front of the middle line of the Jaw.' Living apes do not have a true chin eminence, and neither do *Homo erectus* and *Australopithecus*.

According to Leakey, the purpose of the chin eminence is to strengthen the front portion of the Jaw. In apes this is accomplished by the simian shelf, a ridge of bone running between the two sides of the forward part of the lower Jaw. In Neanderthals, *Homo erectus*, *Homo habilis*, and *Australopithecus*, none of which have a simian shelf, the strengthening is accomplished by thickening the entire front portion of the Jaw.

In making his case, Leakey also considered the presence of a feature of the facial skeleton called the canine fossa. Leakey (1960d, pp. 165-166) stated: "If we look at the facial region of different types of *Homo sapiens* we find that. . . there is always present a depression or hollow in the bone beneath each eye, which is called the 'canine fossa.' . . . In the great apes and in the skulls of human species other than *Homo sapiens* it is only very rarely seen and is more commonly replaced by a convexity or puffing out of the bone in that region."

Other anatomical differences between *Homo sapiens* and its presumed ancestors, as discussed by Leakey (1960d), involved the tympanic plate around the ear hole, the mastoid process, the articulation of the Jaw, and the position of the foramen magnum.

Time, said Leakey, was another problem. Not only was *Homo habilis* contemporary with *Australopithecus*, thus eliminating the latter, in Leakey's mind, as a human ancestor—there was also trouble with the supposed transition from *Homo erectus* to *Homo sapiens*. Leakey (1971, p. 27) wrote: "The textbooks, on the whole, still suggest that *Homo sapiens* stems from *Homo erectus*; this view can no longer be sustained. The time interval between Java and Peking man in Asia, or the Olduvai fossil of *Homo erectus* in Tanzania, and the appearance of *Homo sapiens* over a wide area from Europe and east Africa is far too short." The later specimens of *Homo erectus* in Java and China, and in the upper levels of Olduvai Gorge, existed from

200,000 to 500,000 years ago in the Middle Pleistocene. Early *Homo sapiens*, is said to have appeared 300,000 to 400,000 years ago. In other words, *Homo erectus* and *Homo sapiens* were roughly contemporary, and this, to Leakey, seemed to eliminate *Homo erectus* as a human ancestor, although others might suggest that humans branched from *Homo erectus* far earlier.

Here we are, of course, restricting ourselves to conventionally accepted fossil evidence. In previous chapters, we have argued that the totality of evidence—including the fully modern human skeleton found in a Pliocene formation at Castenedolo in Italy, the advanced stone artifacts and human skeletal remains found in Eocene formations in the California gold country, and much else—does not support an evolutionary origin of the modern human type. If this is correct, then we should not expect the various hominid finds in Africa and elsewhere to line up neatly in an evolutionary sequence. And they do not.

If *Australopithecus*, *Homo erectus*, and the Neanderthals were not human ancestors, then how were they to be explained in terms of evolution? As far as the australopithecines were concerned, Leakey (1960d, p. 180) said it was likely that "they represent a very aberrant and specialized offshoot from the stock which gave rise to man."

Louis Leakey also had some iconoclastic opinions about the relationships among the various australopithecines. "Textbook views that . . . the robust *Australopithecus* is . . . a late specialized variant of the so-called gracile one . . . cannot any longer be regarded as valid," wrote Leakey (1971, p. 27). "A number of examples of the robust Australopithecines have now been found in deposits much older than Olduvai, while side by side with them are specimens that apparently represent ancestors of *Homo habilis*." Leakey here seems to be referring to discoveries by his son Richard at sites near Lake Turkana, Kenya. As we shall see (Section 11.6), the dating of these Lake Turkana deposits was controversial. Richard Leakey originally favored an age of 2.9 million years, but he eventually agreed with critics that the deposits were about 2 million years old. Even so, the robust australopithecine fossils from Lake Turkana would be as old as the gracile australopithecine fossils found in South Africa. The elder Leakey therefore thought he had good reason to challenge the widely accepted belief that the robust australopithecines were derived from the gracile ones. Leakey's proposal was given additional support in 1986, with the discovery of the so-called Black Skull (Section 11.11), which pushed the robust australopithecines back to 2.5 million years ago. Mary Leakey, as we have seen, outdid her husband in boldness—she suggested the robust and gracile australopithecines might be the males and females of the same species.

As for Java man and Peking man, representing *Homo erectus*, Leakey (1960d, p. 186) also considered them "nothing but various aberrant and over-specialized branches that broke away at different times from the main stock leading to *Homo*." Leakey was not alone in his views about *Homo erectus*. In 1972, J. B. Birdsell, an anthropologist at the University of California at Los Angeles, wrote: "It is very difficult to visualize how any of the known forms of *Homo erectus* could have evolved into the grade of *Homo sapiens*. . . nowhere can it be demonstrated that men of the *Homo erectus* grade did evolve into modern populations" (Goodman 1983, p. 121). Of course, there are many who would disagree with Leakey and Birdsell.

Some authorities have placed much emphasis on fossils such as Rhodesia man in Africa, Solo man in Java, and the European Neanderthals. These, they say, show clearly an evolutionary transition between *Homo erectus* and *Homo sapiens*. But Leakey (1971, p. 27) had another explanation: "Is it not possible that they are all variants of the result of crossbreeding between *Homo sapiens* and *Homo erectus*?" One might object that such crossbreeding would have yielded hybrids that were unable to reproduce. But Leakey pointed out that American bison cross fertilely with ordinary cattle.

So whereas some scientists would have *Homo erectus* evolving into the Neanderthals, who then give rise to modern *Homo sapiens*, Leakey would have all three coexisting. And as we have seen (Section 9.2.9), there is substantial evidence from the Chinese Middle Pleistocene that *Homo erectus* coexisted with varieties of *Homo sapiens*, including Neanderthals. In fact, there is evidence

that erectus-like creatures may exist today in isolated wilderness regions, including China (Chapter 10). There are even reports that they have interbred with humans (Section 10.8). All of this agrees with our proposal that various humanlike and apelike creatures have coexisted in the distant past, just as at present.

#### 11.4.4 Evidence for Bone smashing in the Middle Miocene

During the late 1960s, Louis Leakey made some interesting discoveries at Fort Ternan, Kenya. The fossil-bearing formations at this site are said to be from 12.5 to 14.0 million years old (Butzer 1978, p. 198), which makes them Middle Miocene. After noting that hundreds of relatively undamaged fossil mammal bones had been found at Ft. Ternan, Leakey (1968, p. 528) said: "In striking contrast to this situation, there are in the same deposit, and at the same level, small areas of fossils where the bones have been broken up, and where the damage includes excellent examples of depressed fractures of the types usually associated with 'a blunt instrument.' . . . We also recovered a peculiar lump of lava exhibiting several battered edges, and with every appearance of having been used to smash bones." According to Glynn Isaac (1978, p. 229), Leakey believed that the lava was not of the kind found normally in the deposit: therefore it must have been transported to the site. Leakey concluded that an apelike Miocene hominid called *Kenyapithecus* had used the lava stone to crack bones for marrow. E. L. Simons (1978, pp. 548-549) and others considered *Kenyapithecus* to be an African variety of the Asian hominid *Ramapithecus*. Currently, however, scientists do not think the *ramapithecines* can be classified as hominids (Section 3.9). Because no one (as far as we know) now attributes tool behavior to Miocene apes, we are left wondering what hominid used stones to break bones for marrow at Ft. Ternan over 12.5 million years ago. We do not know, but, as we noted in Chapter 2, modern humans leave similar broken bone assemblages.

#### 11.5 A Tale of Two Humeri

In 1965, Bryan Patterson and W. W. Howells found a surprisingly modern-looking hominid humerus (upper arm bone) at Kanapoi, Kenya. In 1977, French workers found a similar humerus at Gombore, Ethiopia.

##### 11.5.1 The Kanapoi Humerus

The Kanapoi humerus fragment, consisting of the intact lower (or distal) part of the bone, was found on the surface. But B. Patterson and Howells (1967, p. 64) noted: "Color, hardness, and degree of mineralization agree with those of numerous specimens collected in situ in the sediments." Potassium-argon tests on volcanic materials above the bone-bearing sediments yielded dates of 2.9 and 2.5 million years. Paleomagnetic tests showed the lava displayed reverse polarity. The Matuyama Reverse Epoch began 2.5 million years ago, consistent with the potassium-argon results (B. Patterson and Howells 1967, p. 64).

The Pliocene lake sediments also yielded a fauna earlier than that found in Bed I of Olduvai Gorge. Patterson and Howells said it corresponded to the early Villafranchian of Europe. Another

researcher later commented on the faunal remains: "These are comparable to those found at the Mursi site in the Omo River valley, with an age of 4.0 to 4.5 million years" (Senut 1979, p. 113). Patterson accepted this as a reasonable date for the layer from which the humerus was thought to have eroded (Oakley et al. 1977, p. 59).

Could the bone have been intrusive in the deposit? B. Patterson and Howells (1967, p. 64) stated: "The excellent state of preservation—the fragment shows no significant postmortem damage other than the break that separated it from the remainder of the original bone—rules out the possibility of derivation from later deposits that may once have been present in the vicinity of the capping lava. "

B. Patterson and Howells (1967, p. 65) said the Kanapoi humerus was "readily distinguishable . . . from gorilla and orangutan." They then made detailed "morphological and metrical comparisons" with human beings, chimpanzees, and

*Australopithecus*.

Patterson and Howells measured 7 features on 40 human humeri, 40 chimpanzee humeri, and a cast of the distal humerus of *Paranthropus robustus* (Kromdraai TM 1517), the only australopithecine distal humerus then available. They concluded: "In these diagnostic measurements Kanapoi Hominoid 1 is strikingly close to the means of the human sample. It is larger than the individual of *Paranthropus robustus* represented by the corresponding humeral fragment from Kromdraai in each measurement" (B. Patterson and Howells 1967, p. 65). Patterson and Howells (1967, p. 65) added: "*Paranthropus* emerges from these morphological comparisons as rather less man-like than Kanapoi Hominoid 1." Further emphasizing the humanlike character of the Kanapoi humerus, they said: "there are individuals in our sample of man on whom measurements . . . of Kanapoi Hominoid 1 can be duplicated almost exactly" (B. Patterson and Howells 1967, p. 66).

Patterson and Howells would not have dreamed of suggesting that the Kanapoi humerus belonged to an anatomically modern human. Nevertheless, if an anatomically modern human had died at Kanapoi 4.0-4.5 million years ago, he or she might have left a humerus exactly like the one they found.

Further confirmation of the humanlike morphology of the Kanapoi humerus (KNM KP 271) came from anthropologists Henry M. McHenry and Robert S. Corruccini of the University of California. Using multivariate analysis techniques, they compared 16 different measurements of the Kanapoi humerus with those of the humeri of all species of anthropoid apes, three species of monkeys, and two fossil hominids—Kromdraai (TM 1517) and East Rudolf (ER 739). McHenry and Corruccini (1975, p. 227) concluded that "the hominid fossil from Kanapoi resembles *Homo sapiens* very closely." Elsewhere in the same study they noted: "The Kanapoi fossil is quite close to *Homo*, especially the Eskimo sample" (McHenry and Corruccini 1975, p. 235). Amplifying this, they stated that "the Kanapoi humerus is barely distinguishable from modern *Homo*" and "shows the early emergence of a *Homo*-like elbow in every subtle detail" (McHenry and Corruccini 1975, p. 240).

In an earlier study, McHenry (1973) wrote: "A humeral fragment has been found at Kanapoi that is almost five million years old yet almost indistinguishable in shape from many modern humeri. Geologically much younger australopithecine humeri at one or two million years are vastly different from those of modern man." In his Harvard doctoral thesis, McHenry (1972, p. 95) stated that the Kanapoi humerus fell "within the human range." We have employed a simple multivariate analysis technique to evaluate the raw data supplied by McHenry in his thesis. We calculated the 16-dimensional vectors represented by his 16 measurements for each humerus, and took the size of the angles between any two vectors as indicators of the degree of similarity between the two humeri. A smaller angle means a greater similarity. This method, it should be noted, is size-independent. In other words, bones of the same conformation, though being of different size, will show a difference of zero degrees. Confirming McHenry, we found that at 2.75 degrees the Kanapoi humerus vector was closest to *Homo sapiens*. For comparison, the angle of Kanapoi with chimpanzee was 4.40 degrees. With *Australopithecus robustus* (Kromdraai TM 1517) the angle was 4.51 degrees, and with *Australopithecus boisei* (East Rudolf) it was 4.83 degrees. In other words, the Kanapoi humerus differed from those of the australopithecines.

C. E. Oxnard (1975a, p. 97) agreed with McHenry's analysis. He stated: "we can confirm clearly that the fossil from Kanapoi is very humanlike." In his discussion, Oxnard pointed out that the Kanapoi humerus, although 4 million years old, was quite modern in form, while the australopithecine humeri from later periods were much less so. This led Oxnard (1975a, p. 121) to suggest, as did Louis Leakey, that the australopithecines were not in the main line of human evolution. Keeping *Australopithecus* as a human ancestor would result in a very unlikely progression from the humanlike Kanapoi humerus, to the markedly less humanlike humerus of *Australopithecus boisei*, and then to one more humanlike again.

Michael A. Day (1978, p. 315) said about the Kanapoi humerus: "It is hard to point to a single anatomical feature or group of features that is not well known in modern man. Functionally it must be nearly identical with the modern human condition."

A dissenting view may be found in a study by Marc R. Feldesman, of Portland State University in Oregon. From his own multivariate analysis of 15 fossil humeri and humeri of 22 species of monkey and apes, Feldesman (1982a, p. 73) concluded: "The Kanapoi distal humerus (KP 271), far from being more 'human-like' than *Australopithecus*, clearly associates with the hyperrobust *Australopithecines* from Lake Turkana." The Lake Turkana specimen closest to KP 271, according to Feldesman, was ER 739, now thought to represent *Australopithecus boisei*. This is exactly the reverse of McHenry's conclusion. McHenry found that KP 271 was close to *Homo sapiens* and distant from ER 739. Because Feldesman did not supply his raw data in his report, we could not evaluate his results.

In our discussion of fossil discoveries in China (Section 9.2.1), we made extensive use of the concept of possible date ranges. That is to say, when confronted with reports giving different ages for certain fossils, we established a range of possibilities that included all likely ages. Here we want to introduce a similar concept—that of possible morphology ranges. Concerning the Kanapoi

humerus, we can say, on the basis of the reports we have cited, that its morphology range extends to the modern human end of the spectrum.

#### 11.5.2 The Gombore Humerus

In 1977, French researchers (Chavaillon et al. 1977) reported finding a humanlike humerus at the Gombore site in Ethiopia, about 55 kilometers south of the capital, Addis Ababa.

The Gombore humerus was, however, more recent than the Kanapoi humerus. Noting that stone tools were found near the Gombore humerus, Brigitte Senut (1979, pp. 112 -113) stated: "The stone industry of Gombore IB is like that of the upper part of Bed I and the base of Bed II at Olduvai (Tanzania), which have been dated at 1.7 million years by the potassium- argon method. The same radiometric method applied to basalt at the Ethiopian site gives the layers in which the Oldowan tools were found a date older than 1.5 million years." The first excavators (Chavaillon et al. 1977, p. 961) also noted: "The site is an Oldowan encampment, with a shelter and organized zones containing different types of tools."

Senut (1979, p. III) said, in an English summary of one of her French papers, that the Gombore humerus could, along with the Kanapoi humerus, "be attributed to the genus *Homo*." Concerning the Kanapoi humerus, Senut was in agreement with

B. Patterson and Howells (1967), McHenry and Corruccini (1975), McHenry (1972, 1973), Oxnard (1975a), and Day (1978), who all thought the Kanapoi humerus to be unlike that of *Australopithecus*. Senut differed from Feldesman (1982a), who thought the Kanapoi humerus to be like that of *Australopithecus boisei* (ER 739).

Like Senut (1979), the original discoverers of the Gombore humerus hesitated to designate it as anything more than *Homo* (Chavaillon et al. 1977). Similarly, Feldesman (1982a, p. 92), who thought the Kanapoi humerus to be like those of australopithecines, said: "The Gombore specimen appears to be closer to *Homo* than to anything else." But Chavaillon and his coworkers (1977, p. 962) noted: "in the lateral view, the bone very much resembles *Homo sapiens sapiens*." Senut later found other features that were humanlike. "Gombore IB 7594,

which was primitively [first] attributed to the genus *Homo* (Chavaillon et al. 1977, Senut 1979), cannot be differentiated from a typical modern human," she wrote (Senut 1981b, p. 91).

So now we seem to have two very ancient and humanlike humeri to add to our list of evidence challenging the currently accepted scenario of human evolution. These are the Kanapoi humerus at 4.0-4.5 million years in Kenya and the Gombore humerus at more than 1.5 million years in Ethiopia. At the very least, the Early Pliocene Kanapoi humerus "could challenge the new phylogenies tending to show that only one genus and one species (*Australopithecus afarensis*) was living at this date" (Senut 1979, p. 111). The Kanapoi and Gombore humeri also support the nonevolutionary view that human beings of modern type have coexisted with other humanlike and apelike creatures for a very long time.

## 11.6 Richard, Son of Leakey

Louis Leakey's son Richard at first avoided fossil hunting, working instead as a safari organizer for clients including the National Geographic Society. Eventually, however, Richard took up the family profession. Although he had no university training, he began to develop his own reputation as a competent paleoanthropologist.

In 1967, Richard Leakey, then just 23 years old, led the Kenya section of an international paleoanthropological expedition to the Omo region of southern Ethiopia. Unhappy at having to turn over fossils he discovered to professional scientists, Leakey suddenly left the Omo site. He flew by helicopter to Koobi Fora, on the crocodile-infested eastern shores of Kenya's Lake Rudolf, now called Lake Turkana. On his very first walk around Koobi Fora, Leakey found a stone tool and a fossil pig jaw. The site was promising, but he needed funding in order to systematically develop it.

In January of 1968, Richard Leakey journeyed to Washington, D.C., where he got a grant of 25,000 dollars from the National Geographic Society's Committee for Research and Exploration. Returning to Kenya, Leakey set up a permanent camp at Koobi Fora.

That first year saw no major discoveries, but in 1969 Richard and his wife Meave found an australopithecine skull. Over the next few years, fossils of three more *Australopithecus* individuals turned up (R. Leakey 1973b, p. 820). Also, Glynn Isaac found hundreds of crude stone tools at several Early Pleistocene sites near Koobi Fora (R. Leakey 1973b, p. 820). *Australopithecus* was not known to have been a toolmaker. So who had made the tools?

### 11.6.1 Skull ER 1470

In August of 1972, Bernard Ngeneo, a member of Leakey's team, found at Lake Turkana a shattered skull that appeared to give an answer. Richard's wife Meave, a zoologist, reconstructed the skull, designated ER 1470. Alan Walker of the University of Nairobi estimated that its cranial capacity was over 810 cc (R. Leakey 1973a, p. 449), bigger than the robust australopithecines. For example, the robust OH 5 *Australopithecus boisei* specimen from Olduvai, formerly called *Zinjanthropus*, had a cranial capacity of just 530 cc (R. Leakey 1973a, p. 450). The ER 1470 skull was in fact as large as some smaller *Homo erectus* skulls, which range between 750 and 1100 cc. The average human skull is about 1400 cc. Among adult humans, the very lowest cranial capacities are in the low 800s (Brodrick 1971, p. 84).

Viewed from the rear, the sides of the reconstructed ER 1470 skull were nearly vertical, as in *Homo sapiens*. In *Australopithecus* and *Homo erectus*, the sides of the skull, seen from the rear, slope noticeably towards each other at the top (Figure 9.1, p. 556). Furthermore, the domed forehead of ER 1470 was not as receding as that of *Australopithecus* or *Homo erectus*, and the brow ridges were smaller. The skull walls of ER 1470 were thinner than those of *Australopithecus* or *Homo erectus*. Also, the foramen magnum, the opening in the base of the skull for the spinal cord, was located farther forward than in *Australopithecus*. In other words, several features of the

somewhat primitive ER 1470 skull were characteristic of advanced species of the genus *Homo* (Fix 1984, pp. 50-51; R. Leakey 1973a, p. 448).

Richard Leakey initially hesitated to designate a species for the ER 1470 skull, but eventually decided to call it *Homo habilis*. This strengthened the evidence for *Homo habilis* from Olduvai Gorge, announced by Louis Leakey in the 1960s.

What made the ER 1470 skull so unusual was its age. The stratum yielding the skull lay below the KBS Tuff, a volcanic deposit with a potassium-argon age of 2.6 million years. The skull itself was given an age of 2.9 million years, as old as the oldest australopithecines. The KBS Tuffs age was later challenged, with critics favoring an age of less than 2 million years (Section 11.6.5).

#### 11.6.2 Evolutionary Significance of the ER 1470 Skull

Louis Leakey was pleased with his son's discovery. ER 1470 vindicated his long-held view that a line of human ancestors, separate from *Australopithecus* and *Homo erectus*, extended far into the past.

Richard Leakey also believed his find had revolutionary implications for human evolution. "Either we toss out this skull or we toss out our theories of early man," he wrote in *National Geographic* (R. Leakey 1973b, p. 819). "It simply fits no previous models of human beginnings." The model most widely accepted involved three steps. *Australopithecus africanus*, with some specimens as much as 3 million years old (Groves 1989, p. 198), gave rise to early *Homo* (*H. habilis* and then *H. erectus*), which in turn gave rise to *Homo sapiens*. But Leakey (1973b, p. 819) believed that the ER 1470 skull, larger and more humanlike than that of *Australopithecus africanus*, "leaves in ruins the notion that all early fossils can be arranged in an orderly sequence of evolutionary change."

J. B. Birdsell (1975) of UCLA agreed this was true, even if the ER 1470 skull proved to be 2 million rather than 2.9 million years old. "From the very nature of its characteristics cranium 1470 does not seem to fit the standard scheme of the three grades of human evolution," he wrote in the second edition of his textbook *Human Evolution* (Fix 1984, p. 60).

In a *National Geographic* article, Richard Leakey included a chart showing two separate lines of hominid development. On one line, at about 3 million years ago, Leakey placed the ER 1470 hominid. Next on this line came *Homo habilis* at roughly 2 million years ago. At 1 million years ago, *Homo habilis* gave way to *Homo erectus*, which was followed at the very top of the chart by *Homo sapiens*.

The second (completely separate) line in Richard Leakey's chart showed *Australopithecus* starting at 3 million years ago and finishing at 1 million years ago. Leakey (1973b, p. 819) commented: "Probably a relative rather than a forebear of mankind, apelike *Australopithecus* existed for at least 2 million years before it reached an evolutionary dead end." Leakey believed, however, that further research would turn up a common ancestor for *Australopithecus* and the *Homo* line at around 4 million years ago.



Richard Leakey differed from his father by keeping *Homo erectus* in the direct line of human ancestry. 'Most people would now agree that '1470' should be called *Homo habilis* and that it is a direct ancestor of *Homo erectus*, he wrote (R. Leakey 1984, p. 154).

But the transition from ER 1470 to *Homo erectus* troubled Birdsell (1975), who wrote: "Anatomically in some ways such an evolutionary stage would seem retrogressive, for in a real sense it postulates that more archaic forms of men evolved out of a surprisingly advanced form. ER-1470" (Fix 1984. p. 137). Birdsell's statement is of interest because the progression from *Homo habilis* to *Homo erectus* is one of the cardinal doctrines of recent evolutionary thought. If this progression turns out to be improbable, that would present severe problems for the conventional account of human evolution. The progression is arguably improbable because it involves, for example. going from skull ER 1470, with moderate brow ridges, to *Homo erectus*, with massive barlike brow ridges, back to *Homo sapiens*. with small brow ridges.

Such difficulties did not, however, trouble Richard Leakey. Recently, he said he considers *Homo habilis* and *Homo erectus* to be nothing more than early stages of one species—*Homo sapiens* (Willis 1989, pp. 154-155).

Richard Leakey has made other interesting statements about human beginnings. For instance. he wrote in his book *Origins*. "If we are honest we have to admit we will never fully know what happened to our ancestors in their Journey towards modern humanity: the evidence is simply too sparse" (R. Leakey and Lewin 1977. pp. 11-12).

And in *People of the Lake* (R. Leakey and Lewin 1978, p. 17), Leakey said: "If someone went to the trouble of collecting together in one room all the fossil remains so far discovered of our ancestors (and their biological relatives) who lived, say, between five and one million years ago. he would need only a couple of large trestle tables on which to spread them out. . Yet with a confidence that may strike the uninitiated as something close to supernatural—if not to plain madness—prehistorians can now construct a view of human origins that is anything but crude, and may even bear some resemblance to the truth." The evidence on the trestle tables would not, of course, be complete. Much has been suppressed or forgotten, and if it were placed back on the tables, it would be harder for confident prehistorians to construct plausible evolutionary lineages.

#### 11.6.3 Humanlike Femurs From Koobi Fora

Some distance from where the ER 1470 skull had been found, but at the same level. John Harris, a paleontologist from the Kenya National Museum, discovered a quite humanlike upper leg bone. Harris summoned Richard Leakey (1973b, pp. 823. 828). who later reported: "Amid a mass of shattered elephant bone lay both ends of the femur of a remarkably advanced hominid. Further search turned up the missing pieces, parts of the tibia and a fragment of the fibula.

. John also discovered another femur. All these leg bones lay in deposits older than 2.6 million years. Do they belong to our new-found '1470 man?' Frustratingly, we cannot be sure. It is quite

clear, however, that these femurs are unlike those of *Australopithecus*. and astonishingly similar to those of modern man." The femurs would later be attributed to *Homo habilis*.

The first femur, with associated fragments of tibia and fibula, was designated ER 1481 and the other ER 1472. An additional fragment of femur was designated ER 1475. Like the ER 1470 skull, the femurs were found on the surface. But Richard Leakey (1973a. p. 448) wrote in *Nature*: "The unrolled condition of the specimens and the nature of the sites rules out the possibility of secondary deposition—there is no doubt in the minds of the geologists that the provenance is as reported. All the specimens are heavily mineralized and the adhering matrix is similar to the matrix seen on other fossils from the same sites." In other words. Leakey was certain the bones had recently weathered out of the fossil-bearing deposits from below the KBS Tuff.

Leakey (1973a, p. 450) stated in a scientific Journal that these leg bones "cannot be readily distinguished from *H. sapiens* if one considers the range of variation known for this species." In a *National Geographic* article, Leakey (1973b. p. 821) repeated this view, saying the leg bones were "almost indistinguishable from those of *Homo sapiens*."

Comparing the newly found ER 1481 femur with a femur of *Australopithecus*. Leakey (1973b. p. 828) said: "The more ovoid, less robust shaft neck of *Australopithecus* implies that the latter, though capable of walking upright, did so only for short periods." The "stronger neck shaft" of the new femur. Leakey (1973b. p. 828) added, "suggests its owner probably walked upright as his normal mode of locomotion. "

Concerning ER 1481. Richard Leakey (1973a, p. 450) wrote: "When the femur is compared with a restricted sample of modern African bones. there are marked similarities in those morphological features that are widely considered characteristic of modern *H. sapiens*. The fragments of tibia and fibula also resemble *H. sapiens*." He further stated: "The head of the femur is large and set on a robust cylindrical neck which takes off from the shaft at a more obtuse angle than in known *Australopithecus* femurs" (R. Leakey 1973a, pp. 449-450).

Other scientists agreed with Leakey's analysis. In 1976, B. A. Wood. anatomist at the Charing Cross Hospital Medical School in London, showed that in terms of three critical variables (femur neck length, femur head size, and femur neck shape), the ER 1472 and ER 1481 femurs always fell within a single standard deviation from the modern human mean. Wood (1976, p. 502) wrote: "The data . . . clearly show that femurs 1472 and 1481 from East Rudolf belong to the 'modern human walking' locomotor group." Christine Tardieu (1981), also identified several humanlike features of the lower parts of the ER 1481 and ER 1472 femurs. Other workers found the femurs different from those of *Homo erectus* (Section 11.7.1).

Although most scientists would never dream of it. one could consider attributing the Koobi Fora femurs to a hominid very much like modern *Homo sapiens*, living in Africa about 2.9 million years ago (about 2.0 million years ago if you choose to believe the revised date of 1.9 million years for the KBS Tuff).

The ER 1472 and ER 1481 femurs show that distinctly anomalous discoveries are not confined to the nineteenth century. They have continued to occur with astonishing regularity up to the present day, right under our very noses, so to speak, although hardly anyone recognizes them for what they are. In Africa alone, we are building up quite a catalog: Reck's skeleton, the Kanam Jaw, the Kanjera skulls, the Kanapoi humerus, the Gombore humerus, and now the Lake Turkana femurs. All have been either attributed to *Homo sapiens* or described as being very humanlike. Except for the Middle Pleistocene Kanjera skulls, all were discovered in Early Pleistocene or Pliocene contexts.

#### 11.6.4 The ER 813 Talus

In 1974, B. A. Wood (1974a, p. 135) described a talus (ankle bone) found between the KBS Tuff, then given an age of 2.6 million years, and the overlying Koobi Fora Tuff, with an age of 1.57 million years. Wood compared the fossil talus, designated ER 813, with hundreds of others, including those of modern humans, gorillas, chimpanzees, and other arboreal primates.

Using multivariate statistical techniques, Wood analyzed the ankle bones in terms of 3 angular and 5 linear measurements. He concluded: "In all the variates, the fossil aligned with the modern human tali" (Wood 1974a, p. 135). Wood further stated: "the functional implications of the canonical analysis results, combined with the close morphological affinity of the fossil talus with the modern human bones, make it possible that the locomotor pattern of this early hominid was like that of modern man" (1974a, p. 136).

If we accept the younger date for the KBS Tuff, the humanlike ER 813 talus would be 1.5 to 1.9 million years old, roughly contemporary with creatures designated as *Australopithecus robustus*, *Homo erectus*, and *Homo habilis*.

In a subsequent report, Wood (1976, pp. 500-501) said his tests confirmed "the similarity of KNM-ER 813 with modern human bones," showing it to be "not significantly different from the tali of modern bushmen." One could therefore consider the possibility that the KNM-ER 813 talus belonged to an anatomically modern human in the Early Pleistocene or Late Pliocene.

C. E. Oxnard (1975a, p. 121) wrote of ER 813: "description and examination using canonical analysis by Wood (1974) confirms that it is indeed very similar to modern man and is thus unlike the australopithecine specimens." Challenging the ancestral status of *Australopithecus*, Oxnard (1975a, p. 121) added: "Unless evolution took the talus through a stage where it was much like man (as at East Rudolf), then through a stage where it was uniquely different from man (as at Olduvai and possibly Kromdraai), and back again to a stage like man (modern man), then australopithecine fossils had to have been unrelated to any direct human line."

Of course, if the KNM-ER 813 talus really did belong to a creature very much like modern human beings, it fits, like the ER 1481 and ER 1472 femurs, into a continuum of such finds reaching back millions of years. In this case, any talk of an evolving human line, to which hominid fossils different from those of modern humans may be related, directly or indirectly, becomes irrelevant.

#### 11.6.5 The Age of The KBS Tuff

The KBS Tuff was named after Kay Behrensmeyer, the Yale geologist who first identified it. Such volcanic tuffs can be dated by the potassium-argon method. If the dated tuff can be properly traced over difficult terrain, it can be used to determine a minimum age for fossils found below it. Over the years, workers obtained differing potassium-argon ages for the KBS Tuff, with substantial impact on the dating of fossil hominids at Lake Turkana.

The potassium-argon (K/Ar) method relies on the decay of radioactive potassium 40 into argon, a stable gas. In principle, one can, by correctly measuring the amounts of potassium and argon in a sample, calculate its age. The more argon, the older the sample.

In practice, there are many difficulties in using this method. For the age range in question (2- 4 million years), the accumulation of argon is very small. The measurements are thus extremely sensitive to any artificial loss or gain of argon.

Exposed to weathering, a sample may lose some of its argon. In this case, the measured age would be younger than the sample's true age. If materials from older deposits get mixed into a sample, thus adding argon, the measured age would then be older than the true age of the sample.

In testing a sample, this question always arises: Has there been any argon loss or gain? In making such Judgements, the investigator has wide latitude for personal interpretation.

Illustrating the difficulties inherent in the potassium-argon method, scientists have obtained ages ranging from 160 million to 2.96 billion years for Hawaiian lava flows that occurred in the year 1800. A report in the Journal of Geophysical Research stated: "It is possible that some of the abnormally high potassium-argon ages . . . may be caused by the presence of excess argon contained in fluid and gaseous inclusions" ( Funkhouser and Naughton 1968, p. 4606).

Potassium-argon tests often yield such unexpected results, far older or younger than the generally accepted ages for the formations being dated. One researcher (Woodmorappe 1979) compiled a list of 275 discrepant potassium-argon dates. From his tables, we have selected a few representative dates from the geological era most relevant to our study, the later Tertiary (Table 11.3).

In a potassium-argon study of formations in the western United States, geologist R. L. Mager (1977, p. 37) stated: "In general, dates in the 'correct ballpark' are assumed to be correct and are published, but those in disagreement with other data are seldom published nor are discrepancies fully explained. " And geologist J. B. Waterhouse (1978, p. 316) noted: "It is, of course, all too facile to 'correct' various values by explanations of leakage, or initially high concentrations of strontium or argon.

TTA.BLE 11.3 Disciepaut Potassium-Argon Dates (La.tE'r Ifriisj ■

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Age

Ob^med

Filiation!

Original

Reference

5

1-10.6

B^ey Ash/CaliforniaT U.S.A.

Bandy and Ingle 1970

9.5

13-31

tuft'fNe\J'aoo, U.SA.

Cole 1978

10

95

b^asalt /Nig^eria

D. Fisher 1971

10

153

Nogales Fo^^tion U.S.A.

et al. 1973

23

3.4

Suuta Volcanic^ Solo^mon: Isl^an.ds

Chivas and MacDoisjfsJI 1976

27

31-43

volcanica'^Kamchatka. R.I.JSian Republic

Poz^w 1973

Data in this table af"e from J. Woi^odmorn^^ (1979, Table 1).

These explanations maybe correct, butttheymust first be related to a time line or cline ofvalues' itself subject to similar adjustments and corrections on a nonstatlstlcal and nonexperlmental basls."

This raises an important issue. E. T. Hall (1974, p. 15), director of Oxford's Research Laboratory for Archaeology and the History of Art, warned: "the greatest temptation is the one which leads an archaeologist selectively to believe evidence which seems to confirm the theories upon which he thinks his professional reputation rests. When the evidence comes from complex scientific techniques which are error prone and involve principles not wholly understood even by the scientists themselves, the dangers are great indeed."

Potassium-argon dating is such an error-prone technique. When, however, ordinary persons. or even scientists in disciplines other than those directly connected with the paleoanthropological enterprise, hear that a fossil has been dated by the potassium-argon method, they think the matter has been settled by science in a thoroughly reliable fashion. But when one gets beyond the screen of footnotes and suitably restrained phrasing in paleoanthropological reports, one frequently discovers that the dating is quite nebulous. The strongest argument in favor of a particular date is often the personal commitment of a scientist whose ideas are supported by the date.

Radiometric dates, said E. T. Hall (1974, p. 15), "tend to acquire a spurious infallibility for the layman or for quasi- scientists like archeologists. They believe because they want to believe. "

#### 11.6.5.2 The Potassium-Argon Dating of the KBS Tuff

In 1969, Richard Leakey sent samples of the KBS tuff to England for potassiumargon testing. According to E. T. Hall (1974, p. 15), the age obtainedwas a seemingly impossible 220 millionyears.

In 1970, F. T. Fitch and J. A. Miller, having received new samples, ran more potassium-argon tests and decided that the KBS Tuff was 2.6 million years old.

In 1972, Richard Leakey discovered the ER 1470 *Homo habilis* skull. Because the skull came from well below the KBS Tuff, he assigned it an age of 2.9 million years. This was controversial, because it made *Homo* as old as the oldest

*Australopithecus*.

Results of paleomagnetic studies by Dr. A. Brock of the University of Nairobi confirmed the potassium-argon date given by Fitch and Miller for the KBS Tuff. (For an explanation of the paleomagnetic dating method, see Section 9.2.10.)

Brock found that samples in and near the KBS Tuff were of normal polarity (Brock and Isaac 1974, p. 346). This was consistent with the potassium-argon date of 2.6 million years obtained by Fitch and Miller, which, if correct, falls in the upper part of the Gauss Normal Epoch (Figure 11.8). Below the KBS Tuff, the samples were predominantly of normal polarity. But Brock also found in this region two short intervals of reversed polarity, which he identified with the Kaena and Mammoth Events (Brock and Isaac 1974, p. 346). This tended to confirm that the KBS Tuff was in the Gauss Normal Epoch and was somewhere between 2.5 and 2.9 million years old.

Matuyama Reversed

Olduvai Event

2.48



Figure 11.8. This is the standard paleomagnetic polarity scale (after Wu, X. and Wang, L. 1985, p. 36). Brock and Isaac (1974) believed the polarity sequence at Koobi Fora supported an age of 2.6 million years, in the Gauss Normal Epoch, for the KBS Tuff.

Brock stated that skull ER 1470 came from "a level equivalent to that in which the Kaena and Mammoth events have been identified" (Brock and Isaac 1974, p. 347). He added: "An age of 2.7 to 3.0 Myr. . . is strongly indicated" (Brock and Isaac 1974, p. 347). Referring to the potassium-argon dates by Fitch and Miller, Brock concluded that "in every case the isotopic and paleomagnetic dates are consistent" (Brock and Isaac 1974, p. 347). Brock also found his version consistent with the faunal chronology prepared by Vincent J. Maglio.

Maglio identified marker fossils in the hominid-bearing sediments of the Lake Turkana sites. Skull ER 1470 was found in the zone containing *Mesochorus limnetes*, an extinct pig. Maglio compared the pig teeth found at Lake Turkana with those found at the Shungura site in Ethiopia, where it had been demonstrated that the pig teeth increased in size with the passage of time. At Shungura, teeth of the size found at Lake Turkana fell in a time period extending from 1.8 million to 2.7 million years ago.

Maglio (1972, p. 383) noted: "The indicated age range includes the K / Ar date of 2.6 my for the KBS tuff located within the sedimentary unit containing this fauna." Richard Leakey believed that Maglio's report supported his position on the ages of the KBS Tuff and the important ER 1470 skull.

But other scientists, who had different ideas about the relative antiquity of *Australopithecus* and *Homo*, were not happy about the potassium-argon age of 2.6 million years for the KBS Tuff (Oohanson and Edey 1981). They pointed to new faunal studies that seemed to make the KBS Tuff much younger.

Basil Cooke (1976), for example, said the size range of pig teeth from below the KBS Tuff matched that of Ethiopian pig teeth with an age of 2 million years. If this correlation were accepted, the KBS Tuff would have to be less than 2 million years old.

Richard Leakey proposed differential rates of evolution as a possible explanation. Perhaps the pigs at Lake Turkana and their teeth got bigger earlier than those in Ethiopia because of a more favorable environment.

The dispute over the pig teeth and the KBS Tuff was a major topic at a February 1975 conference in London. Fitch and Miller presented the results of new potassium-argon tests, which yielded an age for the KBS tuff of 2.4 million rather than

2.6 million years (R. Leakey 1984, p. 167).

Another group of scientists showed uranium fission track evidence confirming the new potassium-argon date of 2.4 million years (Leakey 1984, p. 168). When uranium 238, a radioactive element, decays into lead, particles released during fission leave tracks in zircon crystals. By measuring the amount of uranium and counting the tracks in the crystals, one can estimate the crystals' age.

At the London meeting, Basil Cooke used his fossil pig evidence to dispute the potassium-argon and fission track dates. But Richard Leakey (1984, p. 168), who did not regard Cooke's results as conclusive, strongly defended the potassium-argon age of 2.4 million years for the KBS Tuff.

Not long afterward, Garniss Curtis of the University of California published his own potassium-argon test results (Curtis et al. 1975, p. 395). He obtained ages of 1.6 million and 1.8 million years for the KBS Tuff.

Curtis said the samples tested by Fitch and Miller were probably contaminated with argon from older inclusions. Fitch and Miller said Curtis's samples possibly suffered argon loss, giving a date younger than the actual date of the tuff. Who was right? From the information provided in the published reports, it is hard to tell.

In one of their reports, Fitch and Miller (1976) did, however, give an interesting insight into potassium-argon dating procedures. They arranged some of their dated samples from the KBS Tuff into 4 groups, having average ages of 221 million years, 3.02 million years, 8.43 million years, and 17.5 million years.

They also listed over a dozen other individual samples with ages ranging from 0.52 to 2.54 million years. This bewildering array of dates comprises the actual results of the potassium-argon testing of samples from the KBS Tuff.

All dates older and younger than the ones finally published were thrown out, mainly because the researchers assumed the samples had been in some way contaminated or degassed. They proposed, for example, that flowing water could have mixed new and old volcanic materials or that water from hot springs could have released argon originally trapped in the sampled material (Fitch and Miller 1976, p. 125).

When Anthony J. Hurford and his associates published the conclusions of their fission track test, presented in preliminary form at the 1975 London meeting, they, like Fitch and Miller, disputed the 1.8 million year date for the KBS tuff obtained by Curtis. They stated: "Fission-track dating of zircon separated from two pumice samples from the KBS Tuff in the Koobi Fora Formation, in Area 131, East Rudolf, Kenya, gives an age of  $2.44 \pm 0.08$  Myr for the eruption of the pumice. This result is compatible with the previously published K-Ar and  $^{40}\text{Ar}/^{39}\text{Ar}$  age spectrum estimate of

2.61±0.26 Myr for the KBS Tuff in Area 105, but differs from the more recently published K-Ar date of 1.82±0.04 Myr for the KBS Tuff in Area 131. This study does not support the suggestion that pumice cobbles of different ages occur in the KBS Tuff" (Hurford et al. 1976, p. 738).

Curtis had suggested that the Fitch and Miller dates of 2.61 and 2.42 million years were the result of older pumice included in the KBS tuff. Hurford also pointed out that his results were compatible with the paleomagnetic results obtained earlier by Brock and Isaac (Hurford et al. 1976, p. 740).

In another development, it turned out that Curtis's potassium-argon age for the KBS Tuff was "flawed by an improperly adjusted weighing balance" (Johanson and Shreeve 1989, p. 99).

Meanwhile, Richard Leakey commissioned John Hens and Tim White to study the faunal conclusions reached by Basil Cooke. As it turned out, their investigation confirmed Cooke's results. Leakey, as leader of the Koobi Fora project, prevailed upon Harris to remove any mention of how this faunal evidence related to the hominids of Lake Turkana. White, in protest, asked to have his name removed from the paper before it was published. Harris did not remove White's name. The paper was rejected by Nature, but a revised version was published by the American Journal of Science in 1977 (Johanson and Edey 1981, pp. 240-242).

The controversy dragged on for several years. The younger age for the KBS Tuff was very much favored by Don Johanson and Tim White, who promoted *Australopithecus afarensis* (including "Lucy") as the ultimate ancestor of both *Homo habilis* and *Australopithecus africanus*. *Afarensis* was around 3 million years old. The skull of *afarensis* was typically australopithecine, small-brained with heavy brow ridges. Having the much bigger, smooth-browed ER

1470 cranium at around 2.9 million years, as Richard Leakey originally suggested, would have made *afarensis* an unlikely ancestor of ER 1470, classified as *Homo habilis*.

In order to put an end to the controversy, Richard Leakey decided to call in additional researchers. "It was only in 1980," wrote Leakey, "that a broad consensus was finally achieved. . . . Glynn [Isaac] and I decided we should invite other geophysicists to work on the KBS date. Eventually we managed to arrange for several different laboratories to evaluate the same material from split samples, using two methods: fission-track dating, as well as conventional potassium-argon. This was done quietly and with little fanfare. As a result, it became quite clear that the KBS tuff is no more than 1.9 million years old. . . . It would be prudent to think of the skull KNM-ER 1470 as being about two million years old" (R. Leakey 1984, p. 170).

The case of the KBS Tuff is intriguing. Initially, Leakey had potassium-argon dates, faunal evidence, paleomagnetic dates, and fission track dates supporting an age of 2.6 million years. Then, a few years later, he said new potassium-argon dates, faunal evidence, and fission track dates favored an age of 1.9 million years.

Richard Leakey's allusion to consensus is instructive. Researchers party to such an agreement may announce that their consensus must be correct because it is supported by dating methods A, B,

and C. But as we have seen, various dating methods tend to give age ranges broad enough to support a number of age determinations.

Many place excessive, even unquestioning, faith in published age determinations, unaware of the many sources of error inherent in current dating methods. They do not adequately appreciate the crucial role that the Judgements of individual researchers play in arriving at a published date from among the spread of dates often obtained from a series of tests. These complex Judgements can easily be influenced by the researcher's expectations and preconceptions.

#### 11.7 Oh 62: Will The Real Homo Habilis Please Stand Up?

Artists, working from fossils and reports supplied by paleoanthropologists, have typically depicted Homo habilis as having an essentially humanlike body except for its apelike head (Figure 11.9).

Figure 11.9. Left: This drawing (after Johnson and Edey 1981, p. 286) shows HomohabiHs, as generally depicted before 1987. Below the head, the anatomy is essentially human. Right: After OH 62 was found at Olduvai Gorge in 1987, a new picture of Homo habilis (after Budiansky 1987, p. 10) emerged. far smaller and more apelike

than before.

Occasionally, scientists have raised questions about such depictions. "Were the australopithecines hairy? Was Homo habilis slightly less hairy, Just to give it a hint of human respectability?" asked Richard Leakey. 'Certainly, al the portraits ever painted of our ancestors show this kind of pattern. But as no artist has ever seen a living hominid, and as we have no way of knowing whether they were naked or not, It will remain a favorite topic of after-dinner speculation and fantasy forevermore" (Leakey and Lewin 1978, p. 66).

In any case, a very humanlike portrait of Homo habilis persisted until 1987. In that year, Tim White and Don Johanson reported they had found in lower Bed I at Olduvai the first Homo habilis individual (OH 62) with postcranial bones clearly associated with a cranium.

Johanson and his coworkers (1987, p. 205) stated: "This specimen's

cranlodental anatomy indicates attribution to Homo habilis. but its postcranial anatomy, including small body size [ less than 3.5 feet] and relatively long anns, is striking similar to that of some early Australopithecus individuals." Drawings of the new Homo habilis (Figure 11.9) were decidedly more apelike than those of the past.

Wood (1987, p. 188) noted: "The shape and size of the proximal femur, and the anatomy and relative lengths of the limb bones, both run counter to the view which sees *H. habilis* as a biped with a postcranial skeleton that is essentially modern human in its morphology, proportions and, by inference, function."

Johanson and his coworkers (1987, p. 209) concluded it was likely that scientists had incorrectly attributed to *Homo habilis* many postcranial bones discovered prior to 1987.

#### 11.7.1 Implications for the ER 1481 and ER 1472 Femurs

The OH 62 find supports our suggestion that the ER 1481 and ER 1472 femurs from Koobi Fora, described as very much like those of modern *Homo sapiens* (Section 11.6.3), might have belonged to anatomically modern humans living in Africa during the Late Pliocene. These femurs have been attributed by some workers to *Homo habilis* and by others to *Homo erectus*. But these attributions are questionable. Showing this<sup>11</sup>, however, take a few paragraphs of unavoidably obscure and intricate analysis of bone morphology.

In his book *Lucy's Child* (Johanson and Shreeve 1989, photo section), D. Johanson suggested that the comparatively large ER 1481 femur was the *Homo habilis* male counterpart to the smaller OH 62 female *Homo habilis* femur.

But the attribution of the ER 1481 femur to the same species as OH 62 involves a

Figure 11.10. (Traced from Johanson and Shreeve 1989, photo section.) According to D. Johanson, the KNM-ER 1481 femur from Koobi Fora, Kenya, is from a male *Homo habilis*. It is, however, much larger than the OH 62 female *Homo habilis* femur from Olduvai Gorge. Attributing both femurs to the same species implies an unusual degree of sexual dimorphism. They display a greater size difference than the male (AL 333-3) and female (AL 288) *Australopithecus afarensis* femurs from Hadar, Ethiopia. Some workers have said that the degree of sexual dimorphism in the Hadar sample is too great to be accommodated within a single species. The same may be true of the KNM-ER 1481 and OH 62 femurs.

remarkable degree of sexual dimorphism for *Homo habilis*. The ER 1481 femur is much bigger than the OH 62 female femur (Figure 11.10).

Johanson believed, however, that the femurs of Lucy (AL 288) and a male hominid (AL 333-3) from the Hadar, Ethiopia, site displayed a degree of sexual dimorphism similar to that of the OH 62 and ER 1481 femurs. This made it conceivable, to Johanson, that the OH 62 and ER 1481 femurs might

belong to a single species of hominid. To us, however, the degree of sexual dimorphism in OH 62 and ER 1481 appears much greater than that in the Hadar femurs.

Furthermore, although Johanson thought that both Hadar femurs belonged to one species (*Australopithecus afarensis*), other paleoanthropologists have said that the AL 333-3 femur, along with many other fossils attributed by Johanson to

*Australopithecus afarensis*, actually belonged to *Homo* individuals (Groves 1989, pp. 260-263).

One of these workers (Zihlman 1985, pp. 216-217) demonstrated that putting all the Hadar hominids in one species would involve sexual dimorphism more extreme than that encountered in the most sexually dimorphic anthropoid apes (Section 11.9.8). Zihlman therefore believed Johanson was not justified in assigning all the Hadar fossils to a single species.

If the Hadar fossils were too sexually dimorphic to be included in one species, we believe the same would be true of the ER 1481 and OH 62 femurs, which seem to manifest an even greater degree of sexual dimorphism than the Hadar femurs.

Johanson's attribution of the ER 1481 and OH 62 femurs to a single species appears to be a consequence of his belief that only one hominid species other than *Australopithecus boisei* (namely, *Homo habilis*) existed around 2 million years ago in East Africa (Willis 1989, p. 263).

As we shall see in Section 11.7.5, some workers have suggested that *Homo habilis* represents at least two species, including, perhaps, an australopithecine. Wood (1987), for example, proposed that small, apelike OH 62 might represent an East African gracile australopithecine rather than *Homo habilis*.

Accepting this, one might try to keep the traditional picture of *Homo habilis*. One could then, as previously, attribute the ER 1481 and ER 1472 femurs to *Homo habilis*, as represented by the somewhat humanlike ER 1470 skull. But the ER 1481 and ER 1472 femurs were found some distance from the ER 1470 skull, which means there is no solid reason to connect them. Attribution of the ER 1481 and ER 1472 femurs to *Homo habilis* is therefore questionable.

Some workers have suggested that the ER 1481 and ER 1472 femurs, and other bones attributed to *Homo habilis*, should be attributed to *Homo erectus* (Wood 1987, p. 188).

Even before the discovery of OH 62, Kennedy (1983) assigned the ER 1481 femur to *Homo erectus*. Kennedy's view would involve extending the age of African *Homo erectus* from about 1.6 million to at least 2 million years, since femur ER 1481 was found below the KBS Tuff at Koobi Fora.

In coming to her conclusion, Kennedy relied on comparative analysis of several femoral shaft measurements. But Trinkaus (1984, p. 137) noted that out of these measurements only one, the midshaft diameter, showed a "significant difference" (more than two standard deviations from the mean) from a sample of early anatomically modern human femurs. Trinkaus's early anatomically modern human sample included 24 fossil femurs from Cro-Magnon, Predmost, and other early

Homo sapiens sapiens sites. We suspect, however, that if the midshaft diameter of ER 1481 were compared with a sample that represented the total variation among living humans, it would fall closer to the mean. The other femoral shaft measurements of ER 1481 reported by Kennedy all fell within the range of early anatomically modern humans. This suggests that ER 1481 might be assigned to Homo sapiens rather than Homo erectus.

There are other reasons why attribution of the ER 1481 and ER 1472 femurs to Homo erectus is questionable. Since the discovery of Java man in the 1890s, scientists have written numerous books and articles describing femurs said to be those of Homo erectus. But until recently, no femurs, or other postcranial bones, have ever been found in direct connection with a cranium of Homo erectus. Therefore, it is not absolutely certain that any of the femurs scientists had previously described actually belonged to Homo erectus individuals.

In 1984, however, members of Richard Leakey's team found a Homo erectus boy (KNM-WT 15000) at Lake Turkana. KNM-WT 15000 was assigned an age of 1.6 million years. The skeleton comprised associated cranial and postcranial elements, including the femur (Brown et al. 1985, p. 788).

According to the discoverers (Brown et al. 1985, p. 791), several features of the KNM-WT 15000 Homo erectus femur were different from those normally encountered in Homo sapiens. Other workers (Oohanson et al. 1987, p. 209) also called attention to "Australopithecis-like aspects of . . . proximal femoral anatomy in early Homo erectus (KNM-WT 15000)." On the other hand, several workers have found the KNM-ER 1481 femur to be very much like modern human femurs and unlike those of australopithecines (Section 11.6.3).

Furthermore, Day and Molleson (1973, p. 128) said that most of the hominid femurs generally attributed to Homo erectus (such as the OH 28 femur from Olduvai Gorge and the Sinanthropus femurs from Zhoukoudian) were unlike those of modern human beings.

But Day and Molleson found the Java man femurs from Trinil, generally classified as Homo erectus, to be distinct from the OH 28 and Chinese Homo erectus femurs and almost identical to those of modern humans. They thus concluded that the Trinil femurs belonged not to Homo erectus but to anatomically modern humans (Section 7.1.8). This may put Homo sapiens sapiens at the Trinil site about 800,000 years ago.

In a study by Wood (1976), the ER 1481 and ER 1472 femurs came closer to the human mean in several key features than the Trinil I femur, which Day and Molleson said was anatomically modern and distinct from that of Homo erectus.

All of this suggests that it would not be correct to assign the anatomically modern ER 1481 and ER 1472 femurs to either

Homo erectus or Homo habilis.

#### 11.7.2 The Leap From Oh 62 to KNM-WT 15000

The discoverers of OH 62 had to grapple with the evolutionary link between the new, more apelike *Homo habilis* and *Homo erectus*. "The juxtaposition of an otherwise relatively derived *H. erectus* postcranium at - 1.6 Myr (KNM-WT 15000) and a postcranially primitive *H. habilis* at - 1.8 Myr (OH 62) may imply an abrupt transition between these taxa in eastern Africa," they stated (Johanson et al. 1987, p. 209). In paleoanthropology, the term "derived" is applied to a skeletal element that has supposedly undergone a significant and progressive morphological change relative to the same element in a supposedly ancestral form.

The *H. habilis*-*H. erectus* transition proposed by Johanson involves some rather extreme morphological changes, including a big change in size. Richard Leakey, applying normal human growth patterns, said that the *Homo erectus* boy, who was 5.6 feet tall, would probably have grown to over 6 feet tall as an adult. The female OH 62, on the other hand, was only about 3.25 feet tall, smaller than Lucy, who was about 3.5 feet tall.

How tall were the OH 62-type males? That is hard to say. Some presumably male *Australopithecus afarensis* individuals from the same Hadar, Ethiopia, site as Lucy may have been as much as 5 feet tall. On this basis, one might propose that an OH 62-type male might have been almost 5 feet tall. But, as we have seen, some workers say the size difference between the large and small Hadar specimens is too great to be accommodated within a single sexually dimorphic species. It seems likely, therefore, that the male companion to the 3.25-foot-tall OH 62 adult female was not much more than 4 feet tall. Altogether, an evolutionary leap from small, apelike OH 62 to big, humanlike KNM-WT 15000 in less than 200,000 years seems implausible.

Advocates of the much-debated punctuational model of evolution, however, can easily accept the transition. Unlike the traditional gradualists, punctuationalists assert that evolution proceeds by rapid episodes of change interrupted by long periods of stasis. The periods of change are so brief, say the punctuationalists, that intermediate forms are rarely preserved in the fossil record. Punctuationalism can, therefore, accommodate a variety of troublesome evolutionary anomalies, such as the *habilis* to *erectus* transition proposed by Johanson.

"The very small body size of the OH 62 individual," said its discoverers, "suggests that views of human evolution positing incremental body size increase through time may be rooted in gradualistic preconceptions rather than fact" (Johanson et al. 1987, p. 209). But punctuational views may also be rooted in preconception rather than fact. The paleontological facts, considered in their entirety, suggest that various ape-man-like and humanlike beings, including some resembling modern humans, coexisted throughout the Pleistocene, and earlier.

In summary, the OH 62 specimen, seen as *Homo habilis*, delivers a triple blow to conventional ideas about human evolution. (1) OH 62 shatters the prevailing humanlike portrayal of *Homo habilis*, as presented in book and magazine illustrations, television shows, and museum exhibits. (2) The primitive morphology of OH 62 raises questions about the taxonomic status of very humanlike postcranial bones, such as the ER 1481 femur, which have been attributed to *Homo habilis*. To what kind of hominid should they now be assigned? It is possible they belonged to an anatomically modern human species that coexisted with *Homo habilis*, the australopithecines, and



Homo erectus around 2 million years ago in Africa. (3) The size and geological age of OH 62 make the conventionally accepted evolutionary transition from Homo habilis to Homo erectus less plausible. Of course, if one were to classify OH 62 as an australopithecine that would resolve some of these difficulties.

### 11.7.3 Conflicting Assessments of Other Homo Habilis Fossils

It was not only new evidence such as OH 62 that challenged the long-accepted picture of Homo habilis. Previously discovered fossil evidence relating to Homo habilis, originally interpreted by some authorities as very humanlike, was later characterized by others as quite apelike.

#### 11.7.3.1 The OH 8 Foot

As mentioned earlier (Section 11.4.2), a fairly complete foot skeleton, labeled OH 8, was found in Bed I at Olduvai Gorge. Dated at 1.7 million years, the OH 8 foot was associated with other fossils classified by L. Leakey as Homo habilis (OH 7) and was also attributed to this species (Lewis 1980, pp. 275, 290).

M. H. Day and J. R. Napier (1964) said the OH 8 foot very much resembled that of Homo sapiens, thus contributing to the overall humanlike picture of Homo habilis. According to Day and Napier, the OH 8 foot showed that Homo habilis walked upright.

But O. J. Lewis (1980, p. 291), anatomist at St. Bartholomew's Hospital Medical College in London, wrote: "The attribution of these remains to the taxon Homo has been a source of controversy." He showed the functional morphology of the OH 8 foot was more like that of chimpanzees and gorillas (Table 11.4).

TABLE 11.4

1. Articulations between the metatarsals "are like the chimpanzee" (p. 294).
2. Ankle joint surfaces "retain the apelike form" (p. 291).
3. Form of the talus (ankle bone) is like that "seen in the extant African apes" (p. 291).
4. Disposition of the heel similar to that of gorillas and chimpanzees (p. 291).
5. Hallux (large toe) capable of being extended sideways, with some "residual grasping functions" (p. 293).

#### Apelike Features of the OH 8 Foot Reported by O. J. Lewis (1980)

Commenting on the 1964 study by Day and Napier, Lewis (1980, p. 294) noted that "conservative arboreal features of the tarsus [ankle] . . . escaped comment." The suggestion that the OH 8 ankle manifested arboreal features is intriguing. It certainly does not serve the propaganda purposes of evolutionists to have the public visualizing a supposed human ancestor like Homo habilis climbing trees with an arboreally adapted foot rather than walking tall and brave across the African

savannahs. When the owner of the OH 8 foot did walk on the ground, it probably did so in a chimpanzeelike manner, said Lewis (1980, p. 296).

From Lewis's study of the OH 8 foot, one could therefore conclude that *Homo habilis* was much more apelike than most scientists have tended to believe. The OH 62 discovery supports this view. Another possible conclusion: the OH 8 foot did not belong to *Homo habilis* but to an australopithecine. This view was favored by Wood (1974b) and Lewis (1980, p. 295). A related conclusion is that *Homo habilis* itself was, as Oxnard (1975b) proposed, simply a variant of *Australopithecus*. Oxnard, said Lewis (1980, p. 295), thought "the australopithecines (including OH 8) were at least partially arboreal primates retaining efficient climbing capabilities associated with a bipedal capacity probably of a type no longer seen." Of course, the proposal that *Australopithecus* was even partially arboreal defies the conventional view that this creature was humanlike from the neck down and walked fully upright on the ground. In Section 11.8, we give a detailed discussion of this issue.

Over the years, scientists have described the OH 8 foot skeleton as humanlike (Day and Napier 1964), apelike (Lewis 1980), intermediate between human and ape (Day and Wood 1968), distinct from both human and ape (Oxnard 1972), and orangutanlike (Lisowski et al. 1974). This demonstrates once more an important characteristic of paleoanthropological evidence — it is often subject to multiple, contradictory interpretations. Partisan considerations often determine which view prevails at any given point in time.

#### 11.7.3.2 The OH 7 Hand

The OH 7 hand was also found at Olduvai Gorge (Section 11.4.2), as part of the type specimen of *Homo habilis*. Napier (1962, p. 409) described the hand as quite human in some of its features, especially the finger tips. As in the case of the OH 8 foot, subsequent studies showed the OH 7 hand to be very apelike, calling into question either its attribution to *Homo habilis* or the generally accepted humanlike picture of *Homo habilis*, which the original interpretation of the OH 7 hand helped create.

C. E. Oxnard (1984, p. 334-11) was highly critical of Napier's original study of the *Homo habilis* hand: "being convinced that he was looking at a pre-human hand that made tools, he interpreted three features in which that hand was similar to a human hand as more weighty than ten in which he found it similar to those of apes." Oxnard identified evolutionary bias (seeing a fossil as "pre-human") as the key factor in Napier's attempt to characterize an essentially apelike structure as human.

Randall L. Susman and Jack T. Stern noted that the OH 7 finger bones had large areas for the insertion of a muscle (the flexor digitorum superficialis) that apes use when hanging from branches. "The impressions for this muscle are greater in relative area than in any living ape or modern humans," they said (Susman and Stern 1979, p. 572).

Susman and Stern (1979, p. 565) therefore concluded: "Prominent markings for insertions of these muscles in a fossil hand (such as O.H. 7) suggest use of the forelimb in suspensory climbing behavior."

Susman and Stern (1979, p. 572) noted in addition that the finger bones of the OH 7 hand were thick and cuived like those of chimpanzees, indicating, like the flexor digitorum superficialis muscle, a degree of arboreal suspensory behavior.

In others words, Homo habilis, or whatever creature owned the OH 7 hand, may have spent much of its time hanging by its arms from tree limbs. This apelike image differs from the very humanlike portrait of Homo habilis and other supposed human ancestors one usually encounters in Time-Life picture books and National Geographic Society television specials.

#### 11.7.4 Cultural Level of Homo Habilis

A reevaluation of the cultural evidence at Homo habilis sites also casts doubt on the conventional humanlike interpretation of Homo habilis.

Louis and Mary Leakey designated the Homo habilis sites at Olduvai as "living floors." They viewed particular combinations of hominid and animal fossils, along with stone tools, as signs of permanent or semipermanent habitation. From such interpretations of the evidence came detailed paintings, showing Homo habilis families living in base camps, with hunting parties returning with animal carcasses to be butchered with stone tools.

But according to Binford (1981, p. 252), the Leakeys' characterization of Homo habilis sites as "living floors" was the result of wishful thinking: "the researchers have a generalized idea as to what the past was like and they have then accommodated all the archaeological-geological facts to this idea. This is not exactly science." Binford went on to criticize the notion of living floors in terms of their "integrity" and "resolution."

Binford believed the Homo habilis sites were of low integrity. By this he meant there was no certainty that Homo habilis was in fact responsible for the animal bones found at the sites. The bones could very well have been the result of natural deaths, which would have occurred fairly often on the shores of the ancient lake that deposited the sediments at Olduvai. The bones might also have been brought to their resting places by carnivorous animals rather than hominids.

For Binford, the term "resolution" meant the time during which the faunal remains and artifacts were deposited. For the concept of a "living floor" to be meaningful, the resolution should be quite high—that is to say, the faunal remains and artifacts should have been deposited over a relatively short period of time. But Binford believed that the resolution at the Homo habilis sites at Olduvai Gorge was low, and that the faunal remains and artifacts were deposited over very long periods of time. This would decrease the certainty that hominid behavior was responsible for the association of a particular assemblage of bones and artifacts.

If, for example, one interprets a scatter of stones and bones as having been deposited simultaneously, one might talk of a habitation site. But if the bones and artifacts were deposited one by one over the course of hundreds or thousands of years, as animals chanced to die, and scavenging hominids chanced to drop stone tools, the supposition that one has found a habitation site becomes far less likely.

About the reputed living floor at the famous Zinjanthropus site at Olduvai, where remains of *Homo habilis* were also found nearby, Binford (1981, p. 282) said: "given its demonstrably low integrity and resolution, arguments about base camps, hominid hunting, sharing of food, and so forth are certainly premature and most likely wildly inaccurate. The only clear picture obtained is that of a hominid scavenging the kills and death sites of other predator-scavengers for abandoned anatomical parts of low food utility, primarily for purposes of extracting bone marrow. Some removal of marrow bones from kills is indicated, but there is no evidence of 'carrying food home.' Transport of the scavenged parts away from the kill site to more protected locations in a manner identical to that of all other scavengers is all that one need imagine to account for the unambiguous facts preserved in Olduvai."

Thus, according to Binford, *Homo habilis* was definitely not a hunter. In fact, Binford has concluded that hunting is an activity exclusively characteristic of modern *Homo sapiens*. "There are many people," he said, "who are just outraged because I've suggested that early men, including the Neanderthals, weren't hunters" (A. Fisher 1988a, p. 37).

There are some scientists, such as Henry Bunn of the University of Wisconsin (A. Fisher 1988a, p. 38), who have disputed Binford's conclusions about the Olduvai sites. Nevertheless, Binford's analysis provides a refreshing alternative to the usual overly humanized presentation of "*Homo*" *habilis*.

"There were all these wonderful renderings in popular magazines and books of little bands of bushmanlike people sitting around with daddy off hunting and momma gathering plant foods and grandma teaching the baby. But that was just a projection of modern man onto ancient man," declared Binford in an interview (A. Fisher 1988a, p. 37). "We have had far too much of what I tend to think of as the National Geographic approach to research," said Binford (1981, p. 297).

Binford's revised view of the cultural evidence at *Homo habilis* sites, together with the revised view of *Homo habilis* anatomy, raises many questions about how humanlike *Homo habilis* really was.

Finally, we should remember that *Homo habilis* is not the only creature that could have been responsible for the stone tools found at sites yielding *Homo habilis* fossils. The same is true of the circle of stones found at Olduvai site DK, interpreted by some as part of a shelter. Mary Leakey said that living African tribal people make and use the same kinds of tools and erect the same kinds of shelters (Section 3.7.3). This suggests that beings like modern *Homo sapiens*, rather than *Homo habilis*, could have made both the tools and the shelter about 1.5 million to 2.0 million years ago in the Early Pleistocene.

#### 11.7.5 Does Homo Habilis Deserve To Exist?

In light of the contradictory evidence connected with *Homo habilis*, some researchers have proposed that there was no justification for "creating" this species in the first place.

Doubts about the taxonomic reality of *Homo habilis* arose right from the start. Even Tobias and Napier, who had joined Louis Leakey in proposing the new species in April of 1964, expressed caution. Tobias and Napier wrote in a letter to the Times of London on June 5, 1964 that "anatomy alone could not tell us whether the creature was a very advanced australopithecine or the lowliest hominine" (Cole 1975, p. 256). In making this statement, Tobias and Napier presumably meant that stone tools and broken animal bones associated with the creature's ambiguous skeletal remains justified designating it the earliest representative of the genus *Homo*.

The dental evidence was a cause of concern among some researchers, including T. J. Robinson. Johanson wrote: "He said that one could find greater shape differences in a population of modern humans than Leakey had found between *habilis* and the australopithecines— or, in fact, between *habilis* and *Homo erectus*. Robinson's point was that on dental evidence alone there was too narrow a slot between *Australopithecis* and *Homo erectus* to yield room for another species" (Johanson and Edey 1981, p. 102). As we have seen, however, there are, aside from the teeth, significant differences between *Homo habilis*, as represented by the small OH 62 individual, and *Homo erectus*.

Wilfred Le Gros Clark said: " '*Homo habilis*' has received a good deal of publicity since his sudden appearance was announced, and it is particularly unfortunate that he should have been announced before a full and detailed study of all the relevant fossils can be complete. . . . From the brief accounts that have been published, one is led to hope that he will disappear as rapidly as he came" (Fix 1984, p. 143). Le Gros Clark consistently maintained his early opposition to *Homo habilis*.

And C. Loring Brace wrote: " *Homo habilis* is an empty taxon inadequately proposed and should be formally sunk" (Fix 1984, p. 143).

If the bones attributed by some workers to *Homo habilis* were not to be interpreted as a new species, then what did they represent? T. J. Robinson argued that *Homo habilis* had been mistakenly derived from a mixture of skeletal elements belonging to *Australopithecus africanus* and *Homo erectus*. Even Louis Leakey suggested that *Homo habilis* might actually have embraced two *Homo* species, one giving rise to *Homo sapiens* and the other to *Homo erectus* (Wood 1987, p. 187).

Concerning the new OH 62 discovery, Wood pointed out that this hominid individual had been classified as *Homo habilis* by Johanson and his coworkers primarily because its craniodental remains resembled those of the *habilis*-like Stw 53 skull from the Sterkfontein site in South Africa. But Wood (1987, p. 188) observed: "The logical 'trail' becomes tenuous because Stw 53 has merely been likened to *H. habilis*, and not formally attributed to it, even though more than a decade has

elapsed since its discovery." Wood appeared to suggest, though somewhat indirectly, that OH 62 might in fact be attributed to *Australopithecus africanus*, which he said was "the most likely alternative taxonomic attribution for Stw 53" (1987, p. 188).

According to Wood (1987, p. 187), one interpretation of the OH 62 find is that it "confirms that the range of variation within material from the early Pleistocene of East Africa assigned to early *Homo* is now too great to be sensibly encompassed within one taxon." Wood himself favored this view.

So in the end, we find that *Homo habilis* is about as substantial as a desert mirage, appearing now humanlike, now apelike, now real, now unreal, according to the tendency of the viewer. Taking the many conflicting views into consideration, we find it most likely that the *Homo habilis* material belongs to more than one species, including a small, apelike, arboreal australopithecine (OH 62 and some of the Olduvai specimens), an early species of *Homo* (ER 1470 skull), and anatomically modern humans (ER 1481 and ER 1472 femurs).

### 11.8 Oxnard's Critique of *Australopithecus*

According to most paleoanthropologists, *Australopithecus* was a direct human ancestor, with a very humanlike postcranial anatomy. Advocates of this view have also asserted that *Australopithecus* walked erect, in a manner practically identical to modern human beings. But right from the very start, some researchers objected to this depiction of *Australopithecus*. Influential English scientists, including Sir Arthur Keith (1931), said that the *Australopithecus* was not a hominid but a variety of ape (Sections 11.3.1-3).

This negative view persisted until the early 1950s, when the combined effect of further *Australopithecus* finds and the fall of Piltdown man created a niche in mainstream paleoanthropological thought for a humanlike *Australopithecus*.

But even after *Australopithecus* won mainstream acceptance as a hominid and direct human ancestor, opposition continued. Louis Leakey (1960d, 1971) held that *Australopithecus* was an early and very apelike offshoot from the main line of human evolution (Section 11.4.3). Later, his son Richard Leakey (1973b) took much the same stance (Section 11.6.2).

In the early 1950s, Sir Solly Zuckerman (1954) published extensive biometric studies showing *Australopithecus* was not as humanlike as imagined by those who favored putting this creature in the lineage of *Homo sapiens*. From the late 1960s through the 1980s, Charles E. Oxnard of the University of Chicago, employing multivariate statistical analysis, renewed and amplified the line of attack begun by Zuckerman.

In this section, we shall focus on Oxnard's studies of *Australopithecus*, except those dealing specifically with *Australopithecus afarensis* (Lucy). The latter are included in our general discussion of *Australopithecus afarensis* (Section 11.9).

#### 11.8.1 A Different Picture of *Australopithecus*

In *Uniqueness and Diversity in Human Evolution*, Oxnard (1975a, p. vii) wrote: "Whereas the conventional wisdom about human evolution depends upon the (apparent) marked similarity between modern man and the various australopithecine fossils, the studies here indicate that these fossils are uniquely different from modern man in many respects."

Oxnard's Interpretation of the fossil evidence profoundly unsettles the evolutionary status of *Australopithecus*. According to Oxnard (1975b, p. 394), "it is rather unlikely that any of the *Australopithecines* . . . can have any direct phylogenetic link with the genus *Homo*."

In Table 11.5, we review the observations that led Oxnard to this conclusion. The table also includes material from Zuckerman's studies.

Oxnard believed there is much that remains to be known about *Australopithecus*. and that what we do know does not conform to the customary image of this creature. Oxnard (1975a, p. 123) observed: "All of this makes us wonder about the usual presentation of human evolution in encyclopedias and popular publications, where not only are the australopithecines described as being of known bodily size and shape, but where, in addition, such characteristics as bipedality . . . and even facial features are happily reconstructed."

TABLE 11.5

Anatomical Features cited by S. Zuckerman and C. E. Oxnard indicating that *Australopithecines* were not human ancestors

#### Brain:

"endocranial casts of the *Australopithecinae* ... do not appear to diverge in any material way from existing apes" (Zuckerman 1954, p. 305).

"estimates of endocranial volume do not depart from the range of size met with in the great apes" (Zuckerman 1954, p. 304).

"suggestions that the *Australopithecinae* may have had higher relative brain weights than, say, chimpanzees" have not been substantiated (Zuckerman 1954, p. 304).

#### Teeth and jaws:

'with the exception of their incisors and canines, the size and general shape of the [australopithecine] jaws and teeth . . . were very much more like those of the living apes than like acknowledged members of the *Hominidae*, either living or extinct" (Zuckerman 1954, pp. 306-307).

#### Shape of skull:

"resembles. . . the ape—so much so that only detailed and close studies can reveal the difference between them" (Zuckennan 1954, p. 307).

Shoulder Bone (sterkfontein sts 7 scapula).'

"does not resemble that of man to any degree. . . almost as well-adapted structurally for suspension of the body by the limbs as is the corresponding part of

the present-day gibbon. . . more specialized in this respect than in even the highly specialized chimpanzee" (Oxnard 1968, p. 215). Oxnard dismissed suggestions that the Sterkfontein scapula was too distorted to yield accurate measurements. He also rejected accusations that the scapula was nonhominid.

Has an abnormally large area for attachment of the biceps muscle, which must have been extraordinarily well developed, as it is in the gibbons (Oxnard 1968, p. 215).

Collar Bone (olduvai oh 48 Homo habilis clavicle):

"whereas in humans the clavicle is scarcely twisted at all, in the various apes, as in the Olduvai clavicle, it is heavily twisted. This particular feature does not fit with the idea that the fossils are functionally close to man" (Oxnard 1984, p. 323). Oxnard, like others (Section 11.7.5), considered Homo habilis to be an australopithecine.

Hand Bones:

"quite different from those of humans. . . evidence seems to relate to abilities for grasping with power reminiscent of what we find in the orang-utan. . . some are curved enough that they must have operated in this arboreal- grasping mode" (Oxnard 1984, p. 311, citing Susman 1979, Susman and Creel 1979, Susman and Stern 1979).

Engineering stress analysis showed Australopithecus fingers were inefficient in the chimpanzee knuckle- walking mode but "efficient in the hanging-climbing mode as also is the orang-utan" (Oxnard 1984, p. 313). Human finger structure was "inefficient in both modes" (Oxnard 1984, p. 314).

Pelvis (including sterkfontein Sts 14):

"although there is no doubt about the similarity in shape of the iliac bones of man and Sterkfontein pelvis . . . it is also clear that this blade is positioned quite differently in man and the fossil" (Oxnard 1975a, p. 52).

Joint structure in the australopithecine hip "apparently not inconsistent with quadrupedalism" (Zuckerman et al. 1973, p. 152).

Muscle attachments not "inconsistent with . . . an occasional or habitual quadrupedal gait" (Zuckerman et al.



1973, p. 152).

Pelvic structure points to hindlimb capable of "an 'acrobatic' function" (Zuckerman et al. 1973, p. 156).

Pubis and Ischium (bones of the lower part of the pelvis) chimpanzee-like (Zuckerman 1954, p. 313).

Femurs:

'show the small heads and inclined femoral necks that might be expected in animals capable of quadrupedal activities' (Oxnard 1975b, p. 394).

Talus (ankle bone):

"the general morphological similarity is with the arboreal ape Pongo" (Oxnard 1975a, pp. 86-87). Pongo

Figure 11.11. Most scientists describe Australopithecus as an exclusively terrestrial biped, humanlike from the head down. But according to some studies by S. Zuckerman and C. E. Oxnard, Australopithecus was more apelike. Although capable of walking on the ground bipedally (left), Australopithecus was also "at home in the trees, capable of climbing, performing degrees of acrobatics (right) and perhaps of arm suspension" (Oxnard 1975a, p. 89). The unique functional morphology of Australopithecus led Zuckerman and Oxnard to doubt it is a human ancestor. Illustrations by Miles Tripplett.

## 11.8 .2 The Pelvis of Australopithecus

Of particular interest is the Australopithecus pelvis. Scientists who believe humans evolved from australopithecines often assert that the Australopithecus pelvis is similar to that of modern Homo sapiens. In both humans and australopithecines, the ilium, the broad upper part of the pelvis, is of roughly the same shape. The ilium of the chimp is more narrow (Figure 11.12). Some researchers have taken the visual resemblance between the human ilium and that of Australopithecus as proof that Australopithecus stood upright and walked very much like modern human beings.

But the impact of this demonstration is reduced when one considers the orientation of the ilium to the rest of the pelvis in apes, humans, and australopithecines. The comparison can best be made when the hip sockets are turned toward the viewer (Figure 11.13).

As can be seen, the ape ilium is situated in a manner different from that of a human being. The ape's iliac blade is oriented so that only the edge is visible.

In Australopithecus, the ilium is oriented like that of apes rather than humans (Oxnard 1984, p. 311). To Oxnard and

Zuckerman, this suggested apelike or uniquely nonhuman elements in the musculature, posture, and locomotor pattern of Australopithecus.

The typical visual presentation of the human ilium and that of Australopithecus, showing both to be of the same shape, is therefore somewhat deceptive, in that their different orientations are usually not mentioned.

Even the claimed similarity in shape of the ilium in Australopithecus and human beings is not complete in all respects. Zuckerman (1954, p. 345) observed: "When the least breadth of the ilium

is expressed as a percentage of the greatest breadth.... the [australopithecine] fossils are pongid [apelike]."

Figure 11.12. In *Australopithecus* (right) and a modern human (center), the broad iliac blade of the pelvis is of similar shape. Some have taken this as proof that australopithecines walked upright in human fashion. A chimpanzee ilium (left) is shaped differently. After Oxnard (1975a, p. 53), not to scale.

Figure 11.13. When the pelvis is viewed with the hip socket toward the observer, the ilium of *Australopithecus* (left) is oriented like that of the chimpanzee (right) and not like that of the human being (center). This, and other features of the australopithecine pelvis, indicated to Oxnard and Zuckerman that *Australopithecus* was capable of quadrupedal and tree-climbing behavior similar to that of the orangutan. After Oxnard (1975a, p. 55), not to scale.

There are other significant differences. Zuckerman (1954, pp. 344-345) said about the specimens of *Australopithecus* pelvis that he studied: (1) "in their maximum iliac breadth they were smaller than in man, but of the size usual in apes": (2) "the extent of the gluteal [muscle] areas was significantly smaller than in the chimpanzee and man, but of the size found in the gorilla, and ... in the orang": (3) "The size of the auricular surface, the area with which the sacrum [tail bone] articulates, was significantly smaller than in man, but similar to that in apes."

Regarding the size of the auricular surface and that of the iliac tuberosity (the large rounded prominence for the attachment of muscles and ligaments on the upper part of the ilium). Zuckerman (1954, p. 346) stated: "Schultz (1930) has shown that the great relative size of these two areas in man is related to the erect attitude, and to the transmission of the weight of the trunk, head, and upper limbs on the sacroiliac articulations. Their smaller size in the great apes can be related to the more quadrupedal posture and gait of these animals. In view of their equally small size in the fossil specimens, it is difficult not to believe that the *Australopithecines* walked in the same way as do apes." Modern proponents of a more humanlike view of *Australopithecus* consistently and vehemently deny this possibility.

According to Zuckerman, features of the *Aiisfra/opifheci/s* pelvis Identified by some as decidedly human were subject to alternate interpretations. One of these humanlike features. according to Broom, Robinson, and Schepers (1950), was "the presence of a well-developed anterior inferior iliac spine." Zuckerman (1954, pp. 343-344), however, observed: "Such a spine may imply a ligament whose development is normally associated with the maintenance of the erect posture. On the other hand the spine is also well developed in many quadrupedal animals, e.g. the menotyphlous insectivores, and many carnivores and rodents (Straus 1929)."

In a set of drawings, Oxnard showed the hips and lower limbs of a human, an ape, and an australopithecine placed as if all three were quadrupedal. Oxnard (1975a, p. 57) noted: "The similarities of the ape and *Ai/sfra/opifheci/s* are most evident." This could be taken to indicate that *Ai/sfra/opifheci/s* was well adapted for quadrupedal locomotor behavior.

In 1973, Oxnard assisted Zuckerman and other researchers in conducting an extensive multivariate statistical analysis comparing the pelvis of *Ausfra/opifheci/s* with the pelvises of 430 primates representing 41 genera.

The pelvis study considered 4 measurements relating to Joints and 5 relating to muscular attachments. When all 9 features of the pelvis were considered together, *Ai/sfra/opifheci/s* proved to be unique, differing from both human beings and the nonhuman primates.

Zuckerman and Oxnard therefore concluded that it was "conceivable that the habitual posture and gait of *Ausfra/opithecic/s* might have been unique by displaying a combination of quadrupedalism and bipedalism" (Zuckerman et al. 1973, p. 153).

Amplifying this suggestion, Zuckerman and Oxnard further stated: "the locomotor use of the hindlimb might have been composite, involving possibly quadrupedalism, bipedalism, and maybe other types of activity, such as an 'acrobatic' function" (Zuckerman et al. 1973, p. 156). Their comparative studies demonstrated that among sub-human primates "the group approximating most closely to *Ai/sfra/opithecic/s* comprises genera in which the hindlimb sometimes supports, sometimes suspends the animal, and generally operates in many planes of space" (Zuckerman et al. 1973, p. 159). It is difficult to overstate how strongly this contradicts the conventional picture of *Ai/sfra/opifheci/s*, which is never shown hanging from a tree limb by its legs.

### 11.8.3 Zuckerman and Oxnard on Suppression Of Evidence

The paper by Zuckerman and Oxnard on the pelvic study was originally presented at a symposium of the Zoological Society of London in 1973. At the conclusion of the symposium, Zuckerman made some important remarks. He said: "for more than 25 years anatomists and anthropologists—I am talking about physical anthropologists now—have been turning themselves inside out, persuading themselves and others that the obviously simian characteristics of the australopithecine fossils could be reconciled with the model of some assumed protohuman type. Over the years I have been almost alone in challenging the conventional wisdom about the australopithecines—alone, that is to say, in conjunction with my colleagues in the school I built up

in Birmingham—but I fear to little effect. The voice of higher authority had spoken, and its message in due course became incorporated in text books all over the world" (Zuckerman 1973. pp. 450-451).

The situation has not changed since Zuckennan spoke in 1973. The voices of authority in paleoanthropology and the scientific community in general have managed to keep the humanlike view of *Australopithecus* intact. The extensive and well-documented evidence contradicting this favored view remains confined to the pages of professional journals, where it has little or no influence on the public in general, even the educated public.

Zuckerman (1973, p. 451) also stated: "In my view what above all has denied the study of the palaeontology of the higher

Primates the right to be regarded as a serious science is the fact that over the years ex cathedra pronouncements about what constitutes a unique human characteristic in a bone have usually proved nonsense. My belief is that they will always do so."

Zuckerman (1973, p. 451) explained: "It could well be that some feature or group of features in a fossil bone—maybe those having some definable mechanical significance—proves to be more like the corresponding features in man than in the living apes. Almost invariably other features in the same region would be likely to turn out far more ape-like than human. In combination, we end up with something that differs from both men and apes, and which would thus be unique. What conclusion does one then draw, one might well ask. Are we to suppose that the fossils are ancestral to one group, or to the other, or neither? This is the kind of question people try to answer, but we have to recognize that it is at the same time the sort of question which is not amenable to any answer which would be scientifically final."

Oxnard believed that much of the evidence required to find an answer had dropped out of sight. Reviewing the decades-long controversy about the nature of *Australopithecus*, Oxnard (1984, pp. 317-318) said: "In the uproar, at the time, as to whether or not these creatures were near ape or human, the opinion that they were human won the day. This may well have resulted not only in the defeat of the contrary opinion but also in the burying of that part of the evidence upon which the contrary opinion was based. If this is so, it should be possible to unearth this other part of the evidence. This evidence may actually be more compatible with the new view; it may help open the possibility that these particular australopithecines are neither like African apes nor humans, and certainly not intermediate, but something markedly different from either. "

Of course, this is exactly the point we have been making throughout this book. Evidence has been buried. We ourselves have uncovered considerable amounts of such buried evidence relating to the antiquity of the modern human type.

#### 11.8.4 Opposition to Statistical Studies

Some have claimed that the statistical approach employed by Oxnard and Zuckerman is inappropriate and

misleading.

For example Robert Broom said: "I regard all biometricians in the field of morphology as fools" (Johanson and Edey 1981, p. 76). Donald Johanson, discoverer and defender of Lucy, ridiculed Zuckerman, accusing him of "kicking up more and more biometric dust" and firing off "statistical salvos" (Johanson and Edey 1981, p. 76).

Johanson noted: "To give Zuckerman his due, there were resemblances between ape skull and australopithecine skulls. The brains were approximately the same size, both had prognathous (long, jutting) jaws, and so on. What Zuckerman missed was the importance of some traits that australopithecines had in common with men" (Johanson and Edey 1981, p. 76).

In this regard, Johanson cited Charles A. Reed, of the University of Illinois, who said: "No matter that Zuckerman wrote of such characters as being 'often inconspicuous'; the important point was the presence of several such incipient characters in functional combinations. This latter point of view was one which, in my opinion, Zuckerman and his co-workers failed to grasp, even while they stated that they did. Their approach . . . was extremely static in that they essentially demanded that a fossil to be considered by them to show any evidence of evolving toward living humans, must have essentially arrived at the latter status before they would regard it as having begun the evolutionary journey" (Johanson and Edey 1981, p. 76).

In citing Reed against Zuckerman in this way, Johanson was being somewhat hypocritical. Johanson and others sharing his views certainly did not characterize *Australopithecus* as an apelike creature with "incipient" human features. Rather they said *Australopithecus* was practically human from the neck down, especially in terms of humanlike bipedal locomotion. In other words, Johanson and others were themselves guilty of insisting that a distant ancestor of living humans had "essentially arrived at the latter status." Reacting to this exaggerated claim, Zuckerman, and later Oxnard, were just saying it was wrong, and that the anatomy and locomotor behavior of *Australopithecus* were essentially apelike.

Johanson, Reed, and others have also ignored the implications of findings by Oxnard and Zuckerman that *Australopithecus* had anatomical features that were uniquely different from those of apes and modern humans (Section 11.8.5). Contrary to the usual view, *Australopithecus* was not, according to Oxnard and Zuckerman, morphologically intermediate between humans and apes. Thus it is unlikely that *Australopithecus* was a human ancestor, unless one wants to invoke an evolutionary path that took the human line on a big australopithecine detour.

One point that Oxnard made in response to critics of his somewhat complicated mathematical approach was that simple visual evidence also established his conclusion that *Australopithecus* had a significant degree of quadrupedal, acrobatic, and suspensory capability.

For example, Oxnard observed that the articular, or joint, surfaces of the lower limbs of human beings are large relative to the articular surfaces of the upper limbs. Oxnard (1984, p. 316) stated: "This befits their bipedal status in which the lower limb takes all the body weight. "

Simple visual inspection also revealed that In African apes the articular surfaces of the upper and lower limbs are more equal in size. According to Oxnard (1984, p. 316), this indicates a pattern of behavior "in which both limbs participate in bearing the body weight (and the upper limbs somewhat more than the lower. however that may be, whether through quadrupedal knuckle-walking on the ground or through quadrumanal climbing in the trees)." Quadrumanal (fourhanded) climbing involves use of grasping hands and handlike feet by arboreal primates such as the gibbon and orangutan. In fact, in the gibbon and orangutan. which move through the trees mainly by using their anns. the articular surfaces of the upper limbs are larger than those of the lower limbs.

Oxnard (1984, p. 316) noted that as far as Australopithecus Is concerned, "the fossils . . . resemble most. among living primates, the equivalent parts from apes (and among the apes, the orang-utan) more closely than they do humans." Like orangutans, Australopithecus has larger articular surfaces In the upper limbs than the lower (Oxnard 1975a, pp. 117-119). "These facts should be set alongside the comment of Richard Leakey (1973c). who reports that preliminary indications point to a relatively short lower limb and a long upper limb for the australopithecines," said Oxnard (1984. p. 316). Such proportions are decidedly apelike and. along with the proportions of the articular surfaces, suggest a component of orangutanlike forelimb suspension In the locomotor repertoire.

Oxnard did not deny that Australopithecus manifested bipedal behavior. After all, apes can also walk on two legs in some fashion. Nevertheless, Oxnard (1984. p. 316) concluded about the australopithecines: "however able these creatures were at walking on two legs, they were also convincing quadrupeds and perhaps excellent climbers. feats denied to man today.' Oxnard (1984, p. 316) warned: "Such findings must make us wonder whether the australopithecine pattern of bipedal adaptation really reflects a transitional phase to man.' ' In other words. he doubted the common belief that Australopithecus Is a human ancestor.

#### 11.8.5 Implications of Uniqueness

Summarizing his findings, Oxnard (1975b. p. 393) stated: "Between the very early Miocene apes and ancient man is the tantalizing set of fossils known as Australopithecus. . . . most workers feel that the overall position of these fossils is adequately fixed. with a taxonomic label as clearly Hominidae. an evolutionary label as on the line to man or very close to it. and a functional label as a human type of biped. . . . But our current studies are providing very different Ideas. In the multivariate investigations reported here, the various australopithecine fossils are usually quite different from both man and the African apes. . . . Viewed as a genus, they are a mosaic of features unique to themselves and features bearing some resemblance to those of the orang-utan."

Let us consider one example of uniqueness in the australopithecine anatomy—the talus, or ankle bone. The multivariate statistical technique employed by Oxnard involves measuring a fixed number of features on a bone, in this case the talus. The results of such a study can be visually represented, for each bone, as a point in multidimensional space. For example, If one is measuring

three features of a bone, the combination of these features can be displayed as a point in a three-dimensional space. Four features would require a four-dimensional space, and so on. The relationships between bones or sets of bones can thus be examined. Points clustered together represent bones that are morphologically similar. Figure 11.14 shows the morphological relationships of the ankle bones of modern humans, African apes, orangutans, and Australopithecus. As can be seen, the point representing the talus of Australopithecus lies in its own domain, distant from modern humans and African apes, and close to orangutans. Oxnard found the same to be true of other parts of the australopithecine anatomy.

According to modern theory, the African apes, particularly the chimpanzees, are the closest relatives of modern humans. Scientists hypothesize that the hominids split from the ancestors of modern chimpanzees several million years ago. Since, according to this view, modern humans and chimpanzees share a common (though as yet undiscovered) ancestor, then Australopithecus, as a hominid predecessor of modern humans, should be morphologically intermediate between humans and chimpanzees. Oxnard's finding that the morphology of the australopithecines is uniquely different from that of modern humans and chimpanzees calls into question their supposed evolutionary relationship.

Figure 11.14. This display (after Oxnard 1975a, p. 82) depicts the results of a multivariate statistical analysis of the talus (ankle bone) in various hominids and apes. The talus of Australopithecus and that of Homo habilis (considered by Oxnard to be an australopithecine) are morphologically distant from those of modern humans and African apes. Given the view that humans and African apes such as the chimpanzee share a common ancestor, one would expect the australopithecine talus to occupy an intermediate position. Instead, it occupies a unique position, close to that of the orangutan. The same is true of other australopithecine bones. To Oxnard, this raised doubts about the status of Australopithecus as a human ancestor.

That the anatomy of Australopithecus, although unique, resembles that of Pongo (the orangutans) is particularly troubling. Accepting this, evolutionists would have to say that the hominids developed an orangutanlike functional morphology in the australopithecine stage (independently, however, from the orangutans) and then veered back toward the modern human condition. Of course, given the flexibility of evolutionary theorizing, anything is possible. But the view of Australopithecus emerging from the studies of Oxnard and Zuckerman introduces vexing complications.



Considering the anatomical uniqueness of the australopithecines, Oxnard (1975b, p. 394) said: "If these estimates are true, then the possibility that any of the australopithecines is a direct part of human ancestry recedes." Groves(1989,p.307),

After reviewing studies by Oxnard and others, agreed that "the locomotor system of *Australopithecus africanus* was unique— not simply an intermediate stage between us and apes." He found the same to be true of other species of *Australopithecus*. This fact, along with other aspects of the hominid fossil record, caused him to suggest that "non- Darwinian" principles were required to explain an evolutionary progression from *Australopithecus* to modern human beings (Groves 1989, p. 316).

#### 11.8.6 Oxnard on the Antiquity of Homo

Like Louis and Richard Leakey, Oxnard believed that the *Homo* line was far more ancient than the standard evolutionary scenario allows. In this connection, Oxnard called attention to some of the fossils we have previously discussed, such as the humanlike ER 813 talus, over 1.5 million years old (Section 11.6.4). "Description and examination using multivariate methods [Wood 1974a] confirms that it is indeed similar to modern man and unlike the australopithecine specimens," said Oxnard (1975b, p. 394). He also mentioned the Kanapoi humerus, perhaps

4 or more million years old. Citing research by B. Patterson and W. W. Howells (1967), Oxnard (1975b, p. 394) said the Kanapoi humerus had been "shown to be very similar to that of modern man." His own research backed up that judgement (Oxnard 1975a). From such evidence, Oxnard (1984, p. 332) concluded that the genus *Homo* was 5 or more million years old.

Oxnard (1975b, p. 395) predicted that "more evidence of earlier forms that are more like man than australopithecines <sup>^11</sup> be found. " He held that such "fossil remnants will be discovered outside Africa because . . . human or prehuman populations must have existed in other places, with migrations, and with multiple evolutionary lines." But as we have seen in Part I, much evidence for completely humanlike forms existing at very early times has already been found, in Europe and the Americas as well as Africa. Such evidence is so extensive that talk of evolutionary lines, either single or multiple, becomes problematic.

All one can say with certainty is that various humanlike and apelike creatures seem to have coexisted for millions of years into the past. Oxnard (1975b, p. 395) approached this interpretation when he suggested: "We may have to accept that the australopithecine form (or forms) of locomotion, tool using, and tool making may be merely one (or more) unsuccessful evolutionary experiments existing in parallel with those of man." Here the mention of tool using and making refers to *Homo habilis*, which Oxnard regarded as an australopithecine.

Elsewhere, Oxnard (1984, p. 1) gave this succinct statement of his principal conviction: "the conventional notion of human evolution must now be heavily modified or even rejected . . . new concepts must be explored."

#### 11.9 Lucy in the Sand with Diatribes

Donald Johanson studied anthropology at the University of Chicago, under F. Clark Howell. As a young graduate student, eager to learn the romantic business of hominid fossil hunting, Johanson accompanied Howell to Africa, working at the Omo site in Ethiopia.

After two seasons work at Omo, Johanson found himself in Paris. There he met Maurice Taieb, a French geologist, who told him about Hadar, a promising Plio-Pleistocene site in the Afar desert, in northeastern Ethiopia. In 1972, Johanson surveyed the region with Taieb, and after returning to the United States received a National Science Foundation grant to explore it more thoroughly. Johanson hoped to find hominid fossils.

In 1973, Johanson returned to Africa, but before going to Hadar he attended a conference of paleoanthropologists in Nairobi. There he met Richard Leakey, who had captured everyone's attention with skull ER 1470 (Section 11.6.1), said to be 2.9 million years old. Leakey, by then quite famous, asked Johanson, still an unknown, what he was up to. Johanson replied that he would soon be hunting for hominids at Hadar in northern Ethiopia. "Do you really expect to find hominids there?" asked Leakey. Johanson replied yes, adding "older than yours." He bet Leakey a bottle of wine he would do it. "Done!" said Leakey (Johanson and Edey 1981, pp. 134-135). Right from the start, it seems, Johanson was motivated by glamor. Finding hominids is special. It gets one headlines, interviews, and foundation grants, as well as recognition from one's colleagues.

#### 11.9.1 The Hadar Knee (AL 129)

By the end of his first season at Hadar, Johanson was in trouble. His National Science Foundation grant money, which was supposed to have lasted two years, was almost gone. Johanson worried he would be labeled incompetent. Furthermore, he had not found any of those glamorous hominid fossils. Johanson noted: "I had not exactly promised hominids when I put in my request for funds from the National Science Foundation, but I knew when I wrote up my grant proposal that if I did not include a strong pitch for hominids I would get no money at all; the likelihood of being sent to Ethiopia to collect pig's teeth was remote" (Johanson and Edey 1981, p. 154).

Despite his financial problems, Johanson continued scouting for fossils. One afternoon, he found the upper portion of a tibia, a long bone between the knee and the ankle. The bone was obviously from some kind of primate. Nearby, Johanson found a distal femur, the lower end of a thighbone. From the way the femur and tibia fit together, Johanson believed he had found the complete kneejoint not of some ancient monkey but of a hominid, an ancestor of modern humans. The deposits yielding the fossils were over 3 million years old, making this one of the oldest hominid finds ever made (Johanson and Edey 1981, p. 155).

Johanson felt that "his whole reason for being there, the core of his own most secret motivation" had been justified (Johanson and Edey 1981, p. 159).

In scientific publications that followed, Johanson reported that the Hadar knee (AL 129) was 4 million years old and belonged to a primitive australopithecine with a fully human bipedal gait (Johanson and Coppens 1976).

In support of his contention that AL 129 was characteristically human in structure, Johanson cited the presence of a valgus knee. A valgus knee is one in which the femur slants outward from the knee to the hip, at an angle from the lower part of the leg. Humans have a valgus knee. In African apes, the femur rises straight from the knee to the hip, in line with the lower part of the leg.

At 15 degrees from vertical, the angle of valgus in AL 129 was, however, much higher than the adult human mean of 9 degrees (Stern and Susman 1983, p. 296). This suggests that the locomotor behavior of AL 129, even if terrestrial and bipedal, might have been quite different from that of adult humans. In human children 3 to 4 years of age, the degree of valgus is as great as that in the AL 129 femur (Stern and Susman 1983, p. 296). The high angle is reflected in a child's knock-kneed stance and somewhat awkward gait. The creature with the AL 129 knee may have stood and walked in similar fashion.

Furthermore, Jack T. Stern and Randall L. Susman of the State University of New York at Stony Brook noted that the presence of a valgus knee is not exclusively associated with terrestrial bipeds. Orangutans and spider monkeys, both of which spend most of their time in trees, have valgus knees (Stern and Susman 1983, p. 298).

As we have seen (Section 11.8.5), C. E. Oxnard and others have found that the functional morphology of the australopithecines has orangutan affinities. The valgus knee in AL 129 could thus represent yet another orangutanlike feature in *Australopithecus*. The totality of orangutan resemblances suggests arboreal behavior in *Australopithecus*, which Oxnard, Zuckerman, and the Leakeys did not consider ancestral to modern humans.

In his account of the discovery of AL 129, Johanson did not mention that primates other than humans have a valgus knee. It seems there are two possible explanations why he did not. Either he was unaware that orangutans and spider monkeys have the same outward slanting femur as humans, or he was aware of this but deliberately neglected to mention it because it would have complicated the case he was trying to make.

According to Brigitte Tardieu (1979), key features of the AL 129 femur and tibia, other than the degree of valgus, fell outside the modern human range. "These traits . . . led her to conclude that despite clear adaptations to terrestrial bipedality in the small Hadar hominid, the precise mechanism of this bipedality could not be specified and that it must have occurred along with some degree of arboreal behavior," said Stern and Susman (1983, p. 298).

Stern and Susman (1983, pp. 298-299) themselves concluded: "Since, aside from the degree of valgus, the knee of the small Hadar hominid possesses no modern trait to a pronounced degree, and since many of these traits may not serve to specify the precise nature of the bipedality that was practiced, we must agree with Tardieu that the overall structure of the knee is compatible with a significant degree of arboreal locomotion."

It is intriguing that the views of scientists like Tardieu, Stem, and Susman, though appearing in scientific journals, are rarely encountered in popular presentations or general textbooks. This points to the existence of a pattern of knowledge filtration in the scientific community that tends, consciously or unconsciously, to suppress information that would complicate the relatively simple picture of human evolution presented to the public in general and to students at all levels of the educational system, except, perhaps, graduate students working directly in the field of physical anthropology.

Be that as it may, Johanson's lucky find saved the day for him, sparing him the embarrassment of leaving Ethiopia fundless and fossilless. Johanson said of the Hadar knee find: "It had brought me up a step; in my dealings with other scientists I was standing taller. I now had a unique hominid fossil of my own" (Johanson and Edey 1981, p. 165).

The glamor factor won Johanson 25,000 dollars from supporters in Cleveland, where Johanson held a post at the Museum of Natural History. The new funds allowed Johanson to return for a second year of work at Hadar.

#### 11.9.2 Alemayehu's jaws

Alemayehu Asfaw was an employee of the Ethiopian Ministry of Culture, and by the terms of Johanson's agreement with the Ethiopian government, he was working at the Hadar site. In October of 1974, Alemayehu found a fossil jaw that he thought belonged to a baboon, but Johanson said it was hominid. Other similar jaws soon turned up. Classifying them proved difficult. Johanson asked Richard Leakey to come and have a look at them. Leakey took up the invitation, and arrived accompanied by his mother Mary Leakey and wife Meave. Together with Johanson they examined the jaws and judged them to be Homo, making them the oldest Homo fossils yet found (Johanson and Edey 1981, pp. 172-176).

#### 11.9.3 Lucy

On November 30, 1974, Donald Johanson and Tom Gray were searching Locality 162 at the Hadar site, collecting bits of mammalian bone. After some time, Gray was ready to call it quits and go back to the camp. Johanson, however, suggested they check out a nearby gully. Other members of the expedition had already thoroughly searched it, but Johanson, who had been feeling "lucky" all day, decided to have one more look. Gray and Johanson did not find much. But as they were about to leave, Johanson spotted a piece of arm bone lying exposed on the surface. He thought it was hominid. Gray disagreed, saying it was probably from a monkey. Then Gray found a piece of skull and a part of a femur. They seemed definitely hominid. As they looked around, they could see scattered on the surface other bones—apparently from the same hominid individual. Johanson and Gray started jumping and howling in the 110-degree heat, celebrating what was obviously an extremely significant find. Finally they calmed down, realizing their boots were probably smashing some of the precious bones. After collecting a few hominid fossils, they headed back to camp. That evening Johanson and his coworkers partied while a Beatles song, "Lucy in the Sky with

Diamonds,” blared repeatedly from the camp sound system. From the lyrics of that song, the female hominid received her name, Lucy Oohanson and Edey 1981, pp. 16-18).

By a combination of potassium-argon, fission track, and paleomagnetic dating methods, Johanson determined that Lucy was 3.5 million years old Oohanson and Edey 1981, pp. 200-203).

#### 11.9.4 The First Family

In 1975, Johanson was back at Hadar, this time with a National Geographic photographer, who recorded another important discovery. On the side of a hill, Johanson and his team found the fossil remains of 13 hominids, including males, females, and children. The group was called the First Family. They were the same geological age as Lucy, about 3.5 million years old.

Stone tools were also found at the First Family site. They were made of basalt, and Johanson said they were "of somewhat better workmanship" than tools from the lower levels of Olduvai Gorge Oohanson and Edey 1981, p. 231).

How old were the tools? The fact that they were found on the surface made dating them somewhat difficult. In his book *Lucy*, Johanson reported the views of John Harris, a tool expert, who had worked at Lake Turkana: "He said that It was really impossible to date a surface-found tool at Lake Turkana because modern humans who needed rough blades to chop animals were making similar implements in profusion as recently as a thousand years ago, and that there were even a few people who were making them there today" Oohanson and Edey

1981, pp. 229-230). In Harris's opinion, the surface-found tools at Hadar could also have been recent.

To remove their doubts about the age of the stone tools found on the surface at the First Family site, Harris and Johanson conducted some excavations and were rewarded by discovering a number of tools in situ. They Judged the level at which they were found to be 2.5 million years old Oohanson and Edey along with these tools. Because *Australopithecus* was not known to have manufactured stone tools, Johanson speculated that *Homo habilis* was the toolmaker. But the oldest *Homo habilis* fossils were only about 2 million years old. Johanson simply proposed that *habilis* remains of the same age as the tools would eventually be found. As we have seen, there are, however, fossil remains resembling the modern human type from Early Pleistocene and Pliocene contexts in Africa (Sections 11.1, 11.2, 11.5, 11.6.3, and 11.6.4) and elsewhere (Section 6.2). It is thus possible that anatomically modern

humans could have made the Hadar tools.

With the First Family, the major discoveries at Hadar, which also included the Hadar knee, Alemayehu's Jaws, and Lucy, were completed. We shall now examine how these fossils were interpreted and reinterpreted by various parties.

#### 11.9.5 Two Hominids at Hadar?

In classifying his finds, Johanson initially relied heavily upon the judgement of Richard and Mary Leakey that the Alemayehu jaws and First Family specimens were *Homo* (Johanson and Edey 1981, p. 217). If Lucy and the AL 129 femur and tibia were australopithecine, as Johanson believed, then there were two kinds of hominids at Hadar.

In a December 1976 National Geographic article, Johanson made a clear distinction between the First Family, which he thought represented *Homo*, and Lucy, which he thought represented an early *Australopithecus* (Fix 1984, p. 70). This two-species view was reflected in a number of scientific papers published by Johanson and various coauthors.

Richard Leakey later said that Lucy, with her V-shaped Jaw and other primitive features represented "a late *Ramapithecus*" (Johanson and Edey 1981, p. 279). *Ramapithecus*, as previously noted (Section 3.9), was an extremely primitive apelike creature living in the Miocene and Pliocene. It may be recalled that *Ramapithecus*, originally considered the root of the hominid line, was later reclassified as nonhominid and ancestral to the orangutans.

Given the orangutan affinities of *Australopithecus*, as detailed by Oxnard (Section 11.8.5), maybe Leakey's idea that Lucy was a ramapithecine was right.

#### 11.9.6 Johanson and White Decide On a

##### Single Hadar Species

Johanson was later influenced to change his mind about the number of species at Hadar. The person who convinced him to do so was Timothy D. White, a paleontologist who had worked at Lake Turkana with Richard Leakey. White, on faunal grounds, disputed Leakey's dating of the KBS tuff (Section 11.6.5.2). Eventually, he left Lake Turkana and for a time worked at Laetoli, Kenya, where Mary Leakey had found hominid jaws similar to those at Hadar.

Johanson and White first met briefly in Africa. In the summer of 1977, when Johanson was back at the Cleveland Museum of Natural History studying his Hadar fossils, he asked White to bring samples of the Laetoli fossils.

White came and convinced Johanson to accept the following points: (1) the U-shaped Jaws discovered at Hadar by Johanson and those discovered at Laetoli by Mary Leakey were of the same species; (2) the species was not *Homo*, as Johanson and the Leakeys had originally thought, but a new kind of australopithecine; (3) the V-shaped Jaw of Lucy was also of the same species, being a female sexual variant of the other U-shaped jaws. Referring to a scientific paper in which he had advocated the two-species concept, Johanson said: "I would withdraw that paper today if I could" (Johanson and Edey 1981, p. 209).

Johanson and White (1979) soon announced their new species, calling it *Australopithecus afarensis*, after the Afar region of Ethiopia where most of the specimens were found.

According to Johanson and White, *Australopithecus afarensis* gave rise to two lineages. The first led by way of *Australopithecus africanus* to the robust australopithecines. The second lineage led by way of *Homo habilis* to *Homo erectus* and thence to *Homo sapiens*. In constructing this phylogenetic hypothesis, Johanson relied primarily upon dental evidence. The molars of *A. afarensis* were the smallest of all the australopithecines. The molars of *A. africanus* were larger, and those of the robust australopithecines larger still. This, to Johanson, indicated an evolutionary development. In Lucy's *Homo* offspring, the molars grew progressively smaller, representing a separate, parallel line of evolutionary development. It all seemed to fit together quite nicely.

#### 11.9.7 A. Afarensis: Overly Humanized?

Johanson said that *Australopithecus afarensis* individuals had “smallish, essentially human bodies” (Johanson and Edey 1981, p. 275). But several scientists have strongly disagreed with Johanson's picture of *Australopithecus afarensis*. These dissenters have painted a far more apelike portrait of Lucy and her relatives. In most cases, their views on Lucy parallel the earlier work of Oxnard, Zuckerman, and others on *Australopithecus*. If the dissenting view is correct, as it appears to be, then Johanson's description of *Australopithecus afarensis* can only be considered as misleading.

It seems that Johanson imposed a humanlike interpretation upon Lucy's essentially apelike anatomy for the propaganda purpose of enhancing her evolutionary status as a human ancestor. Johanson himself said: “There is no such thing as a total lack of bias. I have it; everybody has it. The fossil hunter in the field has it. . . . In everybody who is looking for hominids there is a strong urge to learn more about where the human line started. If you are working back at around three million, as I was, that is very seductive, because you begin to get an idea that that is where *Homo* did start. You begin straining your eyes to find *Homo* traits in fossils of that age” (Johanson and Edey 1981, p. 257). Johanson gave this confession to explain why he originally characterized the First Family fossils and the Alemayehu Jaws as *Homo*, but it also applies to his insistence on seeing in Lucy traits of a creature well on the way to becoming human.

The Hadar fossils did not include a complete skull of an *A. afarensis* individual, but Tim White managed to pull together a partial reconstruction, using cranial fragments, pieces of upper and lower jaw, and some facial bones from several First Family individuals. According to Johanson, the reconstructed skull “looked very much like a small female gorilla” (Johanson and Edey 1981, p. 351). The forehead was low, the large jaw projected far beyond the upper part of the face, and there was no chin. The general apelike appearance was also reflected in anatomical details such as the mandibular fossa (the place where the lower jaw attaches to the skull), the tympanic plate, and the mastoid process. All of these were apelike, not humanlike (Johanson and Edey 1981, pp. 272-273). Furthermore, the cranial capacity of *A. afarensis* (380- 450 cc) overlapped that of chimpanzees (330-400 cc) and other apes. Here there was no dispute between Johanson and his critics. Both agreed that the *afarensis* head was apelike.

Johanson and White believed the skull was, however, different from that of previously known australopithecines. But W. W. Ferguson (1984) and P. Schmid (1983) pointed out that White's reconstruction of the *Australopithecus afarensis* skull was incorrect. Correcting the mistake

"makes the resulting construction a great deal more like *A. africanus*," said Groves (1989, p. 263). P. V. Tobias (1980) said all the Hadar and Laetoli fossils were not a new species but were just subspecies of *Australopithecus africanus*. According to Tobias, *Australopithecus africanus* was the ancestor of *Homo*, while for Johanson and White *Australopithecus africanus* was the ancestor of only the robust australopithecines.

Originally, Johanson thought the *A. afarensis* U-shaped jaws were humanlike and like the Leakeys assigned them to the genus *Homo*. Later Johanson said they were "distinct from apes and from any of the later hominids" (Johanson and Edey 1981, p. 271). But his detailed descriptions showed the Hadarjaws to be in fact quite apelike.

In humans, the teeth in the jaw are arrayed in a parabolic curve. In the Hadarjaws, such as AL 200, the teeth on either side of the jaw are set in straight, parallel rows, as in the apes, although the rearmost molars are sometimes slightly displaced (Johanson and Edey 1981, pp. 267-268). Both in apes and the Hadar fossils the palate is flat (Johanson and Edey 1981, p. 270). In humans it is arched.

TABLE 11.6

#### Evidence for Arboreality in Postcranial Anatomy of *A. Afarensis*

1. General anatomy of Lucy's shoulder blade was characterized as "virtually identical to that of a great ape and had a probability of less than 0.001 of coming from the population represented by our modern human sample" (Susman et al. 1984, pp. 120-121).
2. Lucy's shoulder blade has a shoulder joint which points upward (Oxnard 1984, p. 334-1: Stern and Susman 1983, p. 284). This would allow "use of the upper limb in elevated positions as would be common during climbing behavior" (Stern and Susman 1983, p. 284).
3. *A. afarensis* wrist bones are apelike. "Thus we may conclude that *A. afarensis* possessed large and mechanically advantageous wrist flexors, as might be useful in an arboreal setting" (Stern and Susman 1983, p. 282).
4. *A. afarensis* metacarpals (the bones in the palm region of the hand) "have large heads and bases relative to their parallel-sided and somewhat curved shafts—an overall pattern shared by chimpanzees." This "might be interpreted as evidence of developed grasping capabilities to be used in suspensory behavior" (Stern and Susman 1983, pp. 282, 283).



5. The finger bones are even more curved than in chimpanzees and are morphologically chimpanzeelike (Stern and Susman 1983, pp. 282-284; Susman et al. 1984, p. 117; Marzke 1983, p. 198).

6. *A. afarensis* humerus (upper arm bone) has features that are 'most likely related to some form of arboreal locomotion' (Oxnard 1984, p. 334-1; see also Senut 1981, p. 282).

7. One of the long bones in the forearm, the ulna, resembles that of the pygmy chimpanzee (Feldesman 1982b, p. 187).

8. Vertebrae show points of attachment for shoulder and back muscles "massive relative to their size in modern humans" (Cook et al. 1983, p. 86). These would be very useful for arboreal activity (Oxnard 1984, p. 334-1).

9. “Recently Schmid (1983) has reconstructed the A.L. 288-1 rib cage as being chimpanzee-like” (Susman et al. 1984, p. 131).

10. Blades of hip oriented as in chimpanzee (Stern and Susman 1983, p. 292).  
Features of afarensis hip therefore "enable proficient climbing" (Stern and Susman 1983, p. 290).

11. The thighbone of Lucy “probably comes from an individual with the ability to abduct the hip in the manner of pongids,” allowing for ‘movement in the trees’ (Stern and Susman 1983, p. 296).

12. Knee Joint is loose, as in gibbon.  
"The mobility and prehensility of the foot are greatly  
complemented" (Tardieu 1981, p. 76), making it good for climbing.

13. Lucy had valgus knee, as do humans. But “the orang-utan and the spider monkey . . . are extremely able

arborealists that have similar valgus angles as humans" (Oxnard 1984, p. 334-ii; see also Prost 1980).

14. Lucy had "a relatively short hindlimb . . . comparable to that seen in apes of similar body size." This "would clearly facilitate climbing" (Susman et al. 1984, pp. 115, 116).

15. Feet have long, curved toes and a mobile ankle Joint, making them well suited for grasping limbs and climbing In trees (Susman et al.. 1984, p. 125). Also, the big toe is divergent, as in the apes (Susman et al. 1984, pp. 137-138).

As in the apes, the canines of the Hadar Jaws were conical. In humans, the inner surface of the canine is flattened. In order to accommodate the projecting lower canine of *A. afarensis*, the

upper jaw has a noticeable gap between the incisor and the canine. Other australopithecines also have the same gap. This gap, called a diastema, is also present in apes but not in humans.

Departures from the ape condition were minor. In an ape, the first premolar has a single cusp. In humans, the first premolar has a prominent second cusp. In all of the Hadar specimens except Lucy, the first premolar has a slightly developed second cusp (Johanson and Edey 1981, p. 270).

All in all, the apelike condition of the Hadar Jaws is so pronounced that even Johanson admitted: "If David Pilbeam were to find any of them in Miocene deposits without any associated long bones, he would surely say it was an ape" (Johanson and Edey 1981, p. 376).

#### 11.9.7.2 Postcranial Anatomy

Now we move on to the postcranial anatomy of *A. afarensis*, particularly Lucy. Several workers have found *A. afarensis* to be rather apelike, thus challenging Johanson's view that Lucy was terrestrially bipedal in human fashion. Table

11.6 summarizes the evidence for arboreality in the postcranial anatomy of *A. afarensis*, and we shall amplify some of the points in this section.

Oxnard (1968) called attention to features of the Sterkfontein scapula suggesting that australopithecines probably engaged in holding the arms over the head in hanging behavior (Section 11.8.1, Table 11.5). *A. afarensis* has the same kind of scapula. Stern and Susman (1983, p. 284) concluded that the shoulder joint of *A. afarensis* was "directed far more cranially than is typical of modern humans and that this trait was an adaptation to use of the upper limb in elevated positions as would be common during climbing behavior."

Johanson (1976, p. 808) had said that the Hadar hands bore "an uncanny resemblance to our own—in size, shape, and function." But this appears to be incorrect.

Stern and Susman (1983, p. 284) concluded: "A summary of the morphologic and functional affinities of the Hadar hand fossils leads inexorably to an image of a suspensory adapted hand, surprisingly similar to hands found in the small end of the pygmy chimpanzee-common chimpanzee range." M. W. Marzke (1983, p. 198), sharing this view, stated that the curved bones of the *A. afarensis* hand "recall the bony apparatus which accommodates the well developed flexor musculature in living apes and positions it for efficient hooklike grip of the branches by the flexed fingers during arboreal climbing and feeding."

So thus far we have in *A. afarensis* a gorilla-like head, an upward-pointing shoulder joint indicating that the arm was used for suspensory behavior, and a hand with a powerful wrist and curved fingers, suitable for climbing. One can just imagine the effects of a painting or model of Lucy engaged in suspensory or other arboreal behavior. This would surely detract from her image as a creature well on the way to human status. Even if one believes Lucy could have evolved into a human being, one still has to admit that her anatomical features appear to have been misrepresented for propaganda purposes.

The distal humerus, the elbow region of the upper arm bone, fits the apelike pattern already established. Brigitte Senut, a physical anthropologist at the French Museum of Natural History, conducted a study of the outlines of cross sections of the distal humerus in living primates, including human beings, and fossil hominids. Senut (1981a, p. 282) discovered that the distal humerus of Lucy (AL 288-1M) was "pongid-like." Pongids are the anthropoid apes, such as chimpanzees, gorillas, and orangutans. Senut (1981a, p. 282) concluded: "The scheme in the Afar specimen [Lucy] would suggest... Its apelike pattern might be a result of a kind of suspension."

Senut (1981a, p. 282) went on to say: "From our point of view, we would say that this specimen may be too pongid-like (i.e. specialized) to be in our ancestry." From the standpoint of mainstream paleoanthropological thought, this is an extremely heretical view.

Feldesman (1982a, p. 91) found Lucy's humerus to be most closely related to the pygmy chimpanzee, *Pan paniscus*.

As far as the bones of the lower arm are concerned, Feldesman (1982b, p. 187) found that "'Lucy' (AL 288) clearly resembles *Pan paniscus* in proximal ulnar morphology." The ulna is the innermost of the two bones making up the forearm (the radius is the other). The proximal, or upper, part of the ulna joins the humerus at the elbow.

In 1985, Della Collins Cook, an anthropologist, and three coauthors, among them Donald Johanson, published a study of the vertebral column of *Australopithecus afarensis*.

Cook and Johanson claimed: "The AL-288 vertebrae correspond to those of modern humans in remarkable detail" (Cook et al. 1983, p. 84). They noted, however, that the "Hadar vertebrae depart from the morphological pattern found in modern humans in a few details that may have functional significance" (Cook et al. 1983, p. 86). These "few details" were not trivial. For example, according to Cook and Johanson, the spinous processes of the *A. afarensis* neck and upper back vertebrae were quite long. The spinous process, a bony projection on the back side of the vertebrae, serves as a point of attachment for muscles. According to Cook and Johanson, the length and surface features of the spinous processes indicated that in *A. afarensis* the back and shoulder muscles were "massive relative to their size in modern humans" (Cook et al.

1983, p. 86).

Oxnard (1984, p. 334-1) stated that the features of the *A. afarensis* vertebrae reported by Cook and Johanson "are likely to have provided the stress bearing structures necessary to support the actions of very powerful shoulder muscles in climbing and arboreal activities suggested by our prior studies of the scapula and clavicle of other australopithecines."

C. Owen Lovejoy, a supporter of Johanson, claimed that the *A. afarensis* hip was suitable only for upright walking (Johanson and Edey 1981, pp. 347-348). But the *A. afarensis* hip structure is significantly different from that in human beings. In particular, Lucy's iliac blade, like that of other australopithecines, is positioned as in apes (Section 11.8.2, Figure 11.13). Susman said: "Therefore,

we are of the opinion that the orientation of the iliac blades in the Hadar species is well-suited for a part-time climber" (Susman et al. 1984, p. 132).

In *Scientific American*, Lovejoy (1988) reasserted his familiar claims that Lucy's pelvic structure and musculature were very similar to those of humans. We will not here repeat the detailed demonstrations by Zuckerman, Oxnard, Stem, Susman, and others that the pelvic morphology of *A. a/arensis* had quite a bit in common with arboreal primates, and was better suited for climbing than walking (Section 11.8.2).

What is perhaps most significant about Lovejoy's presentation is that he does not once directly mention his opponents and their arguments. This adds to our suspicions that the views of Zuckerman, Oxnard, Stem, Susman, Prost, and others are being suppressed for propaganda purposes on the level of secondary presentations for the wider scientific community, educational institutions, and the public in general. The views of the advocates of arboreality for *A. afarensis* are represented almost solely in the primary level of publication, in the obscure pages of scientific journals intended for specialists. They are, however, not at all well represented in publications like *Scientific American*, college textbooks on anthropology, and popular books and television programs dealing with the topic of human evolution. Arboreal habits would not look well in the hominid advertised as the oldest known creature directly ancestral to modern humanity.

Femurs from Lucy and the First Family group challenge claims by Johanson and Lovejoy that the lower limb of *A. a/arensis* was distinctly human in morphology and function. Stern and Susman (1983, p. 296) concluded that the proximal (upper) part of Lucy's femur "probably comes from an individual with the ability to abduct the hip in the manner of pongids," allowing for "movement in the trees."

Measurements of several features of the lower (distal) end of the AL 333-4 First Family femur showed it to be outside the human range and within the ranges of chimpanzees, gibbons, and several species of monkeys. In fact "the distal end of the AL 333-4 femur actually appears less human-like than that of a woolly monkey" (Stem and Susman 1983, p. 297).

Christine Tardieu, an anthropologist at the Museum of Natural History in Paris, gave a slightly different assessment of the AL 333-4 distal femur, finding it barely within the modern human range, at "the extreme end closest to the apes" (Stem and Susman 1983, p. 299). Thus, as often happens, we find ourselves confronted with contradictory interpretations of the same fossil material, but on the whole, the femurs in question appear to be apelike.

Tardieu, in addition to measuring the AL 333-4 femur of the First Family group, also conducted studies of the distal femur of Lucy. She gave special attention to the notch in the femur that holds the upper end of the tibia, the larger of the two bones of the lower leg. In humans, the spine of the tibia fits tightly into the notch of the femur. In apes, the fit is looser. In this regard, Lucy is in the range of the gibbon. Tardieu (1981, p. 76) stated: "The loose fit of the articular surfaces . . . and the consequent laxity of the knee joint signify that the leg and the foot can be placed on the

substrate in a much freer fashion than in Man." This would be good for climbing, but unsatisfactory for extensive walking on the ground.

Commenting on Tardieu's study of Lucy's knee, Oxnard (1984, p. 334-ii) said she was led to "conclude that...its locking mechanism was not developed. implying that full extension of the leg in walking, a key point of human bipedality, was lacking." Such features "suggested to Tardieu that 'Lucy' spent a considerable period of time climbing in the trees" (Oxnard 1984, p. 334-11).

One can just imagine Lucy, hanging lazily from a tree limb by one of her arms, bending a small, dangling foot back from the ankle, while rotating her lower leg from the knee to bring the backward reaching foot in contact with a nearby limb.

The knee of Lucy (AL 288). like the original Hadar knee complex (AL 129), had a significant degree of valgus. Johanson, Lovejoy, and others held this to be an indication of humanlike posture and terrestrial bipedal gait. But, as we have seen, the orangutan and spider monkey have similar valgus angles. and they are arboreal.

In our anatomical survey, we have now progressed to the controversial feet of *A. afarensis*. Even Johanson had a difficult time disguising the manifestly apelike condition of Lucy's foot. He wrote: "The *afarensis* phalanges are arched. and proportionally a good deal longer than those in modern feet. They might almost be mistaken for finger bones" (Johanson and Edey 1981, p. 345). Johanson also noted that the *A. afarensis* foot had "very large muscles whose presence is betrayed by markings along the sides of the phalanges" (Johanson and Edey 1981, p. 345). Such muscles would have been useful in hindlimb grasping.

It is amazing that Johanson could so candidly acknowledge the 'apelike' morphology of the *afarensis* foot and yet refuse to draw the obvious conclusion that it was used in arboreal behavior. Instead, Johanson stated: "Although similarly curved phalanges and muscle markings are found in the chimpanzee — reflecting the chimp's ability to climb trees—Latimer warns that this does not mean that *afarensis* was a tree climber too" (Johanson and Edey 1981, p. 346). Bruce Latimer was one of Johanson's graduate students and worked with him quite closely in Ethiopia on the Hadar finds, so his impartiality is suspect. He was later employed by Johanson to help with the reconstruction of *A. afarensis*. It is not unexpected that Latimer would agree with his professor, mentor, and employer that *afarensis* was a fully terrestrial biped. But researchers operating from more detached and independent standpoints have reached totally different conclusions, which seem to be more in harmony with the evidence.

In studying the most complete *A. afarensis* foot, AL 333-115 from the First Family group, Stem and Susman (1983, p. 306) found that the proximal phalanges (the bones at the base of each toe) had a "strikingly pongid morphology." This was true in terms of both their length and curvature.

Susman, reporting the conclusions of an investigation into the curvature of proximal phalanges in a variety of apes, stated that the chimpanzee and bonobo, or pygmy chimpanzee, had "the most curved toe bones of any ape plotted" (Susman et al.

1984, p. 125). And the proximal phalanges of AL 333-115 were "more curved than in the average bonobo" (Susman et al.

1984, p. 125). In other words, *A. afarensis* was apparently more apelike. In this respect, than any of the living apes. Human proximal phalanges are nearly straight.

Like the proximal phalanges, the other toes bones of *A. afarensis* also displayed apelike features. Altogether, the long, curved toes of *A. afarensis*, accompanied by powerful grasping muscles, would have been well suited for arboreal behavior.

Susman concluded: "at the very least the small individuals should have been able to grab with their toes as well as 2-year old children grab with their fingers. The large Hadar individuals probably could use their toes for simple grasping as effectively as considerably older human children use their fingers. . . . the strength of the grip may have well exceeded the strength of hand grip in young humans" (Susman et al. 1984, p. 124). Lending support to this conclusion, the *A. afarensis* fibula (the smaller of the two bones of the lower leg) was quite robust, indicating the presence of powerful muscles for flexing the foot (Susman et al. 1984, p. 124).

According to Johanson, Latimer, who was opposed to arboreality, concluded that "*A. afarensis* was an exceptionally strong walker, and that its elongated toes may have been of service to it when moving over rough stony ground, or in mud, where some slight gripping ability would have been useful" (Johanson and Edey 1981, pp. 345-346).

Stern and Susman (1983, p. 308) found this notion "untenable: observing that "curved toes are found only in species that engage in arboreal behavior."

Stern and Susman (1983, p. 308) further stated: "There is no evidence that any extant primate has long, curved, heavily muscled hands and feet for any purpose other than to meet the demands of full or part-time arboreal life. "

Another apelike feature of the *A. afarensis* foot can be found in the hallux, or big toe. Studies by Susman showed that the *A. afarensis* hallux could be extended sideways, like the human thumb (Susman et al. 1984, pp. 137-138).

The hallux of *A. afarensis* was relatively smaller than that of some arboreal primates, causing Latimer to suggest that *A. afarensis* was not well suited for climbing trees. But Susman pointed out that the highly arboreal gibbon also has a small hallux (Susman et al. 1984, p. 137). Altogether, the picture that emerges of the *A. afarensis* foot is extremely apelike—a foot with long, curved, fingerlike toes and a highly mobile, thumblike big toe.

Tim White, one of the promoters of *A. afarensis*, has responded negatively to attempts to characterize Lucy as fully, or even partially, arboreal. White stated: "We are wary of this approach which makes the interpretive leap from curved phalanges into the trees" (White and Suwa 1987, p. 512). As we have seen, wariness is always required in approaching empirical treatments of human origins and antiquity. But we should perhaps be more wary of the interpretive leap from

curved phalanges of the toes, since greatly curved phalanges in extant primates are an exclusively arboreal adaptation. This is especially true of curved phalanges existing in combination with an upward pointing shoulder joint and other signs of arboreal capability.

From the toes, let us now move on to the *A. afarensis* ankle, including its articulations with the tibia and fibula, the bones of the lower leg.

Regarding the articular surfaces of the fibula of *A. afarensis*, Stern and Susman (1983, p. 305) wrote that they provide "evidence for a significant component of arboreality in the behavior of *A. afarensis*." Johanson's supporters such as Latimer disagreed with this analysis (Latimer et al. 1987). In overall appearance, however, the lower part of Lucy's fibula, the part that connects with the ankle, is different from that of a human being and almost identical to that of the pygmy chimpanzee (Susman et al. 1984, p. 130).

Stern and Susman (1983, p. 302) argued that Lucy's foot could be bent back further than in humans. "This trait would seem to be useful in reaching for branches with the feet and in hindlimb suspension," they noted (Stern and Susman 1983, p. 299). According to Stern and Susman (1983, p. 300), Lucy's ankle was structured so that she would have "had difficulty in maintaining a vertical orientation of the tibia and might have progressed bipedally in a manner unlike that of humans and more like that of an African ape."

Johanson's supporters took a completely opposite position, namely, that the ankle of *A. afarensis* was almost totally adapted for a humanlike, terrestrial bipedal gait, making impossible any substantial arboreal behavior. Latimer and Lovejoy in particular have published several articles micro-analyzing every curve of the *A. afarensis* foot and ankle bones as proof of exclusive terrestrial bipedalism (Gomberg and Latimer 1984, Latimer et al. 1987, Latimer and Lovejoy 1990a, Latimer and Lovejoy 1990b).

We note, however, that an author of a recent survey (Groves 1989) takes the side of Stern, Susman, Tardieu, Oxnard, and others who have argued for a substantial component of arboreality in *Australopithecus afarensis* and the australopithecines generally. Groves (1989, p. 310) said that in the australopithecines "bipedal locomotion was only part of a pattern which also incorporated sophisticated climbing ability."

J. H. Prosser (1980) of the University of Chicago concluded that the australopithecines, including Lucy, were primarily quadrupedal vertical climbers. "Quadrupedal vertical climbing produced a large number of . . . traits which have incorrectly assumed to have been bipedal adaptations," stated Prosser (1980, p. 186).

According to Prosser (1980, p. 175), the australopithecines, including *A. afarensis*, would have possessed, in addition to their arboreal capabilities, the capacity for "facultative terrestrial bipedalism." The word facultative means "optional" or "taking place under some conditions but not under others." In other words, the predominantly arboreal australopithecines, if the situation demanded, would have been able to move bipedally on the ground, perhaps in running from one

tree to another some distance away. This type of behavior is observed in many primates, including chimpanzees, orangutans, and gibbons. So the fossil evidence in no way obligates one to attribute to *A. afarensis* any specifically human locomotor behavior. According to Pross (1980, p. 188), the first true terrestrial bipedal hominid was *Homo habilis* (as understood before the

discovery of the apelike OH 62 Individual) or early *Homo erectus*.

R. H. Tuttle posited the existence of preaustralopithecine hominids displaying a kind of arboreal bipedalism. He called them hylobatians, after the genus *Hylobates*, which includes the modern gibbon. Tuttle (1981, p. 90) stated: "Vertical climbing on tree trunks and vines and bipedalism on horizontal boughs were conspicuous components of their locomotor repertoire. They commonly stood bipedally while foraging in trees . . . Short bursts of bipedal running and hindlimb-propelled leaps may have been important for the manual capture of insects and small vertebrates with which they supplemented their vegetable fare."

According to Tuttle (1981, p. 89), the Hadar hominids "had curved fingers and toes, strong great toes and thumbs, and other features that suggest they were rather recently derived from arboreal hominids [his hylobatians] and that they probably continued to enter trees, perhaps for night rest and some foraging. "

Studies of primate behavior apparently support the arboreal implications of the fossil morphology of *A. afarensis*. Susman stated: "We feel, based on extensive literature on free-ranging primates, that creatures such as represented by AL 288-1 could not have survived full-time on the ground. Today, all primates from common chimpanzees (which range from 27 up to 70 kg [59 to 154 lb.]), to vervet monkeys and baboons (which range from less than 3 to over 40 kg [7 to over 88 lb.]), are obliged at least to sleep in trees (or on rocky cliff-faces). They all feed in trees" (Susman et al. 1984, pp. 150-151). Susman

pointed out that pollen studies showed the presence of trees at the Hadar site (Susman et al. 1984, p. 151).

Having completed our review of the anatomy of *Australopithecus afarensis*, we conclude that Johanson was incorrect in stating that Lucy and her relatives were predominantly terrestrial bipeds and had "essentially human bodies" (Johanson and Edey 1981, p. 275). The picture that emerges is one of an arboreally adapted creature with long, curved toes and fingers, a long, heavily muscled arm equipped with an upward-pointing shoulder joint, a pelvis structured like that of apes, and a knee complex resembling that of the orangutan.

This view is not, however, very well represented in popular presentations. In order to maintain a believable human evolutionary sequence, the scientific community apparently requires, for propaganda purposes, a credible human ancestor in the Late Pliocene and Early Pleistocene. The erect, bipedal, nonarboreal hominid, with apelike head and humanlike body, as portrayed by Johanson and his disciples, satisfies this requirement far better than the almost totally apelike and



wholly or partially arboreal creature that emerges from the studies of Stern. Susman, Oxnard, and others. This Judgement is supported by the fact that the views of Johanson, Latimer, and Lovejoy make their way into college textbooks, popular books on evolution, television specials, and so on, with hardly a hint of any serious opposing conception. This, we believe, is not an accident. The informal gatekeepers and guardians of scientific orthodoxy are apparently quite careful about what reaches the public.

#### 11.9.8 Opposition to the Single Species Hypothesis

The idea that the large and small hominid individuals from Hadar and Laetoli represent a single sexually dimorphic species (*Australopithecus afarensis*) has not won universal acceptance among scientists.

Adrienne Zihlman (1985, p. 214) of the University of California (Santa Cruz) stated: 'The interpretation of extreme sexual dimorphism for these fossils has been a mere assertion from the beginning . . . and has continued to be so.'

In one of her reports, Zihlman (1985, pp. 216-217) supplied some data on sexual dimorphism in human beings, various apes, and *A. afarensis*. She found: "The Hadar fossils suggest even greater dimorphism than exists in orangutans, a species where males may be more than three times the body weight of females. This means that '*A. afarensis*' is more sexually dimorphic than any living hominoid. From the point of view of size, more than one species is strongly implied."

In the human species, males average only about 20 percent heavier than females. So even if, for the sake of argument, one accepts that *A. afarensis* with males more than three times heavier than females, did represent one species, the extreme degree of dimorphism argues strongly in support of apelike rather than humanlike morphology and behavior. And if Zihlman is right, and there were two species, not one, at Hadar, then Tim White sold Donald Johanson an illusion.

Todd Olson, an anthropologist at the City College of New York, concluded from cranial evidence that more than one species was present at the Hadar site. Olson discovered that the mastoid process in the larger Hadar individuals (such as AL 333-45) was "pneumatized" with small air pockets. The mastoid process is a bony projection behind the ear. A pneumatized mastoid is characteristic of *Australopithecus robustus*. The mastoid in the small Hadar individuals (Lucy), *A. africanus*, and *Homo sapiens* is nonpneumatized. The difference in mastoid structure between the large and small Hadar individuals, along with dental evidence, convinced Olson that two species rather than one were found at Hadar (Herbert 1983, pp. 10 -11). The larger individuals were, according to Olson, a population related to *Australopithecus robustus*, and the smaller individuals, including Lucy, were the earliest members of the *Homo* line. This is an interesting variation of the original two-species interpretation of the Hadar fossils, as proposed by Richard Leakey, who placed the larger individuals in the *Homo* line and characterized the smaller Lucy as a surviving *Ramapithecus*. Johanson and his supporters "took great exception to Olson's analysis, showing that the AL 333-45 basicranium is distorted and, if anything, is *Homo*-like" (Groves 1989, p. 262).

Dental evidence has also caused some workers to question the the view that a single species was present at Hadar. In Lucy, the first premolar has a single cusp, but in the other Hadar Jaws. the premolars, like those of modern humans. have a double cusp. Science News reported: "Yves Coppens, director of the Musee de l'Homme in Paris . . . and an original cosigner on the paper identifying *A. afarensis* as a species has now reversed himself based on the dental evidence — specifically the existence of both single-cusp and bicuspid premolars in the sample — he says there must have been two species coexisting at Hadar" (Herbert 1983, p. 11). Johanson and White, however, said that in an evolving line, some individuals would have the single cusp and others the bicuspid tooth.

Stern and Susman, like Johanson, originally believed the Hadar fossils represented the males and females of a single species exhibiting a high degree of sexual dimorphism. According to their view, the small females, including Lucy, would have been quite arboreal, the larger males less so.

Stern, however, eventually backed down from the sexual dimorphism concept. Science News reported in 1983: "he argues that the finger bones clearly sort themselves into two groups: one group [the small individuals] has strongly curved fingers — exactly like African apes — and the other [the large individuals] has less curved . . . fingers, halfway between gorillas and humans" (Herbert 1983, p. 9).

Stern said: "The finger bones pushed me over the edge. Taken in conjunction with the differences in the ankles and leg bones, I had to ask myself: Do you ever see such difference in living animals? And the answer is no — never. It's just too big a difference to be sexual dimorphism" (Herbert 1983, p. 9). Apparently, both species would have manifested arboreal behavior. Even the large First Family specimens had finger bones curved more than those of humans. They also had, as we have seen, long curved toes and a femoral anatomy similar to that of apes.

Where does all this leave us regarding our understanding of *Australopithecus afarensis*? Johanson and White and their supporters say *A. afarensis*, a terrestrial biped, was ancestral to *A. africanus* and the robust australopithecines, a line that finished in extinction. They also said *A. afarensis* was ancestral to the line leading from *Homo habilis* to *Homo sapiens*. Others say *A. afarensis* was a variety of *A. africanus*. which gave rise to the *Homo* line. Still others take a two-species approach. Tardieu (1981), studying the postcranial evidence, particularly the femurs, concluded that the larger individuals at Hadar represented the *Homo* line and the smaller individuals, like Lucy, something else. Y. Coppens, from studies of the dental evidence, reached a similar conclusion (Weaver 1985, pp. 592, 595). Richard Leakey also took the multiple-species approach, claiming that Lucy was a surviving *Ramapithecus* whereas the larger Hadar specimens represented the *Homo* line. Olson, studying features of the cranial anatomy, concluded that the larger Hadar individuals were like *Australopithecus robustus*, whereas Lucy was the first species in the *Homo* line (Herbert 1983, pp. 10-11). Susman felt the large and small Hadar types represented a single, partly arboreal species. Stern originally agreed with this, but later adopted a two-species view, as a result of his studies of the finger anatomy. Finally, Oxnard and others believed *A. afarensis* to be an apelike arboreal creature with no direct relation to the human line.

This brief review does not, however, exhaust the various opinions about the phylogenetic status of *A. afarensis*. "For Ferguson (1983, 1984) the Hadar sample contains three different taxa: *Sivapithecus* sp., *Australopithecus africanus*, and *Homo antiquus* (new species)," noted Groves (1989, p. 262). Groves himself (1989, p. 263), in his comprehensive taxonomic survey of the hominids, said: "Certainly the post-cranial data are absolutely clear, and split the Hadar sample into two divisions." Groves (1989, p. 263) classified one Hadar group as early *Homo* and the other as an unnamed new hominid genus. Under the species designation *Australopithecus afarensis*, he kept only the Laetoli Jaws. So Groves, like Ferguson, found three species instead of one in the *A. afarensis* fossils of Johanson and White.

Within the scientific community there is as of yet no unanimous picture of what the australopithecines, including *A. afarensis*, were really like, both in terms of their morphology and their phylogenetic relation with modern humans. The field is still wide open and full of conflicting views.

Nevertheless, we find the argument for a substantial component of arboreality in the locomotor behavior of *A. afarensis* more credible than that for exclusive terrestrial bipedalism. There also appears to be good reason to suppose the Hadar hominid fossils represent more than one species. Furthermore, we favor the view, espoused by Louis and Richard Leakey, that no australopithecine, including *A. afarensis*, warrants being labeled a human ancestor.

Just as today we find true humans coexisting with various categories of apes, some more humanlike than others, the same was true in the past, as far back as our research can carry us. In fact, an objective review of the evidence yields signs of anatomically modern human beings tens of millions of years ago, a fact distinctly incompatible with any current evolutionary model.

#### 11.10 The Laetoli Footprints

The Laetoli site is located in northern Tanzania, about 30 miles south of Olduvai Gorge. Laetoli is the Masai word for red lily. The area was first explored by the Leakeys in 1935. Later, Mary Leakey returned to Laetoli and discovered some hominid jaws, which she regarded as early *Homo*.

One day in 1979, Dr. Andrew Hill of the Kenya National Museum and several other members of Mary Leakey's expedition were playing around, throwing pieces of elephant dung at each other. In the course of this sport, Hill noticed some marks on the ground. They proved to be fossil footprints of animals. Subsequently, Peter Jones and Philip Leakey, the youngest son of Louis and Mary Leakey, discovered among the footprints some that appeared to have been made by hominids. The prints had been impressed in layers of volcanic ash, dated by Gamiss Curtis, using the potassium-argon method, at from 3.6 to 3.8 million years old.

National Geographic magazine featured an article by Mary Leakey titled "Footprints in the Ashes of Time." A caption to a photo of some hominid prints read: "The best-preserved print shows the raised arch, rounded heel, pronounced ball, and forward-pointing big toe necessary for walking erect. Pressures exerted along the foot attest to a striding gait" (M. Leakey 1979, p. 452). Dr.

Louise Robbins, a footprint expert from the University of North Carolina, observed: "They looked so human, so modern, to be found in tuffs so old" (M. Leakey 1979, p. 452).

Readers who have accompanied us this far In our intellectual Journey will have little difficulty in recognizing the Laetoli footprints as potential evidence for the presence of anatomically modern human beings over 3.6 million years ago in Africa. We were, however, somewhat astonished to encounter such a striking anomaly in the unexpected setting of the more recent annals of standard paleoanthropological research. What amazed us most was that scientists of worldwide reputation, the best in their profession, could look at these footprints, describe their humanlike features, and remain completely oblivious to the possibility that the creatures that made them might have been as humanlike as ourselves.

Their mental currents were running in the usual fixed channels. Mary Leakey (1979, p. 453) wrote: "at least 3,600,000 years ago, in Pliocene times, what I believe to be man's direct ancestor walked fully upright with a bipedal, free-striding gait. ... the form of his foot was exactly the same as ours."

Who was the ancestor? Here we once more confront the debate, between the Leakeys on one hand and Johanson and White on the other, about the number and type of species represented by the fossil materials from Hadar and Laetoli.

Taking the Leakeys' point of view, the Laetoli footprints would have been made by a nonaustralopithecine ancestor of *Homo habilis*. Taking the JohansonWhite point of view, the Laetoli footprints would have been made by *Australopithecus afarensis*. In either case, the creature who made the prints would have had an apelike head and other primitive features.

But why not a creature with fully modern feet and fully modern body? There is nothing in the footprints that rules this out. Furthermore, we have compiled in this book quite a bit of fossil evidence, some of it from Africa, that is consistent with the presence of anatomically modern human beings in the Early Pleistocene and the Late Pliocene.

The most prominent set of tracks at Laetoli represented the footprints of three hominids, one larger than the others. Applying an anthropological rule of thumb that a hominid's foot length represents 15 percent of the creature's height, Mary Leakey (1979, p. 453) calculated that the largest hominid stood 4 feet, 8 inches tall, whereas the next largest stood 4 feet tall. The smallest would have been still shorter. Leakey hypothesized that the largest individual was an adult male, the next largest an adult female, and the smallest a child. Admitting this was only a guess, she suggested the alternative possibility that the second largest set of prints might represent a Juvenile male (M. Leakey 1979, p. 453). One cannot, however, be certain that the largest tracks represent a fully adult form either. Even so, the heights of the creatures that made the two larger sets of tracks, as estimated by Mary Leakey, fall within the modern human adult range.

Are we perhaps exaggerating the humanlike features of the Laetoli footprints? Let us see what various researchers have said. Louise M. Robbins, who provided an Initial evaluation of the Laetoli prints to Mary Leakey In 1979, later published a more detailed report. Several sets of tracks,

Identified by letters, were found at Laetoli. In examining the "C" trails, representing the three individuals described by Mary Leakey as a possible family group, Robbins (1987, p. 501) found that the prints "share many features that are characteristic of the human foot structure."

Robbins (1987, p. 501) noted: "Each hominid has a non-divergent great toe, or toe 1, and that toe is about twice as large as toe 2 beside it." She found the spacing between toes 1 and 2 "no greater than one finds in many people today, including individuals who habitually wear shoes" (1987, p. 501). Robbins also found "the ball region of the hominids' feet is of human form" and added that the feet displayed "a functionally stable longitudinal arch structure" (1987, p. 501). Finally, she observed that "the heel impressions in the hominids' footprints appear human in their form and in their locomotory performance" (Robbins 1987, p. 501).

Robbins (1987, p. 501) therefore concluded that "the four functional regions—heel, arch, ball, and toes—of the hominids' feet imprinted the ash in a typically human manner" and that "the hominids walked across the ash surface in characteristic human bipedal fashion."

Concerning the size of the prints, Robbins (1987, p. 502) stated: "The assumed dimensions of the C-2 footprints do indeed fall well within the adult male range of a sample of American subjects, and the measurements of C-3's footprints fall in the lower portion of the range for adult females in the American sample. The dimensions of the C-1 footprints, however, are well below dimensional ranges for American adults but within foot length and width ranges for a small sample of immature individuals. . . . Nonetheless, it is mere conjecture at this stage of hominid footprint investigation to suggest that the Site C hominids may have been a male, a female, and an offspring who were walking from an area of falling volcanic ash."

M. H. Day studied the prints using photogrammetric methods. Photogrammetry is the science of obtaining exact measurements through the use of photography. Photogrammetric methods are extensively used by cartographers in making accurate contour maps from aerial photographs. Day (1985, p. 121), having found the same techniques useful on the miniature geography of footprints, stated: "What these footprints, and their photogrammetric analysis, show is that bipedalism of an apparently human kind was established 3.6 million years ago. The mechanism of weight and force transmission through the foot is extraordinarily close to that of modern man." His study showed the prints had "close similarities with the anatomy of the feet of the modern human habitually unshod: arguably the normal human condition" (Day 1985, p. 121).

Typically, Day (1985, p. 125) concluded: "There is now no serious dispute as to the upright stance and bipedal gait of the australopithecines."

But what proof did he have that an australopithecine made the Laetoli footprints? There is no reason to rule out the possibility that some unknown creature, perhaps very much like modern *Homo sapiens*, was the cause of them.

R. H. Tuttle (1981, p. 91) stated: "The shapes of the prints are indistinguishable from those of striding, habitually barefoot humans."

Tuttle (1987, p. 517) concluded: "Strictly on the basis of the morphology of the G prints, their makers could be classified as *Homo sp.*' because they are so similar to those of *Homo sapiens*, but their early date would probably deter many palaeoanthropologists from accepting this assignment. I suspect that if the prints were undated, or if they had been given younger dates, most experts would probably accept them as having been made by *Homo*." Tuttle (1987, p. 517) also stated: "They are like small barefoot *Homo sapiens*."

Furthermore, Tuttle held that the *A. afarensis* foot could not have made the prints. Of the AL 333-115 foot, he said: "The shafts of the proximal phalanges are markedly curved ventrally. This feature is characteristic of certain full-time and part-time arboreal apes and monkeys. ... It is difficult to imagine a foot with such markedly curved phalanges fitting neatly into the footprints at Laetoli" (Tuttle 1981, p. 91). The same would be true of any australopithecine foot.

Stern and Susman (1983) objected to this. Convinced that the apelike *A. afarensis* foot had made the Laetoli footprints, they proposed that the ancient hominids had walked across the volcanic ash with their long toes curled under their feet, as chimpanzees have sometimes been observed to do. Curled-under toes would explain why the *A. afarensis* footprints at Laetoli so much resembled those made by the relatively short-toed human foot.

Could an australopithecine walking with curled toes have made the humanlike prints? Tuttle (1985) found this extremely unlikely. If the Laetoli hominid had long toes, then, said Tuttle, one would expect to find two patterns of toe impressions— long extended toes and short curled toes, with extra-deep knuckle marks. Tuttle (1985, p. 132) observed: "Neither pattern exists at Laetoli G so we can infer that their lateral toes were quite short. " This meant the long-toed *afarensis* foot could not have made the prints.

Even Tim White, who believed *Australopithecus afarensis* made the footprints, stated: "The Stern and Susman (1983) model of toe curling 'as in the chimpanzee' predicts substantial variation in lateral toe lengths seen on the Laetoli prints. This prediction is not borne out by the fossil prints" (White and Suwa 1987, p. 495).

Stern and Susman did in fact claim that a few of the Laetoli footprints gave signs of toes longer than in humans. Tuttle (1985, p. 132) admitted that "the right foot of G-1 sometimes left peculiar marks distal to the toe tips." To Stern and Susman, the marks forward of the "toe tips" represented the actual toe tips of uncurled toes. But Tuttle had another explanation for the marks. He wrote: "These are best explained by . . . the tendency for G-1 to drag its foot on lift off probably due to pathology of the lower limb" (Tuttle 1985, p. 132). The fact that the peculiar markings appeared only on one foot of one individual, and then only sometimes, lends support to Tuttle's explanation.

Stern and Susman (1983) also suggested that the Laetoli prints did not have a deep rounded impression at the base of the big toe, representing the ball of the foot in humans. They regarded this as evidence that the foot that made the prints was not human. But Tuttle (1985, p. 132) observed that "humans commonly leave prints devoid of these features as may be seen in prints

on the beach." And, as we have seen, Robbins (1987, p. 501) said the prints she studied did have a "humanlike" ball region.

Directly challenging Johanson, White, Latimer, and Lovejoy, who asserted *Australopithecus afarensis* made the Laetoli prints, Tuttle (1985, p. 130) said: "Because of digital curvature and elongation and other skeletal features that evidence aboreal habits ... it is unlikely that *Australopithecus afarensis* from Hadar, Ethiopia, could make footprints like those at Laetoli." Such statements have provoked elaborate counterattacks from Johanson and his followers, who have continued to promote the idea that *A. afarensis* could have made the tracks.

Tim White, for example, published a study (White and Suwa 1987) of the Laetoli prints in which he disputed Tuttle's contention that their maker was a hominid more advanced than *A. afarensis*.

White asserted: "there is not a single shred of evidence among the 26 hominid individuals in the collection of over 5,000 vertebrate remains from Laetoli that would suggest the presence of a more advanced Pliocene hominid at this site" ( White and Suwa 1987, p. 496). But, as we have seen in our review of African hominid fossils, there are in fact a few "shreds" of evidence for the presence of sapiens-like creatures in the Pliocene, some not far from Laetoli. Also, it is well known that human skeletal remains are quite rare, even at sites where there are other unmistakable signs of a human presence.

Like Tuttle, White rejected the curled-toe hypothesis of Stern and Susman. Instead, White tried to fit the foot of *A. afarensis* to the Laetoli prints. This was very difficult because no complete foot skeleton of *A. afarensis* had been found at the Hadar site. A partial foot skeleton, however, had been recovered. This was the AL 333-115 foot skeleton, which included only bones from the front part of the foot—phalanges and metatarsal heads.

According to White, the best tracks at Laetoli were in the G-1 trail, representing the smallest of the three individuals of the G group. Even White admitted that the phalanges of AL 333-115 were "obviously incompatible with the G-1 tracks" (White and Suwa 1987, p. 497). Stern and Susman, and Tuttle, found them incompatible with any of the tracks. White, however, pointed out that the AL 333-115 individual represented one of the larger, presumably male, members of the First Family group and proposed that the foot of Lucy, one of the smaller, female individuals, might have fitted the G-1 Laetoli prints.

But the only bones recovered from Lucy's foot were an ankle bone and two toe bones. White therefore decided to use a partial *Homo habilis* foot skeleton (OH 8) from Olduvai Gorge to reconstruct the rear part of Lucy's foot. White reduced the OH 8 foot by 10 percent to bring it down to the size of Lucy's ankle bone (talus). He then scaled the large AL 333-115 toes bones down to the size of Lucy's few toe bones, and used them to make up the rest of the foot ( White and Suwa 1987, p. 502). According to White, this speculatively reconstructed foot matched the prints.

White predicted that “the discovery of a complete foot skeleton at Hadar or Laetoli will conform in its basic proportions with the reconstruction described in this paper” (White and Suwa 1987, p. 512). But this prediction remains to be fulfilled. It is interesting that the most complete afarensis foot skeleton now available (AL 333-115) definitely does not fit any of the prints.

White also predicted that “the Laetoli prints will eventually be shown to be subtly distinct from those left under analogous conditions by anatomically modern humans” (White and Suwa 1987, pp. 510, 512). But as far as anyone can see now, they are indistinguishable from those of modern humans. Even White himself once said: “Make no mistake about it. They are like modern human footprints. If one were left in the sand of a California beach today, and a four-year-old were asked what it was, he would instantly say that somebody had walked there. He wouldn't be able to tell it from a hundred other prints on the beach, nor would you. The external morphology is the same. There is a wellshaped modern heel with a strong arch and a good ball of the foot in front of it. The big toe is in a straight line. It doesn't stick out to the side like an ape toe” (Johanson and Edey 1981, p. 250).

And Tuttle (1985, p. 130) noted: “in all discernible morphological features, the feet of the individuals that made the Laetoli trails are indistinguishable from those of modern humans.”

#### 11.11 Black Skull, Black Thoughts

In 1985, Alan Walker of Johns Hopkins University discovered west of Lake Turkana a fossil hominid skull stained dark by minerals. Called the Black Skull, it raised questions about Donald Johanson's view of hominid evolution.

According to Johanson, *Australopithecus afarensis* gave rise to two lines of hominids. This arrangement can be visualized as a tree with two branches. The trunk is *Australopithecus afarensis*. On one branch is the Homo line, proceeding from *Homo habilis* to *Homo erectus* to *Homo sapiens*. On the second branch are the australopithecines arising from *Australopithecus afarensis*.

Johanson and White claimed that *Australopithecus afarensis* gave rise to *Australopithecus africanus*, which in turn gave rise to *Australopithecus robustus*. The trend was toward larger teeth and jaws, and a larger skull with a ridge of bone, the sagittal crest, running lengthwise along the top. The sagittal crest served as a point of attachment for the powerful jaw muscles of robust australopithecines. *Australopithecus robustus* then supposedly gave rise to the superrobust *Australopithecus boisei*, which manifested all the above-mentioned features in an extreme form.

In an article titled “Baffling Limb on the Family Tree,” Walker's wife Pat Shipman, also of Johns Hopkins University, explained the evolutionary significance of the Black Skull, designated KNM-WT 17000.

The first specimens of *Australopithecus robustus* were, it was thought, about 2 million years old (Johanson and Edey 1981, p. 283). But the Black Skull, with its *Australopithecus boisei* features, including the largest cranial crest of any hominid (Shipman 1986, p. 91), was 2.5 million years old.



Shipman believed this meant that *Australopithecus boisei* and the boisei-like Black Skull could not be descended from *Australopithecus robustus*, as believed by Johanson and others.

So where does that leave us? Here is one possibility suggested by Shipman. On our hominid family tree, we could now go from *Australopithecus afarensis* up one branch to *Australopithecus africanus*. Then from *Australopithecus africanus* could come two separate branches. On one branch is *Australopithecus robustus* and on the other *Australopithecus boisei* and the boisei-like Black Skull. In other words, instead of deriving *Australopithecus boisei* from *Australopithecus robustus*, both originate from *Australopithecus africanus*.

But perhaps not. "All known africanus skulls share many features that are derived, i.e., advanced, relative to those of the new skull, such as a moderate flexion or angling of the base of the cranium and a deep jaw joint with a bony lump in front of it." said Shipman (1986, p. 91).

So, according to Shipman, another possibility now emerges—that *Australopithecus africanus*, although ancestral to *Australopithecus robustus*, might not have been ancestral to *Australopithecus boisei* and the boisei-like Black Skull.

This leaves us with a three-branched family tree. Down at the bottom we still have *Australopithecus afarensis*. Above are three branches—the Homo line on the first, *Australopithecus boisei* and the Black Skull on the second, and then *Australopithecus africanus* on the third, leading to *Australopithecus robustus*.

But Shipman pointed out that it then becomes difficult to account for the fact that *Australopithecus boisei* and *Australopithecus robustus* are so similar. If *Australopithecus robustus* came from *Australopithecus africanus* and *Australopithecus boisei* from *Australopithecus afarensis*, then *Australopithecus boisei* and *Australopithecus robustus* would have had to develop their robust similarities independently by parallel evolution, something that is possible but unlikely.

According to Shipman, another way to explain the similarities between *Australopithecus boisei* and *Australopithecus robustus* is to propose that *Australopithecus robustus* was not descended from *Australopithecus africanus* and that *Australopithecus robustus* and *Australopithecus boisei* had a common ancestor besides *Australopithecus africanus*—perhaps *Australopithecus afarensis*.

So now we have a four-branched tree, with *Australopithecus afarensis* at the bottom. Above are the Homo line, *Australopithecus africanus*, *Australopithecus robustus*, and *Australopithecus boisei*, all separate from each other.

Shipman found it very hard to believe that a single species, *Australopithecus afarensis*, could have given rise to four separate lineages. So where did the four new species come from?

Shipman suggested that one should take a very hard look at the idea that *Australopithecus afarensis* represents just one sexually dimorphic species. She pointed out, as we have discussed in

Section 11.9.8, that some scientists have concluded that "at least two species of *Australopithecus* and possibly *Homo* are mistakenly lumped together into *afarensis*" (Shipman 1986, p. 90).

Walker said it is likely that "the specimens identified as *Australopithecus afarensis* include two species, one of which directly gives rise to *Australopithecus boisei*" (Walker et al. 1986, p. 522).

How did Johanson respond to the discovery of the boisei-like Black Skull? He admitted that the Black Skull complicated things, making it impossible to arrange *Australopithecus africanus*, *Australopithecus robustus*, and *Australopithecus boisei* in a single line of succession coming from *Australopithecus afarensis*. Johanson proposed 4 possible arrangements of these species, along the lines we have been discussing, without suggesting which one was correct (Johanson and Shreeve 1989, p. 126). There was, he said, not yet enough evidence to decide among them.

The uncertainty about the number of species at Hadar, combined with the confused relationships among the successor species (*Australopithecus africanus*, *Australopithecus robustus*, *Australopithecus boisei*, and *Homo habilis*), create problems for evolutionists attempting to construct a phylogenetic tree for these hominids. Shipman (1986, p. 92) stated: "the best answer we can give right now is that we no longer have a very clear idea of who gave rise to whom." Walker warned that the discovery of KNM 17000 suggested "that early hominid phylogeny has not yet been finally established and that it will prove to be more complex than has been stated" (Walker et al. 1986, p. 522).

In the midst of the new complexity, one question is especially important—the origin of the *Homo* line. Shipman told of seeing Bill Kimbel, an associate of Johanson, attempt to deal with the phylogenetic implications of the Black Skull. "At the end of a lecture on *Australopithecine* evolution, he erased all the tidy, alternative diagrams and stared at the blackboard for a moment. Then he turned to the class and threw up his hands," wrote Shipman (1986, p. 93). Kimbel eventually decided the *Homo* line came from *Australopithecus africanus* (Willis 1989). Johanson and White continued to maintain that *Homo* came directly from *Australopithecus afarensis*.

After she considered various phylogenetic alternatives and found the evidence for all of them inconclusive, Shipman (1986, p. 93) stated: "we could assert that we have no evidence whatsoever of where *Homo* arises from and remove all members of the genus *Australopithecus* from the hominid family. . . . I've such a visceral negative reaction to this idea that I suspect I am unable to evaluate it rationally. I was brought up on the notion that *Australopithecus* is a hominid." This is one of the more honest statements we have heard from a mainstream scientist involved in paleoanthropological research.

In the foregoing discussion, we have considered only the evidence that is generally accepted by most scientists. Needless to say, if we were to also consider the evidence for anatomically modern humans in very ancient times that would complicate the matter even further.

Having reviewed the history of African discoveries related to human evolution, we can make the following summary observations. (1) There is a significant amount of evidence from Africa

suggesting that beings resembling anatomically modern humans were present in the Early Pleistocene and Pliocene. (2) The conventional image of Australopithecus as a very humanlike terrestrial biped appears to be false. (3) The status of Australopithecus and Homo erectus as human ancestors is questionable. (4) The status of Homo habilis as a distinct species is questionable. (5) Even confining ourselves to conventionally accepted evidence, the multiplicity of proposed evolutionary linkages among the hominids in Africa presents a very confusing picture. Combining these findings with those from the preceding chapters, we conclude that the total evidence, including fossil bones and artifacts, is most consistent with the view that anatomically modern humans have coexisted with other primates for tens of millions of years.

# FORBIDDEN SCIENCE

Journals 1957-1969

Jacques Vallee

North Atlantic Books  
Berkeley, California

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# FOREWORD

It is unusual *for scientists* to keep diaries and even more unusual for them to make them public. While we know much about the intimate lives and personal motivations of musicians, movie stars and literary figures, the day-to-day life of scientists remains carefully veiled, as if science somehow arose spontaneously by a process which superseded the mere activities of mortals.

Like most of my colleagues, I have followed this rule of silence for the last thirty years, never expecting that these Journals would be published before my death. But I have finally decided that I had no right to keep them private any more. Although they contain many passages that are very personal and some that are painful, they **also** provide a primary source about a crucial fact in the recent historical record: the appearance of new classes of phenomena that highlighted the reality of the paranormal. These phenomena were deliberately denied or distorted by those in authority within the government and the military. Science never had fair and complete access to the most important files. This fact has been alleged before, but never proven. The present book proves it.

Publication was not considered when the pages of these Journals slowly accumulated in the form of copybooks, loose pages, letters and marginal notes. I simply regarded it as a useful intellectual and spiritual discipline to review for myself the events of each period, if not those of each day. At first this exercise helped me cope with the uncertainties and the rapid changes in my life as a student in France. Later, when I moved to the United States, the Journal became a confidant and, more importantly, an adviser, a crystal ball, a tool to interrogate the future and to explore its potential.

It turns out that the thirteen years covered here, from 1957 to 1969, saw some of the most exciting events in technological history: the first space adventures, the rise of the computer, the electronic revolution, the invention of advanced software, the flight to the moon, the first detailed **images** of other planets. As a young scientist I was a minor contributor to

some of these events, an avidly interested observer of others. These developments which changed our world are well-documented in countless books. Behind the grand parade of the visible breakthroughs in science, however, more private mysteries were also taking place. The paranormal, with its claims and counter-claims about telepathy, dowsing, astrology, healing and other effects, was a matter of sharp debate and secret passion among believers and skeptics. And there were even more exciting events taking place: all over the world people had begun to observe what they described as controlled devices in the sky. They were shaped like saucers or spheres. They seemed to violate every known principle in our physics.

Did these objects constitute the first signal of imminent contact with alien civilizations from outer space at a time when we were designing our own space probes? Governments took notice, organizing task forces, encouraging secret briefings and study groups, funding classified research . . . and all the time denying before the public that any of the phenomena might be real.

What the media and the scientific world were told by those responsible for public welfare had little to do with what was happening. Anyone reviewing that period and looking solely at the official story will have no chance of coming to grips with the truth about the unfolding drama. In fact, ***the major revelation of these Diaries may be the demonstration of how the scientific community was misled by the government, how the best data were kept hidden, and how the public record was shamelessly manipulated.***

Witnesses of the strange occurrences numbered in the millions. But the study of their observations had been forcefully driven underground. It had turned into a fascinating discipline in a hypocritical modern world that claimed rational thought and open inquiry as its highest standard: *it had become a Forbidden Science.*

No reminiscences of that era can be credible unless they are supported by the daily record of conversations, meetings and research results made by a participant in the actual events. I kept such a record and I was such a participant, first as a direct witness to the phenomenon in 1955, then as a French Government astronomer, and later as a computer scientist who played a significant role in detecting and publishing some of the major patterns behind the mystery and in arguing for its reality. In that phase of my work I was a close associate of Dr. J. Allen



## FOREWORD

Hynek, the man who was scientific consultant for the U.S. Air Force on the UFO problem for nearly a quarter century, specifically from 1947 to 1969.

Several factors make it important to bring these notes, however personal and fragmentary, to the attention of the public. Only one book was published by a professional historian who took an interest in the field, but it is marred by distortions and errors of omission. And there is a growing misunderstanding of the actual role played by Dr. Hynek in the study of unidentified flying objects.

Allen Hynek liked to remind us that beyond today's science there would be a twenty-first century science that would have to take into account phenomena that seemed paranormal to us simply because of our parochial mental attitudes and the limitations of what he aptly called our cultural provincialism. I hope to bring him back to life here, along with Dr. James McDonald and other figures of that era.

The record stops twenty years ago, as I arrived in California where I now live with my family. I have augmented it with an Epilogue that brings the reader up to the present. Indeed, many important events that have taken place in the intervening period throw new light on the theories I formed before 1969. Some of these theories have turned out to be quite accurate; some were wrong, and the true facts were only revealed later. Other facts are still hidden. When they eventually come to the surface, as they must, it is my hope that this statement of the early years of our research into *Forbidden Science* may serve to highlight their true significance.

I fully recognize that this is only one man's perspective on a series of very complex events. Because this book is a compilation of diaries, it contains opinions that are no longer mine and judgments I now regret, along with much evidence of mistakes I made along the way. I owe many thanks to Janine, to Richard Grossinger and especially to Lindy Hough at North Atlantic Books for their guidance in editing, pruning and streamlining the text. However it was not appropriate, of course, to change the record. At this late date I can only beg the forgiveness of those who may feel that my pen, often "hurriedly dipped in the inkwell of frustration," was overly rash.

Jacques Vallee  
San Francisco, January 1992.

Part One

# SUB-SPACE

# 1

## **Pontoise. Christmas Eve 1957.**

Never again will I wait for Philippe near my house on Saint-Jean street. Such is the sudden realization that fills my mind, and these words seem to match the color of my wall covered with red ivy, the color of my whole childhood.

Philippe is a high school classmate, an old friend. When I lived here in my parents' house, my house, he used to come and pick me up every morning precisely at ten to eight, on the way to school. I would already be in the street, walking ever so slowly, to give him time to appear at the turn beyond the grocery store. This went on for years, when we were eight, when we were fifteen. The perspective of things changed gradually without our noticing it.

Now life is separating me from all the things I have known. I suddenly realize that this little town where I grew up is no longer my town; that I do not belong in the streets along which I walked in years past. Philippe is going away to study for a bachelor's degree in physics, my other friends are scattered far and wide. As for me, I am eighteen years old now. I am already forgetting the speeches of Cicero and the art of scaring away the neighbor's cat with my slingshot. Only last week, in Paris, I parted with my first mistress, a tiny girl from Brittany who cried at the movies.

I am trying to enter the life of a scientist, of a man who peels apart new concepts like the skins of an onion to remove each layer. Yet on the other side, on the side of my ivy-covered wall, I have a hard time giving up the slingshot kid.

This anguish of a Christmas night filled with the books of old and the tree of tradition, this anguish is born of sorrow. There was a need for something that would mark the transition. Indeed everything will change again: my father will not live long. A certain pain in my brother's voice, a pain he could not hide over the phone, was the signal.

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I will not be very demonstrative when the end comes. But this is Christmas and the bells, the obsolete bells of nearby Saint Maclou, ring at midnight, urging me to write.

Consider the whole existence of a man, with all of the ramifications that implies in the existence of others, in their minds, in their consciousness: How can all that be annihilated by more or less simple chemical degradation of the cells? I understand why people still need to erect a God to store within it this kind of dilemma. Never again will I wait for Philippe. And soon I will no longer see my father, my old father, walking from one room to another in my old house. And my old house on Saint-Jean street must disappear forever as well, with all the stars that are above and the trees around it. How am I expected to find a grave big enough for all that?

### **Paris. 10 January 1958.**

I spent New Year's Eve at the Mexican cafe, our Headquarters on *Place de la Contrescarpe*. It is a tiny square on the eastern edge of the Latin Quarter, surrounded by quaint shops and picturesque buildings, home to tramps and winos. The whole gang was there, including Granville and the Baron, my friend Claudine and others, unavoidable others. Now classes are starting again at the Sorbonne.

My brother is a medical specialist. He has discussed our father's illness with his colleagues in Pontoise: there is no hope.

### **Paris. 20 January 1958.**

A frozen impression, a strange release: My father is dead. Oddly, I don't have the feeling of having "lost" anything, of having less substance. On the contrary, I have this absurd sense: I have come closer to a certain reality. But here is the sorrow, to have lost in potential what I gained in knowledge. The new emotions I have just gained are useless.

After I watched him die, and kissed him, I went out into the street. The first snow had fallen, in fine heavy layers, pure perfection against a great blue silent sky. I was astonished at the sudden beauty of the world.

In today's society there can be no harmony among people like my father, my brother, and me. We disagree viscerally on too many subjects, from morality to music, or the war in Algeria. He was a stern conservative and we yearn for change. That's normal, and also cruel. The Norm

excludes any tears: it rules, that is all.

Will things go the same way for me and my children? Probably. Unless I find what I am looking for: well-defined substance, unbounded potential within myself. Is that possible?

Back in Paris, at the student house where I live, I am pretending that life carries me along normally. I have not taken anyone into my confidence. I want to keep all my strength for the future.

### **Paris. 15 February 1958.**

The French Astronomical Society has just published an account I sent them of an unusual sighting of the first *Sputnik*. It's an observation I made last year in Pontoise, from the terrace behind the house. The event took place three months ago on Sunday, 24 November 1957 at 5:54 p.m. Watching the object, probably part of the booster rocket, I found it similar to Jupiter in apparent size and luminosity as it passed through Cassiopeia. It got lost in the Southeast in less than two minutes.

Having heard that the booster of *Sputnik* had broken up into several pieces I waited for any other object that might follow on a similar orbit. Indeed at 6:10 p.m. I saw a faint luminous trail with the naked eye. It was rising between the first two stars of the Big Dipper, in the direction of Polaris. I looked at it with a telescope given to me by my uncle Maurice, my father's brother. This instrument is an antique World War I artillery refractor with a magnification of 25 which enabled me to see a small orange point at the tip of the trail. I lost it after about 15 seconds, but the trail remained clearly visible in the sky and it drifted to the zenith at 6:30 p.m. I sent an account of the whole thing to the French Astronomical Society and to Paul Muller, head of the artificial satellite service at Paris Observatory in Meudon. Now it turns out that another amateur astronomer saw precisely the same thing from Joinville, and our two observations appear together in *L'Astronomie*.<sup>1</sup>

### **Paris. 20 March 1958.**

The world is changing. This city has been shaken by sudden political upheaval. History is accelerating. I can feel it turning from its usual elusiveness to the consistency of a liquid or a jelly. The Fourth Republic is threatened from the Right, as a result of the lingering, impossible war in Algeria where a large, conservative and increasingly militant French

## FORBIDDEN SCIENCE

population remains. My friends among the students expect things to turn nasty. Trucks covered with greenish-brown fabric are in evidence throughout the Latin Quarter; jeeps equipped with radio transmitters drive up and down the Boulevard Saint-Michel. I have just seen half a dozen trucks filled with gendarmes parked in front of the Pantheon. They seem to be expecting a full-scale riot.

French democracy may be about to pay for all the mistakes it has made over the years, especially this stupid war. Like my fellow students, I am outraged at all the lies spread by the bureaucracy, the censorship, the denial of the tortures committed in North Africa by the French Army: our country is engaged in the same kind of actions that we were taught would forever designate the German Nazi to the shame of the whole world. We wonder what this means for us. What kind of future are we studying for?

### **Paris. 27 March 1958.**

I just wrote a letter to my mother, who is now alone in that big house my parents have rented in Pontoise for the last sixteen years:

I got back safely last Sunday on that excellent train. I was back exactly at 9:30 p.m. Some thirty cops were stationed at the subway entrance. They had machine guns and everything that is necessary in order to kill people. They were systematically stopping anyone who looked like an Arab.

My work goes along well. I am studying hard for the Analysis exams, and now it's only a question of spending more time with the books and the homework problems. My goal is to pass the written part in June.

I will make arrangements to spend more time with her at Easter. It makes me sad to imagine her alone in Pontoise. She has always looked at the world through the eyes of a great lady. She is quick to assess people's character, quick to rescue the lost child, to feed the poor beggar, to get angry at injustice. She comes from an industrious family of Protestants whose various branches extend all over Europe. Her parents went broke when the flood of 1910 wiped out their fur and pelt trading business. She raised her thirteen brothers and sisters by herself, and there was no opportunity for her to finish school. But her heart is as big as the whole

world and her mind has the direct intuition that needs no schooling.

Recently I came across a picture of us taken when I was about eight years old, on the terrace in Pontoise. My father is dressed in his Sunday best, a three-piece suit and a tie. My mother holds my hand. I lean against the wall, without a care in the world.

**Paris. 16 April 1958.**

Normal work has become impossible, life is suspended. The Government has fallen. Socialist leader Guy Mollet warns of "a crisis of Regime" and calls for a new Popular Front, while powerful appeals to a neo-fascist "Comite de Salut Public" (Public Salvation Committee) are heard from the Right. All this is drowned in idiotic commentaries by our well-informed media: "the crisis will be long and painful," a political journalist has stated in all seriousness; "the President expects to solve it rapidly." The President in question is Monsieur Coty, a nice old man who has never done anything rapidly in his life. I have heard another politician, Le Troquer, adding ponderously that "depending on the circumstances, the crisis may be long or short." Only one thing seems clear to me: if the Assembly does not come to some decision soon, time will work in favor of an overthrow of the regime: we will either get General De Gaulle, or a new Popular Front.

**Paris. May Day 1958.**

The crisis has entered its fifteenth day. Fights have started. This afternoon I found myself on rue Mouffetard returning from a demonstration in protest of the execution of a young Algerian. The Latin Quarter was full of sunshine. Around us the market was bustling with activity, with its open-air displays of fruits and vegetables, the stalls selling meat and fresh fish, flowers, ham and sausages. Suddenly, frantic screams made us whirl around: A struggle was erupting. Foolishly, we were tempted to watch and we came closer. A dozen men were engaged in a brawl in the narrow street. One of them, a fellow in his forties, produced a heavy stick and started swinging, but others jumped on him and the stick rolled away. As he freed himself, a gun in his hand caught the sun. Stunned, his assailants took a hurried step back. We did the same, with that sick feeling: who would catch the first bullet? I was less than ten feet away. He turned and rushed ahead into the crowd.

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Someone yelled: "The cops!" The participants scattered down the side streets.

Soon the entire area was surrounded, from the Gobelins to rue d'Ulm. Police buses blocked every corner, machine guns and radio transmitters in evidence. I saw a car and two motorcycles coming down the medieval streets of Contrescarpe. We ran away from them towards Place Monge. A helicopter flew low over the rooftops. Now there is a rumor that the man was a provocateur, that he worked for the police, who were seeking an excuse to come into the area in force.

A huge black bus full of cops-, unable to wedge its way down the narrow street to the church of Saint-Medard, was forced to drive backwards all the way up rue Mouffetard under the catcalls and the jokes of the shopkeepers, the peddlers, the old women of the market.

The Latin Quarter, which has seen many a revolution down through history, remains effervescent tonight. The helicopter keeps flying in narrow circles.

### **Paris. 12 May 1958.**

Things are getting worse. A very bland politician named Pflimlin is attempting to form a new Cabinet. The Far Right seems ready to take drastic action to overthrow him and seize control. In our section of Paris there is an intense war of the walls. Graffiti of both sides, childish, cover every fence, every available space. When we walk back from our evening coffee at Contrescarpe we can't resist scribbling over the rightists' slogans. Thus "Vive Le Pen" becomes "Vive Le Penis!" But we worry about the future, even as we confidently sing "Fascism shall not pass!" People look at the empty sky, naively expecting it to fill up at any time with paratroopers from Algiers in full battle gear, red berets on their heads, machine guns at the ready.

### **Paris. 13 May 1958.**

The Prime Minister seems to have gained the upper hand: "It is an insult to suggest that I would permit Algeria to be lost," he says. "Algeria shall remain French."

In the meantime, back in Algiers, an anti-Government demonstration initially scheduled for the middle of the afternoon has been delayed by two hours to allow it to gain strength by merging with a rally planned for



the same evening. The general strike is beginning. French troops have been ordered to remain in their quarters.

10:10 p.m. The creation of a Public Salvation Committee has just been announced. Predictably, it is headed up by rightist General Massu. The French who live in Algiers are taking to the street to greet Soustelle, an ultra-conservative politician. The University is in such turmoil that normal studies are out of the question, many classes have been called off, others are constantly disrupted by demonstrations and political meetings.

### **Paris. 14 May 1958.**

It is 9:40 a.m. Telephone communications between Algiers and Paris have been cut off by the Government. Maritime shipping traffic is being detoured to Tunisia. Several people have been arrested in Paris. In the Latin Quarter the excitement I witness is unprecedented. There is an air of insurrection in every gathering.

Yesterday I joined a demonstration in the courtyard of the Sorbonne. Fine speeches were made, announcing great imminent movements on the part of "The People" and "The Masses." But it was hard to find two individuals in the crowd with the same interpretation of current events. Pflimlin, who is still in charge of the Government, has banned all political gatherings. De Gaulle is rumored to be in Paris, in an office located on rue de Solferino.

### **Paris. 15 May 1958.**

Like every French citizen I am staying close to the radio to follow events in Algiers minute by minute. The Soviet Union has launched its third *Sputnik*, but in the current political frenzy no one seems to care that a major new step in the conquest of space has just taken place. The satellite weighs a ton and a half, and the booster rocket is in orbit along with it.

General Salan has stunned the country by delivering a speech which ended with "Vive la France! Vive l'Algerie Francaise! Vive De Gaulle!" In Paris the government called this statement an "optical illusion." But De Gaulle has answered the call by announcing that he was indeed ready, "as in 1945, to assume the widest responsibilities." What we are seeing is the unfolding of an obvious conspiracy to bring the General back to

power, and to bury the Fourth Republic.

**Paris. 27 May 1958.**

It is 1:40 p.m. Planes carrying paratroopers and units of the Special Forces are said to have landed near Paris. The French fleet is in Algerian ports. De Gaulle is definitely in Paris. Pflimlin vanished during the night. Has President Coty met with De Gaulle? There is talk of an insurrection in the Southwest.

**Pontoise. 29 June 1958.**

I have learned that I flunked General Mathematics. This throws my life into further uncertainty. Yet passing this examination is a crucial requisite for me. Without it I can do nothing. All the recent political turmoil in Paris, the demonstrations, the strikes, have not helped my studies; neither has the life I have led, these last few weeks, with Claudine. She creates a feeling of impossible nightmare. As dawn arrives, a weak whitish daylight leaks into her room through the curtains. We wake up in the low bed. I lose all sense of time. Since the first day, there has been an invisible barrier between us.

It was on Monday evening that I found out that I had failed, after a huge scuffle to fight my way through the crowd of students and to get near the posted results. I spent Tuesday night with Claudine. This time I found her less tormented, more accessible. On the bedside table there was a love letter in fine handwriting, addressed to her: "My Darling Claudine ..." I did not read it, but I was indiscreet enough to glance at the signature: it was from another woman who lives in the Midi.

**Paris. 21 July 1958.**

Finally, De Gaulle is here. What seemed unthinkable has happened quite naturally, in spite of all the Leftists who were clamoring that his return would surely trigger an insurrection, a terrible civil war. In fact, after a few days of disorder, during which madness did rule and newspaper headlines became huge, all the political parties have simply resumed their old intrigues as if nothing had happened.

I saw Claudine on rue Monge.

"Do we say hello?" I asked her.

In response she simply gave me her hand.

"You would come and have a drink with me, if you were a true friend," I added.

We went to the corner bistro and we had a cup of coffee. We were very close again, very tender. The next morning we took the train and went to Pontoise, where I now spend every week-end visiting Maman. My brother was there; his children cheered up Claudine.

On Friday we had dinner with my friend Granville, who studies for a degree in pedagogy. I was rather somber at first, but I soon found it funny to watch our strange bohemian group. We looked like the survivors of a wreck, a band of drifters united by their uncertain destiny. Claudine was terribly out of place in her red party dress. Granville had plastered some sort of white powder over his face. I was wearing a dirty old jacket. I had been painting my walls all day, fixing up as best I could the little room into which I will soon be moving, at the other end of Paris. My fingers were still spotted with paint. To make things worse we decided to eat at a fancy Chinese restaurant, where the waiters looked down on us in disapproval. Yet I felt this pantomime was a fitting way to bring to a close my two years of wandering in the old Latin Quarter, two years devoted more to the vibrant streets than to serious study on the hard benches of the Sorbonne.

When we got out of the restaurant we danced on the sidewalk like three idiots, not caring about tomorrow. Yet later, on the Metro, Claudine held my hand in a strange, serious, almost desperate way.

Paris. 7 August 1958.

My friend Marcel was right the other day when he asked me: "Why is it so damned important for you to study science?" He was right, but only in asking the question. It would be a drastic limitation to dedicate myself exclusively to the study of science, like a priest dedicating himself to God. I will indeed study science, but I will do it with the knowledge that an appreciation for art, fantasy and sensitivity is not a "negative trait" that I ought to suppress within myself.

During my first year at the Sorbonne I was frequently discussing these lofty topics with a girl who had befriended me. One day she brought a small package: "This is for you," she said. "It was among some books my grandmother left when she died. I think you should have it." It was *Histoire et Doctrines des Rose+Croix*, by Sedir (1932). I lost sight of the girl,

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but I have treasured the book ever since, and it is with me now, a source of inspiration and a tangible link to the deeper questions I long to explore.

I want to look behind the scenes of our human existence. Unfortunately I have found no one who is able to answer my questions about forbidden things: What is research? Does it consist simply in tiring our minds while looking for impossible solutions? Could one find the ultimate secret by simply giving up the search, satiated with the pointless, superficial agitation of life, and looking instead at the infinite void beyond it?

When we discuss love, sex and destiny Claudine cautions me: "You're only nineteen. At twenty-two you will run the risk of discovering that you have already known what most men only experience at thirty or even, for most of them, never."

Funny how she still uses the formal *vous* with me. Perhaps it is true that I have been here, inside this particular body, for nineteen years. But in reality I feel that I have always existed. My brother is a **hard-boiled** physician, an agnostic and a cynic. But for his attitude towards life to be justified, the ancestral terrors I hear blowing through my soul would have to stop, the universe would have to become limited, time finite. Everything would have to die and go away.

Paris. 8 August 1958.

Who will tell me what death is?

My father has ceased to think, to hear and to see. In the last few hours before he died he thought he heard music. He asked my mother if it was a piece by Bach playing on the radio. But do I feel any call from him? No: nothing but the whispers of eternity itself, which I cannot hush within me. Night beckons to me in a similar way, can I deny it? I can almost hear the night, falling in fine drops around me, when I am holding Claudine's sleeping body against mine; and the starry night calls out to me too, a living mystery filled with other worlds. What is this attraction of nothingness we feel, beyond the fabulous amount of matter radiating in the dark sky? So much substance, metals, energy, explosions, just to create the tiny point of a star in my eye! Nature is multiplying these orgies of time and distance beyond the understanding of my poor human spirit, and to prove what? The existence of Nothingness?

Claudine, I sought the answer in your own life, in a tenderness that re-

mained beyond reach. This strange privilege you afforded me, giving me your body without letting me touch your soul. . . . What were you afraid of?

## 2

### Paris. 25 August 1958.

Now I have my own room in Paris, close to Porte Champerret. It is one of the small rooms on the seventh floor of the building, under the roof, which in more elegant times used to be allocated to the chambermaids of the bourgeoisie. The elevator only reaches to the sixth floor, then I walk up one more level up the servants' stairs. This is a tiny place, barely ten feet long and seven feet wide, into which the slope of the roof cuts an angle. But it is mine. I am in bed at last, lying under a blue blanket. It is 10:20 p.m.

At first it was nothing but a dusty mess, to be truthful, my little *mansarde*. I was thrilled two months ago when Claudine told me it was available, because I have no money and I certainly cannot afford an apartment or even a studio. I have made some improvements: a few electrical connections, a movable lamp. I cleaned up the floor, I installed a small water tank above a wash basin (there is no running water, no sewer: I carry the waste water and empty it in the lavatory down the hall). I put up shelves for my books. I nailed a piece of plywood to the wall and I painted it black to serve as a chalkboard for math problems.

This part of the city was unfamiliar to me, but it is now coming alive through many tiny scenes, as I wait for the 84 bus every day, or as I take my breakfast at the *Cafe des Sports*.

On Wednesday I found a letter from Claudine, so unsure of herself. So direct: "*Pas ma fete a moi.*" Not my day. Write to me, she was asking.

Is there another level of life and awareness? I have long been aware that I could pass almost at will from the plane of normal consciousness to • . . another plane. There are dozens of examples: all those circumstances when something like an electronic relay suddenly seems to close deep inside me, when time starts flowing at a different speed, when new angles

of reality are revealed. My strengths become more clear then, my body goes on automatic. The spirit flies off.

Whoever possesses this "other kind" of thought recognizes it at once. It comes with the feeling that we do not really "exist" any more in this world than a single note in a symphony exists, or a single spark in the fireplace. We are both creators and tributaries of the universe we perceive.

A chance meeting I recently had with one of my neighbors, a strange mystical man from the Middle East with an advanced degree in engineering and a passion for ancient texts, makes me experience once again this unusual ability of my mind. He noticed the urgency I was putting into my work. He told me: "You seek to create in order to fulfill something within yourself. That's absurd, my friend. What is zero plus zero? Instead you should create through the mere desire to create: inspiration pure and simple. Never look at your own work. If you want to be a master some day you must find pleasure in creating without having a precise objective, without pursuing a rational goal."

**Paris. 27 August 1958.**

I am now reading a book called *Mysterieux Objets Célestes*,<sup>2</sup> which is challenging the very depths of my mind. It was while browsing at the *Bazar de l'Hotel de Ville* department store that the title caught my eye. I grabbed it immediately. At last, an intelligent book about flying saucers! Yet I suppose that for those who are rooted in the ordinary world, it does not matter if a few researchers have found that the immense contour of other shapes, other civilizations, could be discerned beyond our world.

Will these strange events begin again soon? Deep within myself *I passionately want them to wait for me*, and to find me established in my future life as a researcher. This is an ironic thought, knowing as I do that I will probably die without seeing any solution to this immense problem, or without being able to contribute to it.

On a more finite level I have a new girlfriend named Juliette. Something tells me that some serious developments could take place between us. Claudine has awakened my instincts in this domain where I was blind, deaf and mute. Yet there has been nothing said between Juliette and me, not even a hint of a flirting gesture. Only the atmosphere getting heavier.

**Paris. 1 September 1958.**

My interest in "flying saucers" goes back to the Fall of 1954 when there was a deluge of sightings in France, and indeed throughout Europe, from England to Italy. Every day the front page of all the newspapers, from *L'Aurore* to *France-Soir*, carried big headlines and surprising claims which the radio amplified with commentaries and on-the-air interviews. My father, a respected magistrate, a former investigative Judge who had been promoted to Paris as a Justice of the Court of Appeals, would scoff at such reports: in his profession, he pointed out, he had become leery of the weakness of human testimony. Especially that of experts.

As a kid I remember hearing one of the earliest French witnesses, a railroad worker named Marius Dewilde, telling his story to radio broadcaster Jean Nohain in a live interview on the evening news: "I had gone out to piss ..." he bluntly told the whole nation. He had seen two little robots next to a dark machine resting on the nearby railroad tracks. The air police found traces of a large mass. A strange ray issued from the object and paralyzed Dewilde. I believed his story at the time. I still do. During the three months the wave lasted I carefully gathered such clippings and glued them into a fat copybook.

It was during the following year, a Sunday in May 1955, that I observed a flying saucer over Pontoise.

My mother saw it first. She had been working in the yard, pulling weeds and caring for her flowers. She was getting ready to put her tools away to prepare the afternoon coffee, a sacred tradition in our family. She had to scream to get our attention, because my father and I were up in the attic, where he had his woodworking room. He was busy and did not consider such an event significant enough for him to come down. I rushed to a window that had a Southern exposure but could see nothing. I ran down three flights of stairs into the yard to join my mother, and then I did see it.

What I observed was a gray, metallic disk with a clear bubble on top. It was about the apparent size of the moon and it hovered silently in the sky above the church of Saint-Maclou. I have no recollection of seeing it go away. My mother says it flew off, leaving a few puffs of white substance behind. Remembering the war years, she first thought they were parachutes.

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I was left with the single strong impression that *we must respond*; that human dignity demanded an answer, even if it was only a symbolic acknowledgment of our lack of understanding. I realized then and there that I would forever be ashamed of the human race if we simply ignored "their" presence.

The next day I met with my closest friend Philippe at the College, where we were "cramming" for the Baccalaureate examinations. He mentioned seeing the same strange object from his house, half a mile North of my position, on higher ground. He had watched it through binoculars, and confirmed my description.

My father was sternly opposed to our making any kind of report. The family of a distinguished judge does not get his name into the papers with some flying saucer story. What we had seen must be some kind of new aircraft, he insisted, something explainable. I convinced myself that he must be right.

Now Aime Michel has reopened the whole question: studying all the sightings of 1954, he has found that they fell along straight alignments that criss-cross the French territory. He calls this pattern "orthoteny," from Greek words that mean "drawn along a straight line."

Paris. 9 September 1958.

I have written to Aime Michel. My letter begins:

I have just put down your book, and this is far from a gratuitous act\_\_\_\_On every fundamental point you bring reason where the best people who came before you gave us nothing but a multiplicity of excuses.

It is only with a few of his conclusions that I argue, when he despairs of our position with respect to the beings who control the objects. I find two arguments against this despair:

1. Faced with "orthoteny" (the fact that saucer sightings seem to occur along straight lines), you compare us to an eight-year-old boy standing before Einstein's blackboard. Yet the boy, when he grows up to be thirty and is educated in math and physics, may be an even greater genius\_\_\_\_How can we believe that beings with the degree of evolution we can reasonably ascribe to "them" would not have methods of education superior to ours?



Of course, in order to educate us, they would have to find us worthy of a dialogue with them. When we probe their behavior, what do we find? The gap between their knowledge and ours does not appear to be so enormous. And they seem to reason along a set of concepts analogous to ours.

2. If we believe the flying saucer witnesses who speak of seeing small hairy beings, we should also believe them when they claim to have seen these same beings along with others who were morphologically human. This implies a similarity of level between us and them. There are indeed differences, but mere "differences" can be bridged. I am only a math student, and my nineteen years do not give me that right to prophecy that some scientists are so quick to claim for themselves. But it seems to me that if we extrapolate our civilization by fifty or a hundred years, we could well find round flying machines in our own future, as well as excursions beyond the solar system.

I close my long letter by thanking Aime Michel for writing the book: "It gives us a reason to face the problem. It enables us to begin valuable research quietly. Serious work can start at last, because of you."

**Paris. 13 September 1958.**

How should one speak of a night of love? What is the use of words beyond meaning? I only want silence, warm lingering rest. My room has lost its arid, awkward face. Upon waking up, next to Juliette's long black hair curling up over the blanket, I cast an ironic eye on the word "*Ascese*," asceticism, which I had painted on a curtain in purer, lonelier days. This bed lost all shape last night, this narrow single bed torn away by passion. Why should I describe our trust on paper, when I can still taste it on my lips?

**Paris. 16 September 1958.**

There are diverse ways towards life. I need to find one, and I need new ground on which to build, to open new roads: I must give free rein to a new intelligence within myself. I am not speaking so much of building my own life as of achieving the final destruction of the lives of others within myself. I long for the end of adolescence, that worthless tumult.

How lonely I have been all these years!

**Paris. 21 September 1958.**

Aime Michel has answered me. He thinks that I underestimate the problem of communication between us and X:

My book gives a misleading feeling of simplicity because of the restrictions I imposed on myself. There are some sightings, as credible as those I have quoted, where the witnesses saw the object disappear instantaneously without any spatial displacement. There are others where a solid geometric object changed shape in a fraction of a second. Can you imagine a pyramid turning into a cube? Remember what Poincare said about the fundamental importance of solid bodies in our logic.

He points out that there are some domains in which no one will ever do better than man, not even God if He exists: Mozart's oboe and clarinet concerto, for instance. "There are human absolutes," he says.

By the way, allow someone who was your age when you were born to give you some advice: you have a remarkably gifted mind. Do not let yourself get abused by the idea of "getting to the bottom of things," which is only a mirage. Cultivate your mind like a flower but be careful: the pavot is a flower.

**Pontoise. 12 October 1958.**

The qualifying examinations come two days from now. I have another shot at General Mathematics. When I read all this again later, I am not sure I will remember my obscure battle against the wind and the mud, the stupid fight in which I am now engaged as I try to get out of the quicksand of these studies.

Today I have given myself solemn instructions for the creation of a new being. In two days I will go to this exam, this last fight. Do not be concerned with it. Born from me, leave me quickly. Bury me deep within the memory of Granville, Claudine and my other friends. I will be at ease there. Be free and go, as a little Sphinx who already bites and flies. I do not understand your enigma, but I believe in an escape towards the new dimension you represent.

**Paris. 17 October 1958.**

I took the General Mathematics examinations on Tuesday and Wednesday. I spent both nights in chaste, peaceful sleep near my "sister" Claudine. Everyone says that the examination was tougher than at the June session, when I failed.

**Pontoise. 27 October 1958.**

Success! I have passed the test. Egotistically, I savor this victory. I feel that I now belong to a new world, and I am proud of it. It is the same happiness I experience when I am patiently scanning the craters of the moon, or watching whirling counters in the physics lab and when I think of all the people whose lives are confined to the weekly movie, the soccer game and the nearest bistro. My inner happiness doesn't come from being different from them, and I certainly do not feel superior to them. But I am proud to have gained a wider vantage point on the world.

**Paris. 12 November 1958.**

My next goal: a bachelor's degree in science. How can I describe our crowded lecture rooms at the Sorbonne? Four hundred seats and eight hundred students, people sitting on the ledge of the windows, on the stairs? How can I describe this wretched French University system, against the backdrop of our continuing colonial wars which consume most of the available funds the government should be putting into education and the modernization of this old country?

Our generation will have to re-invent everything. Centuries of civilization and philosophy seem of little help here. Contemporary artists from Varese to Pierre Schaeffer and from Dali to Miro have already destroyed the old standards and the old morality, bringing the blast of their dynamite all the way into the exploded language, freeing up design and painting, yet science still follows the ancient models. It too will have to be shaken up. Then everything will have to be rebuilt within a society that doesn't provide us with any useful models.

**Paris. 13 November 1958.**

A kind of quiet harmony is spreading around me. Perhaps it comes with the fog over Paris, which drowns the trees and the car headlights; or

with the emotion in a friend's handshake in a Paris cafe, or the fall of dead leaves swinging all the way down to the wet pavement like a jazz melody. Perhaps it comes with the words of scientific reality exchanged among young men in impeccable white coats in the corridors of the Radium Institute, where I now attend some of the lectures.

There are many lessons to be drawn from ancient Magic. For we are still in the Dark Ages: Consider our churches, our Lords! Look at the serfs here, the Baroness passing by in her beautiful coach, our men-at-arms swinging their sticks! See our fortresses, our quaint coins, our narrow minds! See the little compartments of our science humbly growing in the midst of public indifference. The only new fact is the uncontrolled use of this science by the government and the military. The wise *men* of the Middle Ages, at least, knew how to hide their discoveries behind obscure Latin paragraphs. If necessary they took them into their graves.

I spent a long time talking to Claudine the other evening, in an ugly bistro on rue Saint Jacques. The place looked like the inside of a submarine. But we were warmly squeezed against each other, like good close friends.

### **Paris. 22 November 1958.**

The little *cafe* is very poorly lit. We have made it our headquarters because we are used to the fare. We bring along our mistresses, the girls bring their lovers on the back seat of little Italian motorcycles called "scooters." We are these peculiar, ~~privileged~~ creatures, *etudiants*. We have friends who arrive from Japan or China. They speak slowly, with the peculiar tone of voice that becomes those who have travelled far in spite of themselves, and have seen much. They play absent-mindedly with the match-boxes left on the table, they drain their cup of coffee, and go back to the Sorbonne to apply for another travel grant. There is nothing here of the intense discussions I used to have with Granville and with Marcel, from which arose something mystical. Instead we confront serious, rational ideas. Coffee and conversation are thick with the dust of learning.

Occasionally I drag Claudine here, literally, by the hood of her white and blue coat. She is older than I am. She laughs at being treated like a kid. I write lyrical things, strange poems. People tell me I'm young, with the tone of an insult. Since I am always hungry (at the student restaurant they serve us pure shit), I stay here to fool my stomach with coffee and to

work on topology problems. I draw funny shapes on ashtrays. My heart isn't in all this. I am growing tired of all the silliness of Paris.

**Paris. 5 December 1958.**

Instead of promoting *mass* communications *one* should isolate each man, isolate him inside himself, in order to build up his spiritual life. How useless, stale and empty is the intellectual life of this famous Rive Gauche! How flat are the sex stories, how uniform are all these "original" people, gossiping about the obsolete Absolute!

**Pontoise. 13 December 1958.**

Since the death of my father almost a year ago, Maman has been living in this large house on Saint-Jean street. Her neighbors are provincial *bourgeoises* who share nothing of her enthusiasm for space exploration. The other day she heard on the radio that a team of English astronomers had bounced a signal off the moon. "Hello!" They said. "Hello!" answered the lunar surface a few seconds later. When Maman told her neighbor she had heard the exchange the lady looked at her skeptically: "My goodness," she said, "you must be spending all your time at that window!"

**Pontoise. 14 December 1958.**

Slowly, I am beginning *to* understand the feelings of people, I appreciate better their complexity. Could I have been touched? No, who could be touching me? Juliette has disappointed me, and Claudine is "just a friend." But I am beginning to understand those who love and to realize the complexity of the relationship between spiritual and physical pleasures. I seek the *terrestrial* foods, without flaw or complication or pretense.

**Paris. 16 December 1958.**

Now I am fed up with our little group which always meets at the same *cafe* near Port-Royal. Fed up with the people themselves, their humorless lives, their habit of talking forever about the same meaningless details of their petty lives. I want to move away.

Every chance I get I rush to a little open-air bookshop on the Boulevard des Italiens which sells used science fiction. I devour everything, from Van Vogt to Heinlein and from Jimmy Guieu to Asimov.

**Paris. 22 December 1958.**

Everything seems to confirm a single observation: we are living fake lives, absurd lives in today's cities. Nothing actually exists of these so-called "acts" and "opinions" of ours. Truly important decisions are made beyond our observation, beyond the control of ordinary citizens. Everything we see is fake, a stage drowned in movie fog. We come and go like puppets in search of their own strings.

I long to send this message to a wiser man somewhere in time, far away: "You should know that down here we are managed, surveyed, and classified like insects by police and publicity men, or simply by the mechanical stupidity of our own bureaucracy."

Slowly, revolt after revolt, torture after torture, this earth will eventually emerge into its true history. In the meantime I am eager to learn what is outside all these events, I want to see the mechanism beyond time itself.

**Paris. 6 January 1959.**

Juliette wrote to me today: "Do not wait for me tonight, or tomorrow, or ever. It is too hard for me to start again, to rebuild something."

I felt deeply hurt. Everything seemed to be collapsing. But the storm has now swept the sky clean.

# 3

**Paris. 10 February 1959.**

A proposal: To go straight ahead, wisely and quietly, without jealousy or hate ... To walk through one's life in long equal steps. To put everything we are, especially our love, into our gestures.

At night I try unsuccessfully to travel in spirit through the whole night of Paris: I am quickly brought back to reality when a ten-ton truck rumbles down the canyon at the base of this huge building; in the next block a tall chimney throws up torrents of black smoke; hideous yellow dogs, taking hideous old yellow ladies on a routine walk at the end of a leash, piss all over my scooter parked on the sidewalk ... I have to pull my

thoughts away from these sordid scenes.

I drape the covers over the shoulders of the girl sleeping next to me. We commune in warmth and tenderness. Some day we will leave this city for a place where we will no longer be cut off from nature.

Janine is a schoolteacher from Normandy studying for a Master's degree in child psychology under Professor Piaget. She has moved into the room next to mine, a pretty brunette with green laughing eyes. For some reason she thought I worked as a photographer. We happen to own the same records, easily heard through the thin wall at night. We made love for the first time a week ago, and we have been together since.

**Paris. 28 February 1959.**

Without forethought I have started to write a novel I call *The Praxiteles Network*. It has to do with the adventures of a group of kids amidst the ruins left by war. The idea came over dinner with Granville, who told me of "something silly he was writing for a publisher, hoping to make a few francs." I decided to do the same for fun, without any plan. I am letting the story develop. A year ago I found it hard and painful to write. I am surprised to see how much easier the process is becoming.

The other day I found a note from Janine under my door:

I went away from your room utterly distressed, probably because what you say resonates deeply within me. You will get through because you see things, not in terms of yourself but in a detached, impersonal way. I have not reached that point yet. I feel I will only be able to achieve this after resolving some conflicts that I do not master, because I don't know where they come from.

**Paris. 2 March 1959.**

I am in love with her. I was speaking of a high point, of new horizons, yet I wasn't even able to see the landscape. Now I feel like a pilot in flight who suddenly breaks through the clouds and watches the sunshine illuminating some wonderful island below....

we are crazy. We are two crazy lovers. Janine has set a new machine into motion within me. She is holding my life on the highest wing of the storm.

I have finished and set aside the manuscript of *The Praxiteles Net-*

*work*. I have begun with even greater enthusiasm a science-fiction novel called *Sub-Space*.<sup>3</sup> I condense my current life within it: in the middle of a big stupid city there are people who love and search; they are forced to go beyond the limits of the world both outside and within themselves. It's a fun process, because the story writes itself in fury and disorder, carrying me at a gallop pace. When I see a new protagonist, he often moves without warning out of the context I had prepared for him. I am just as unable to say what my character Alexis Nivgorod will do in fifteen pages as what I will do in thirty years.

Janine poses a deep question to me. She is carrying within herself a powerful secret anguish. Our love goes faster than light.

At night the glow of the record player scatters iridescent droplets all around my room, and over the night shirt of simple white fabric she has dropped on the dark red carpet.

### **Paris. 13 April 1959.**

The weather has turned hot and heavy. I have to stay in Paris to study in the midst of mirages. The random notes I write down in these pages are only useful because they provide me with a standard, a reference point among all the illusions.

It is so hot today that the asphalt melts, sticking to the feet and to the mind like caramel candy. The ugly buildings, with their rococo style, seem to crush our lives under the weight of their dripping ornaments. Dust is flying, soft and sour, over Place du Chatelet. Tourists stare at the column through their binoculars. Who needs science fiction? No telepathic Martian with green tentacles will ever be more weird than they are. This city is only livable when you walk along with blinders on, going about your own business. You pay a high price for trying to get out of the maze, to think different thoughts, to discover an alternative to common customs.

Within French society are lines of equi-stupidity which cannot be crossed without much pain and a huge energy quantum. This is an atrocious, absurd, unjust system.

In the meantime I am within it, whether I like it or not. I have no choice but to get a bachelor's degree, sitting on the benches of the Sorbonne next to a few battalions of armored girls from the Catholic Center and a bunch of stubborn, narrow-minded fellows whose sole ambition is to graduate quickly to earn more money and buy bigger cars. Fortu-



nately I have the greatest teachers I could hope for: the whole *Bourbaki* school of mathematics<sup>4</sup> has come back to Paris now that the old guard of French academics has died off. Thus I study under Godement in Analysis, Chevalley in Set Theory; a team of internationally known mathematicians has decided to train the mass of the younger students like me rather than concentrating on a small group of higher graduates. They are an exciting Faculty, but I am tired: I haven't had a vacation in three years.

**Pontoise. 25 April 1959.**

I just saw a movie about volcanoes directed by Tazieff. It contains a strong, appealing idea: No point on earth is immune to a sudden eruption. In space the most ambitious realizations of Man only rest on a thin layer of the planet; in time they can be encompassed within the first page of the topmost book in a pile of volumes as tall as the Eiffel Tower. I do like this idea. It satisfies me to think that the Arc de Triomphe, for instance, that "bearer of eternal symbols" of military glory and horrible death is actually resting on the original boiling magma of the planet which makes a mockery of this exalted hoax. Thought and sex are the only human activities which are not totally ridiculous. As soon as man makes a gesture which is not intended for love or for discovery he is nothing but a dirty little beast, a swindle, a pest unto the universe.

**Pontoise. 1 May 1959.**

Fantasy alone is what should drive me forward. It is a tumultuous torrent, but my boat is sturdy enough not to capsize within it.

I had forgotten what Spring could be in blessed Ile-de-France, with these towering branches in bloom, these multiple levels of sumptuous colors in the leaves around us, and the majesty of the huge pine tree in the neighbor's yard.

There is a song by Jacques Douai:

Que sont mes amis devenus  
Que j'avais de si pres tenus,  
Et tant aimes?

*(What has become of my friends,  
Those I loved so dearly,  
And held so close?)*

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My friends have melted away in the breeze, in the mud of the fields, the sand of holiday beaches.

### **Pontoise. 13 May 1959.**

At twenty years of age my contemporaries are attracted by powerful myths: the myth of intellectual comfort, of material riches, of a "career path" to success and respectability among the bourgeois, leading to quiet retirement. At forty their minds are sclerosed. Rare are those who keep a strong spirit till the end. I think of my father as an exception. His spirit came through in the way he would open a book by Barbey d'Aurevilly, the way he taught me the difference between oak leaves and aspen leaves as we walked through the woods.

It was he who showed me how to make a slingshot, bows and arrows out of branches of hazelnut tree, whistles out of reeds. He had grown up between Caen and Cerences. He knew all the tricks of the clever Norman farmers.

Once he took me above the Pontoise railroad station, along the road to Rouen, to show me some large flat stones buried in the fields. They were the remains of an older highway: the road built by Julius Caesar when he crossed the Oise on his way to Great Britain, nearly two thousand years ago. From these trips with him I learned about the depth of time and the widely scattered wonders covered with moss and weeds, just waiting to be discovered and pulled out.

The changes of the post-war period took my father by surprise. He was shocked by the selfishness of politicians, the brash explosion of the media. He regarded the new movies as a social evil. French society was shaking free of its older models: the songs became irreverent, the moral references imposed by the established order collapsed. A free-thinker, a fervently independent mind, deeply devoted to the ideals of the Republic (a framed *Declaration of the Rights of Man* was hanging on the wall behind his desk), he was not part of the traditional Old France of religion and wealth. Yet when I confronted him about the stupidity of the Algerian war, as my brother before me had confronted him about France's attempts to keep its colonies in Indochina, he sought refuge in simple-minded slogans ("My country is never wrong") which infuriated me.

It is very important to refuse to take any of the predetermined paths society offers us. There are no predetermined paths in nature, only rela-

tivity of directions and goals. True initiation deals with the Whole, and with love.

What is Destiny? Are some individuals just carried along by events, while others spin around in narrow circles, unable to find a solution to the simplest problems? Events do happen at the right time if one knows how to place himself at the spot where their greatest probability of happening lies. Destiny might simply be a measure of this ability.

### **Paris. 5 September 1959.**

These pages are nothing but a schoolboy's notebook, in the strange classroom we call life.

Over the bridge at Bezons, the huge deserted bridge, twilight made the sky gray and mauve as I was riding through tonight on my way home. The wide road swept up and I could see nothing beyond it, only the tall street lights flooding the wide pool of asphalt, thin lines of sodium over the moribund Summer. Suddenly this huge bridge appeared to me as a fine and rich thing, a novel image of beauty. I felt the approach of Winter. Under the rain or under the nourishing fog the most ordinary objects suddenly seem able to meditate and resonate beyond our wildest thoughts.

Life in my little room is reduced to bare threads. I have passed a new batch of examinations. I will gladly walk away from the Sorbonne, clutching my science diploma. Janine only comes to see me in the evening, when she mends my unstable existence. She has completed her Master's degree in psychology, and the Administration has reassigned her to a school in Amiens. Emotionally, we are both ready to leave Paris behind. Already, she has moved her things out of her own room.

My thoughts are already shifting to the flat landscapes of the North of France, to the city of Lille where we have already made a few quick explorations together. Lille is not too far from Amiens. Janine will be able to come and visit me often. I relish the idea of moving to a city where I do not know anyone, where nobody expects me to be. I like the sad, gray, quiet suburbs of Lille and its small, one-dome observatory where I will be studying towards a Master's degree next year. In Paris they teach astronomy without ever showing you the sky. The lecture halls are crowded with hundreds of students who have no interest in the subject but need the credits.

I was created in the form of a man. This is supposed to be obvious: "I

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am a man." Yet there is an infinite distance between "me" and "the man I am." We can only shape our life through the control of everyday acts, with that fine knowledge of the structure of destiny which is provided by the constant proximity and vibrant awareness of death.

Science, too, is supposed to be obvious. But it is nature which is important, not science. Physics is nothing but a user's manual, a cookbook based on a narrow conventional language. Physics is a confession of weakness. I can only believe in simple, beautiful things. Why do they show us science as such a complicated structure? There is nothing very complicated in the world, only states of mind which get in the way and complicate simple things.

### **Paris. 24 September 1959.**

Today is my twentieth birthday.  
I am only myself when I am with you.

### **Paris. 8 October 1959.**

Alone in my room, I wait for Janine. Winter is coming, my winter. This city is moribund, except for two movies by Ingmar Bergman that have just been released. It is odd, how his songs of death are supplying the only spark of life in these dull gray buildings, these idle masses of stone.

# 4

### **Lille. 24 October 1959.**

Childhood: the spring itself is forgotten when we can see the river rolling along. Yet it is the same water, in color and in taste. The memories of man, as far back as he goes, are lost in question marks. And the very source of being, one's origin, remains as exciting a mystery as the state that follows death.

The records of our ancient dreams are the most fascinating of all bedside books. They paste an ironic smile over today's freshly hatched plans.

I was born in an interesting year, 1939, a point of low birth rate for the French nation because of the combined effects of brewing international

tension and the lack of marriageable males, a long-term consequence of the massive killings of the First World War. It was in 1939 that the first digital computer was demonstrated,<sup>5</sup> and that Roosevelt learned from Einstein's secret letter that an atom bomb could be designed and built.

Even the timing of my birth, on 24 September, was poorly chosen. The Second World War had been declared just three weeks previously. German planes were preparing future campaigns by executing bombing raids against strategic objectives. Pontoise was high on the list. It had both a highway bridge and a railroad bridge, and held the key to Normandy. The Luftwaffe was pounding away at the little town. The midwife, who lived on the other side of the river, was unable to come. It was the doctor who delivered me amidst the sound of the first air strike. My father was fifty-five, my mother was thirty-nine, and I had a brother who was finishing High School.

A few short months, and the invasion of Hitler's Panzers came from the East and the North. A great panic swept the French population into a mad exodus. My parents left Pontoise on 10 June 1940 to seek refuge among our cousins in the safer province of Normandy, where they spent several months. I naturally have no memory of it. One day a former neighbor who was passing through Normandy told my parents what had happened to the fourth-floor apartment where they lived, and where I had been born: the Germans had entered Pontoise on June 11th. The day had been marked by numerous incidents. It was alleged that a sniper had hidden himself in the attic of our building. Wehrmacht soldiers rushed in and threw incendiary devices into every apartment, then they just watched the whole structure turn into ashes. The fire destroyed my parents' small treasure: a few pieces of furniture, many books; but we had escaped with our lives. Amidst the great tragedy of Europe that was known as being lucky.

When they returned to Pontoise they rented a small house on the hill that overlooked the river, a rocky escarpment where medieval walls used to defy the invasions. A little knowledge of history would have discouraged such a move. The medieval fortified site of Pontoise was the birthplace of alchemist Nicolas Flamel. It was once so prominent and wealthy under the banners of Saint Louis, the White Queen and Philippe-Auguste that its cathedral was larger than Notre-Dame in Paris, but it was consistently attacked throughout the centuries because of its key

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position on the river. Every invader since Julius Caesar had crossed the Oise at that spot. The British and the Americans now needed to destroy the bridge to cut off Hitler's forces from their reinforcements. They bombed the river, reducing many houses in the vicinity, including the one we rented, to mere dust.

I remember my mother picking me up in her arms and walking through the rubble. I stared at a door frame still standing amidst the destruction: it was all that was left of our previous home. Fortunately, just the day before, we had been evicted by Anna, our greedy landlady who was hoping to rent her house more profitably to some German officers. In fact that scene, too, has remained as one of my earliest memories, my mother holding me with one arm while striking Anna with her kitchen rag in utter frustration.

This time my parents wisely moved farther away from the river, to the house on Saint-Jean street.

One evening the *Resistance* blew up the switches of the railroad station. We went up to the attic to watch the drama unfolding. My father, who had been at Verdun in the previous war, had a very sure sense of danger. He also knew that our cellar afforded no realistic shelter and that we were better off watching the battle. For a five-year-old child the spectacle of war was a fantastic game, a splendid education in the unreliability of the world. I remember the rails being thrown up in the air like matchsticks. In later weeks and months waves of airplanes came from the West, flying towards the Ruhr in triangular formation. I watched aerial dogfights in which wings were torn off. German batteries would fire pitilessly at the bodies of helpless Allied pilots swinging down from the bright blue sky at the end of their white parachutes. Every day in the garden we gathered tinsel and radar-fooling "chaff." Soon came the mighty rumble of Patton's tanks, behind which marched tall laughing Americans with chewing gum in their mouths and nets over their helmets. Interminable truck convoys took over the main roads.

Slowly my parents resumed their existence, watching every franc and every sou. In my father's papers, under the heading "rebuilding," I have found an official document dated from 1953, eight years after the end of the war. It read:

The undersigned expert went to the new domicile of the person who has suffered the loss. He observed that no professional effects were in existence, except for a few books. The furniture currently used by this individual and his wife is old furniture coming from his mother's home.

My father never owned a house, a car or even a telephone.

### **Lille. 27 October 1959.**

I am early for a physics class. Sitting in my car near the University I can see the wet cobblestones, the kids in their hooded coats fighting against the wind that shakes my old Renault 4CV with violent blows. Behind me the storm sweeps the Square Philippe Lebon in the gray morning. It picks up dead leaves and sends them flying clockwise around the statue. A shiny black car slowly drives by. The driver lowers his window and calls out to his girlfriend on the sidewalk. She stops, turns around; I see her eyes moist and wrinkled with stormy rain. She laughs as she joins him. I cannot hear what she says. They are lost down the street, absorbed into the mystery that lies beyond the corner where an orange light continues to blink stubbornly.

The rain batters the roof of my shelter. The howling wind blows, as it did last night in the fireplace of the room I am renting in a drab suburb South of the city. Girl students walk close to the walls like ghosts in their white raincoats. They look like the Touaregs of *Morocco*, their faces hidden by scarves and high collars.

Here in Lille abstract things seem easier to grasp. Abstraction lies in wait within every object and every gesture; every wall seems to contain an idea, fine and straight as a javelin, luminous and precise as the spot of an oscilloscope.

Aime Michel writes to me:

Any progress of the mind consists in gradually stripping away the preconceived ideas, the systems you have inherited. You are right to stir all that up. But do not expect to find the idea that will reassure you, "the Truth" if you will. Above all, Truth means understanding why we don't understand. Wisdom is to be able to measure what is certain and what is uncertain in science, a feat most so-called "scientists" are incapable of accomplishing.

**Lille. 21 March 1960.**

This evening we opened the dome of the observatory at 8:30 p.m. and we took three spectra of Arcturus (each a twenty-minute exposure) with the refracting telescope, a fine old instrument twenty-one feet long. We worked until 3 a.m.

**Lille. 15 April 1960.**

My first job: I have been making a little money by doing lengthy hand calculations of the integrated energy of eleven open "galactic" clusters, among them the Pleiades, Praesepe, Hyades and Coma for a research project headed up by Professor Kourganoff. I also compute the integrated color. Each calculation represents a long set of operations with a Marchant tabulating machine.

Although Lille University is one of the first campuses in France where programming of electronic computers is taught, we do not have any means of computation at the Observatory. Everything is still done by hand. Even the decision to teach about the new technology has caused something of a scandal: there was no one on the Faculty with any experience in programming, so we have to take a course which is taught (and taught quite well) by a local IBM engineer. To save face the University has asked a prestigious academician, Professor Kampe de Fériet, to come from Paris twice a week to lecture on information theory. This gives the course on computers some semblance of respectability in this traditional, conservative institution\_\_\_\_

**Lille. 26 April 1960.**

One o'clock in the morning. From my little whirring car I step out into the silence, cross the deserted street to my room. I will sleep like an animal. Tomorrow I must work on the math problem with a friend.

Last Sunday I went to Paris and by chance I met Granville. Has he changed? Not at all. He is so identical to the image of him I have kept that he is able to say:

"You see, I have been waiting for you, on this park bench, for the last three years\_\_\_\_"



**Lille. 8 June 1960.**

The crowd moves ahead like a mechanical storm, tough and swirling, heavy with obscure prophecies. We follow it, walking down the wide streets. We pass houses where landladies sit, knitting in the moist darkness of their hallways like spiders in their holes. The new sun is crushing the city. Men in shirt sleeves are taking the bus.

**Lille. 10 August 1960.**

*Some months ago I sent* an application to the Rosicrucian Order,<sup>6</sup> whose French branch has its headquarters in Villeneuve-Saint-Georges. Now I find their documents to be an interesting spiritual complement to my scientific training. Every month I receive a set of course material through the mail. It includes both theoretical reading and instructions for simple rituals, promising insight into higher realities.

**Lille. 19 October 1960.**

Janine and I were married today at the Lille City Hall. We had made arrangements with a friend, a research associate at the Observatory, to be our witness, but the law required a second one. On the sidewalk before the University entrance I found one of my classmates:

"How would you like to be a witness at a wedding this morning?" I asked.

"Whose wedding is it?"

"Mine!"

He laughed and looked at me with the indulgence reserved for lovers and other simple-minded folk.

"When is it?"

"In half an hour!"

We were married at 10:30. It was a very relaxed and informal affair. It isn't the external trappings that count, or how much one spends, but what we feel inside. We took our witnesses to the local student bistro for a couple of beers before returning to class.

**Lille. 12 February 1961.**

I've written a letter to Georges Gallet, the editor of the science-fiction collection for Hachette, the large Paris publisher to whom I have sub-

mitted my manuscript of *Sub-Space*:

My protagonists are simple researchers coming out of the crowd under pressure from fantastic events. They are "awakened" to the absurdity of the world in which they have lived until then. They have to conquer it, not only outside but within themselves. The monsters from sub-space illustrate this transformation. . . . At the end of the book the world appears no less absurd than it was at the beginning, but a dozen scientists have understood its genuine depth.

I end my letter with a timid request for a personal meeting.

**Lille. 20 February 1961.**

We now rent a single large room on the fifth floor of a hotel near the railway station, in the center of Lille. In one corner is a small kitchen with a primitive stove. There is no elevator. The bathroom we share with other tenants is five floors below, behind the bar. Our favorite dinner is a plate of Frankfurt sausages with French fries and mustard, eaten in some cheap bistro, listening nostalgically to Edith Piaf who sings Milord: "*Vos peines sur mon coeur, et vos pieds sur une chaise . . .*"

Many thoughts are rushing through my brain. The tension between the high potential and the petty reality of yet another series of examinations to prepare causes me both exaltation and pain. Early this morning Janine has gone to her work in Amiens, leaving my room full of sunshine. I am immersed in my notes from the astrophysics course, taught with great wit and wisdom by Vladimir Kourganoff.

**Lille. 22 February 1961.**

Janine and I have good long talks in the evening, until midnight. We discuss the course of our earlier studies and I see a similar force within both of us, a vision of life, an anticipation of the future, a certain way of committing ourselves to it. Something inside us seems to know where the path leads. It is as if we were marching towards another world, and as if we knew that other individuals throughout the earth are going in the same direction to meet us there. Perhaps we are going towards Paul Eluard's *other universe* which, as he says so eloquently, "lies within this one."

Each new discovery brings with it renewed silence. For those who

have pierced the barrier, words have never represented more than the emerging part of thought. Beyond words is the second meaning, the third meaning, the true ones.

Since September I have been working on a new science-fiction novel entitled *Dark Satellite*.<sup>7</sup> I am writing very fast, swept along by passion and Janine's kisses. Every evening I am anxious to read the new pages to her.

**Lille. 23 February 1961.**

Georges Gallet of Hachette has answered me:

I will be happy to see you if you come to Paris, any day next week at your convenience, as long as you let me know forty-eight hours in advance. In the meantime do not worry about your *Sub-Space*. You will probably be pleased to learn that it is among the manuscripts we have selected for the Jules Verne Prize.

**Lille. 20 April 1961.**

Too many ideas at the same time. I lack a roof, a job, in short life itself. How simple everything would be if I could just accept to be "a student." But I am not content with University life. Quite different is the world I see when I visit Hachette on Boulevard Saint-Germain in Paris, where *Sub-Space* is now under serious consideration for publication. I meet other writers there, free spirits, thinking minds like Jacques Bergier, who has recently published, with Louis Pauwels, *The Morning of the Magicians*. Reigning over this world of creative confusion Georges Gallet is warm and generous, jovial. He becomes enthusiastic as he talks about the early days of sci-fi, when he discovered that novel form of American popular literature as an interpreter with Allied troops during the war. He has met most of the classic U.S. authors and is a friend of Forrest Ackerman, the legendary collector of weird tales, who lives in Los Angeles.

**Lille, 23 April 1961.**

Psychology has to start from the relationship between man and the tools that link the psyche to the environment. But our tools have changed, changing the environment in turn and offering new paths of develop-

ment for man's intelligence to follow. Properly speaking, man has not "gone into space" yet, contrary to what journalists are fond of saying. Gagarin and Sheppard have simply flown higher than the others, **but** they did reach a new realm outside the atmosphere of Everyday Life. Tomorrow we will truly take mankind away from the earth, squirting humanity into the blackness of real space, deep space, splashing the universe with our childish laughter and our profound terrors. But that adventure may be a devastating test for us: The old type of man will not survive it.

The human being who will be able to function in space is already living among us. He has always been within ourselves, in every cry of despair of the soldier cursing the sky, every heartbeat of the young girl looking at the rising moon. Our scientists, romantics of the Sputnik era, may speak eloquently about the future, but they fail to see the mysteries buried in the present: infinity brushing against us in the anonymity of crowds, galactic trapdoors at the street corner\_\_\_\_

Those who are dead to the potential of the earth will also be dead when they fly into space. Those who have not eagerly expected the sudden glow of a star amidst fast-changing clouds will never be able to grasp its reality, even if some day they happen to hurl their rocket ship into the burning blast of a supernova.

What seems like the most opaque fabric from a distance appears under the microscope as a network of loosely textured threads. The human eye can see through the toughest of metals, if it wants to.

### **Lille. 26 May 1961.**

One of Janine's psychology teachers at the Sorbonne, Professor Rene Zazzo, wrote in 1946 in *The Future of Intelligence*.<sup>8</sup>

The metaphysics of the irrational is an excuse for every shameful act, every surrender. I know that it seems to answer a need for spiritual liberation, and I also know that this need has led the poets of surrealism and the philosophers of supra-rationalism to some magnificent attempts to reach beyond the world of everyday habit...but beyond reality what we find is still reality, only richer. Beyond reason what we find is still reason, only wider and deeper.

He adds:

Nothing is obscure except through our ignorance; nothing is fantastic except through our terror. It is by refining our reason that we will discover the laws and the rhythm of those things that are still hidden, not by some indescribable experience, some vague ecstatic intuition, some Dionysiac drunkenness\_\_\_\_

It would be absurd to imagine that we will simply be able to transplant our old mental habits to Mars or Venus. There will be a major change this time, perhaps even a conscious one. In this sense, Zazzo's faith in reason is well-founded.

We are just entering into the world. We are going into a universe from which technical difficulties had kept us. Until now we could be satisfied with a contemplative, idealistic or romantic attitude. Planets, solar systems, comets and galaxies were topics to be discussed under the heading of speculative philosophy or position astronomy. This will no longer be the case tomorrow. They will become our familiar landscapes, our everyday risks. In that new world we will have to learn again how to love, to cry, to laugh. The earth will no longer provide appropriate standards.

The European countries are making a number of grave mistakes right now in this domain. They are incapable of resolving their ancient cultural contradiction between "literary" and "technical" modes of thought. They are headed towards asphyxiation of their creative faculties. Any nation that approaches space research with the mindset of a graduate of *Ecole Polytechnique*<sup>9</sup> is a nation that will not survive the very first discoveries from that research without some major changes.

Sources of imagination are drying up in Europe. This is an amazing fact. Literary people are cut off from reality because they stubbornly remain blind to the technical underpinnings of the modern world. They cannot feed their younger public with new hopes or new images. As for the scientists, they deny or they ignore the cultural value of their own work. The way they look at science fiction is a case in point. Many view it with contempt, as a lower form of literature. Their science has lost all contact with the public. The chasm that a man like Flammarion<sup>10</sup> had once succeeded in bridging between the man in the street and the scientists has been opened again, and widened: modern scientists are becom-

ing needlessly, hopelessly specialized. Zazzo writes that "nothing is fantastic, except through our terrors." But our terror is a force that propels us forward. This new terror may be the most dynamic, generous attribute we have, and the only thing about us which is still pure.

**Lille. 31 May 1961.**

We will soon be going back to Paris. I have completed my examinations for the Master's degree here. The time has come to finish the operation.

Kourganoff has told me that the artificial satellite department at Paris Observatory was looking for people: I am applying for a position under Paul Muller, the same man to whom I had sent my *Sputnik* observation of November 1957. But in the long run Kourganoff tells me that I will be wasting my time if I stay there: "Artificial satellites do not have any real significance for science," he insists. Already last year he tried to discourage me from taking courses in programming: "Electronic computers are just toys for engineers."

In saying this he is merely reflecting the opinions of the academic mainstream. Four years ago the world was amazed to learn that the Russians had launched the first artificial satellite. My father did not believe in it any more than he believed in flying saucers: "Communist propaganda," he stated immediately. It was only when the conservative papers confirmed the news that he was forced to admit the evidence. But he never agreed that an astronaut would some day orbit the earth: "Man cannot get out of his sphere," he would say.

Even professional scientists were caught completely unprepared. No less an authority than the Astronomer Royal of Great Britain had said, a mere four months before *Sputnik*, that "Space Travel is Utter Bilge."

**Lille. 9 June 1961.**

In *Dark Satellite*, which takes place on Venus in the twenty-first century, I had to invent a social system that would replace both Marxism and Capitalism, since it is hard to imagine that either of these could support the expansion of human activity beyond the earth. I have dreamed up a new political organization called *Peripherism*. Under Peripherism the world would be structured, not as a consortium of big nations, but as a network of smaller regions. Most of Europe, for instance, would break up into areas similar to the old Provinces, with autonomy for well-defined

cultural, linguistic and economic units which would receive key services from the larger world. I imagine autonomous regions linked together through a giant computer that could manage Martian bases and lunar cities, as well as terrestrial units. The key idea in Peripherism is that by breaking down the major countries into their local components it would be much easier to defuse cultural antagonisms, to force people to be responsible for their own destiny, and to create an enormous federation serving everyone's interests.

I do not know what the destiny of *Dark Satellite* will be, but Georges Gallet has told me that *Sub-Space* had won the Jules Verne Prize. I have just signed the contract with Hachette. There will be a first printing of 15,000 copies, and a prize of 100,000 francs.<sup>11</sup>

**Paris. 14 June 1961.**

The medal of the Jules Verne Prize was given to me today at the Eiffel Tower by the pretty hands of actress Mylene Demongeot, who kissed me on one cheek while Janine smiled. More formally, someone from Hachette gave me my check, photographers took pictures and the *Tout-Paris* gorged itself on caviar and *petits-fours*.

Georges Gallet, kind and good-humored as usual, was greatly amused by my shyness. He took me aside: "Do you have any idea how much all this costs, renting the Eiffel Tower for a day, and all this food?" I replied that I did not. "Eleven million francs," was the answer.<sup>12</sup> Suddenly the check of one hundred thousand I had folded into my wallet had taken ridiculously small proportions. It was clear that all these people had not come to see me, or to read my book. This was purely a promotional event for Hachette. If I suddenly dropped dead they would all go on eating caviar and *petits-fours* as if nothing had happened. A wonderful, humbling lesson in the true meaning of literary glory!

The high points of the afternoon for me were the chance to meet Forrest, the "father" of Barbarella, who has designed a magnificent cover for my book, and Daniel Drode, last year's laureate, who had an even stranger experience than mine:

I was a soldier in Algeria," he told us, "stationed at the edge of the Sahara. I was staring at acres of hot sand beyond the nose of my machine gun. The field telephone rang. It was my Colonel. *Drode*, he said, *you have just won the Jules Verne Prize. Report to Paris immediately.* He gave me

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an exceptional two-day leave."

Poor Drode barely had time to fly to Algiers, and from Algiers to Paris, where he arrived without any sleep, changed into civilian clothes, rushed to the Eiffel Tower, was kissed by a lascivious movie star, watched the *Tout-Paris* gorging itself on eleven million francs' worth of delicacies, and flew right back to the front lines.

"I thought the sun was giving me hallucinations," he added. "Periodically I had to take my eyes off the sights of my gun, pull out the bronze medal from the pocket of my uniform and stare at Jules Verne's face to be certain all that had actually happened!"

# 5

### **Paris. 12 August 1961.**

I believe in terror. I believe in glimpses of beings from Beyond seen in dreams, reflections on layers of time. I have become interested in the saucer phenomenon again.

The flying saucers! For years I have thought about what we saw in Pontoise, that bright afternoon in 1955. I have tried to fathom what kind of research may be going on in "high places" and what data must be kept in the official files. I wrote to Aime Michel when he published the book in which he announced the claim that apparently unrelated sightings of a single day occurred along straight lines. Our correspondence has continued, without reaching any conclusion about the nature of the problem. Now the subject is taking on a new aspect.

This is not at all a propitious time for me to become actively interested in this particular problem. Since June I have become a government employee serving on the staff of the artificial satellite service of Paris Observatory. Actually we are part of the recently created Space Committee, which reports to the Prime Minister. I have a beautiful card in my wallet, where my picture is struck diagonally with the official tricolor. Naively, I started work here with great enthusiasm, assuming that we would be engaged in genuine research, in the highest quest for truth. That is not what I found.



## SUB-SPACE

The artificial satellite service is located at Meudon, on a high plateau near a fine forest from which one can see the whole of Paris. The staff is composed of three scientists under Paul Muller, with three secretaries and a computer programmer. Our mission is to track as many orbiting space objects as possible to keep every ephemeris up to date. The orbital elements we calculate are later used to improve theories of the shape and weight distribution of the earth, and to advance the calculation of satellite trajectories. Our equipment is primitive. During the day we compute the visibility windows of each satellite to build up an observing schedule; we answer the mail from the public; we plot the passages of the objects over a map of Europe; we run the programs that reduce the observations of the previous nights, using an old IBM 650 located in the former stables of the King's mistress in Meudon castle. We use theodolites, small but highly maneuverable telescopes that can be pointed quickly and with high precision. Every night we set them up in the dewy grass under the open sky. We patiently wait for our satellites to come and we aim our instruments at them as they cross the sky: "It's like hunting rabbits," says Monsieur Muller. We use a red traffic light down in the valley in Clamart as our reference point, azimuth 265 degrees 19 minutes. Electrical cables power the lights on the circles of the theodolite, the tape recorder, the precision chronographer.

After each series of observations I go back inside a nearby dome where we have set up our field office. Under the creaking floor I can hear the rats scurrying around. Rubbing the sleep from my eyes I reduce the data to punch up a Telex tape which will be transmitted to the U.S. Navy in Paris and, from there, to the Smithsonian Astrophysical Observatory in Cambridge, Massachusetts. In the nearby woods strange animals wail and scream. There is an owl that shrieks with an especially disturbing, strident cry, like a slaughtered baby.

Occasionally we observe objects that remain unidentified. Thus on 11 July at 10:35 p.m. I saw a satellite brighter than second magnitude. I had time to log a few data points. On another occasion several of us recorded no less than eleven points. The next morning Muller, who behaves like a petty Army officer, simply confiscated the tape and destroyed it, although a similar object had just been tracked by other astronomers at Desançon and by Pierre Neirinck, a satellite expert based in Saint-Malo.

Why don't we send the data to the Americans?" I asked him.

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Muller just shrugged.

"The **Americans** would laugh at us."

He seems terrified at the idea that the morning papers might come out with the headline: "Paris Observatory tracking something it cannot identify." Muller is a tough man who believes in discipline and a simple world where everything is neatly labeled. His previous career in astronomy was based on measuring the angular separation of thousands of double stars, the most painstaking work imaginable. He approaches artificial satellites in the same spirit.

We receive many letters from the public because artificial satellites are a hot topic. Many contain mad theories, deranged proposals complete with convoluted color diagrams and prophecies of the end of the world, of imminent doom. Many others simply submit sincere and accurate observations of various satellites which we can readily identify. I spend about an hour every day answering such letters, since we are a publicly financed service of the French government, with a duty to respond to public inquiries.

One morning Muller read to us passages from a letter he had just opened. It came from none other than Aime Michel (who doesn't know I work here): "A few years ago I have been unfortunate enough to publish a book about flying saucers," the letter said. "I have been the recipient of hundreds of reports describing observations that are of potential interest to science. I am in poor health and I have reason to believe that I suffer from a brain tumor. I would like to turn over all my records to an institution such as your observatory, where they can be preserved. Even if you do not agree that research on these phenomena is warranted, at least the records should be protected from potential destruction after my death."

"You see," said Muller with contempt, "that's another letter for the crackpot file. Although properly speaking Aime Michel is not really a crackpot, he is a crook." ("*Ce n'est pas un fou, c'est un escroc.*")

What could I reply? I felt incensed at the narrow-minded stupidity, at the injustice of this remark. Aime Michel sought no money, no publicity in return for his offer to turn over all his files. It was a proposal from a truly desperate man.

I went home to this one-bedroom apartment we are renting on the edge of *Les Halles*. I sat at my desk and I wrote a letter to Aime Michel, suggesting a meeting.

**Paris. 20 August 1961.**

I am just coming back from *my* first meeting with Aime Michel. He lives in an apartment in Vanves, just South of Paris, on the second floor of a building that overlooks the park. I barely caught sight of his wife, who opened the door and ran off shyly into the darkness of the hallway without speaking to me. He greeted me and took me into his office, a warm little room with a desk overloaded with papers, piles of books, articles in various languages, many letters. Notes are pinned to the fabric which covers the walls.

In control of that mass of information is an amazing gnome of a man, short and deformed, who barely reaches to my stomach. Yet he radiates a kind of beauty that is unforgettable, a beauty that comes from the mind and from the nobility of his piercing eyes. He shakes his bald head and lights up with a wonderful smile as he tells me: "You know the worst thing about being crippled as I am? I will never be able to kick some of these arrogant scientists in the *derriere* as they deserve!"

Yes, he tells me, he wrote this letter to Muller because he is fed up with flying saucers. He has amassed so many documents that he will never be able to process them all by himself. He thought he was close to a breakthrough a few years ago when he discovered his *famous* alignments among the sightings, but the underlying order, if there is one, eludes him to this day. All of that cries out to be checked by professional scientists. If he turns out to be wrong, so be it. At least he will be free from the anguish of bearing this awesome responsibility by himself.

He is utterly disheartened by the onslaught of bitter criticism he has received from the so-called "rationalists." They go so far as implying that he has actually invented the observations, drawing the lines first and then writing to newspapers in the towns that fell on each alignment to inquire about possible sightings! One of the most rabid of the group, cosmologist Evry Schatzmann, has even told him: "Your alignments cannot possibly exist, since flying saucers cannot exist!"

Well, let them take all these letters," he says as he lifts piles of envelopes from his cluttered desk. "Let them check, they will find that all *these* people do exist. Then correlate what they have seen with the *news*-Paper clippings, with all the published *documents*!" He pauses, looks *me* straight in the eye:

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"They will find out that I am not lying."

He is surely not lying, I tell him. But truth will not impress those people. I describe what has happened to his letter to Muller, and I quickly add:

"Don't worry, those who do all the real work at the observatory are the young researchers. We program the computers, we track the satellites at night. You don't need official recognition from the old guard, what you need is some people who will roll up their sleeves and do the real work. What do you want me to do?"

"Are you serious?"

"I wouldn't have come to see you if I wasn't serious."

He whistles softly.

"That changes everything."

He remains silent for a few minutes. He looks out at the park, then brings his eyes back to me:

"There is one thing you could do. You see, my alignments don't really make any sense if they stop at the French border. If they are real, they must indicate a world-wide pattern. Every one of them must be a section of a great circle. It would be interesting to take some of the best-authenticated straight lines and to extend them around the world, to see where they go. Would you know how to do that?"

"I wouldn't be a very good astronomer if I couldn't compute a great circle."

"That would be a lot of work."

"I'll use an electronic computer."

I assure him I will find a way to discreetly use the IBM 650 at Meudon to perform this calculation. I can see he is skeptical that I will actually get to work on the problem. Many people have promised to help him before, and he has never seen them again.

Even the Americans, with their enormous technical means, don't seem to have done any real analytical research.

On my way home I have started to think about the data reduction in spherical coordinates. I have decided to begin with the Bayonne-Vichy line which links no less than six independent sightings made on the same day of September 1954, as the French wave was just beginning.

Paris. 28 August 1961.

A letter mailed today to Aime Michel provides him with the track of the Bayonne-Vichy line computed as a great circle of the earth. I did the first calculations by hand, using only the two end points. But that will already enable him to plot the coordinates on a map and to verify his hypothesis, using foreign cases this time, well beyond the French borders.

Paris. 24 September 1961.

Aime Michel has answered me: the great circle was remarkable, he wrote. Intrigued, I called him on the phone this afternoon from the subway station. (We don't have a phone at home. It would take money to get on the waiting list and three years to obtain the equipment.) He explained to me why he was so excited: the line goes through three major areas of high concentration, linking saucer waves in Brazil, New Guinea and New Zealand. In the latter case the line goes exactly over the harbor at Wellington, where a celebrated observation was made. Is that significant?

On September 1st a former astronomer from the satellite service called us: he had seen another mysterious object at 22:00 UT. It was near the zenith and as bright as the American *Echo* satellite. Janine saw one on September 3rd at 20:01 UT. Muller himself has recorded several unknowns with his staff. He classifies them as "aircraft" or "mistake." Later they get lost.

Paris. 26 September 1961.

Today a very discreet conference about flying saucers took place at Meudon observatory. I had succeeded in getting the whole staff of the service to attend, except for Muller, of course. I have started to use the IBM computer after hours and on weekends to compute Michel's great circles. Our staff programmer is teaching me the assembly language of the 650 to enable me to encode my algorithm into this antique machine. This is a formidable engine with a 2,000-word drum memory, and it takes up an entire room. It looks like a locomotive, and it occasionally sounds like one.

A scientist named Pierre Guerin, who is a planetary expert with the Astrophysical Institute and a friend of Aime Michel, has joined our

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group. He told us that in his opinion the saucers existed and were probably operated by space visitors. They could be biologically advanced in the evolutionary sense with respect to humans.

Guerin, a tall fellow with jet-black hair whose family comes from the rebellious Vendee, is prone to snap judgments and doesn't like to be challenged. Yet I did question his conclusions about the occupants. I still see all assumptions about "their" superiority as grossly premature.

We debated whether or not it was desirable to seek official recognition for the subject. I am not so sure it would be a good idea. With official recognition would come bureaucratic procedures, lengthy delays, committees about everything. The people entrusted with the power to supervise the research and to control the budget would be the same old scientists who have denied the reality of the problem all along, and have called Aime Michel a crook. Our research would be emasculated by their lack of creativity and their need to reduce everything to that dull state of uniformity they mistakenly label as "rationalism."

### **Paris. 27 September 1961.**

We have to be very careful now or I could lose my job. Our little gathering yesterday has been noticed and the staff of the observatory is already gossiping about us. The people next door to us belong to a group headed by Dollfus, who does balloon-based astronomy. They know Guerin very well: Dollfus and Guerin are both former students of Gerard de Vaucouleurs, and both are interested in Mars. Now the group wonders, why would Guerin, who is a planetary expert, come to Meudon to talk to satellite trackers? It seems amazing to me that people should find it suspicious and undesirable for scientists of adjacent disciplines to talk to one another. Isn't that what science is all about? The maps I have been drawing are also attracting suspicion. Fortunately for me, nothing looks as much like a satellite orbit as a plot of Aime Michel's Bayonne-Vichy line!

All this work after hours or on Saturday and Sunday gives me very valuable hands-on experience at the console of the computer, but the learning curve can be very frustrating, because our equipment is notoriously unreliable.

The most beautiful sound I have ever heard is the pure and highly musical hum of the memory drum of the IBM 650 when the computer

dies. All power goes out. The motors are still. The console lights stop blinking and of course the program is lost. I become aware of the summer sun in the dusty courtyard behind me. I hear the birds playing and singing. But it takes many minutes for the big drum to slow down to a complete stop. The high pitch gradually turns into a sustained, **thrilling** note, unnoticeably shifting to a rumble, then just a murmur. Eventually, the drum joins the rest of the computer in death.

This kind of incident happens to us once or twice a day because our power supply often fails. Even when things are working properly this computer is very slow. The satellites go around the earth in ninety minutes, but it takes the machine two hours to compute an orbit, so we are always hopelessly behind. Yet many of our astronomical colleagues consider this computer an example of extravagant waste. They are jealous of us: Think of all the shiny telescopes one could buy with the same money!

#### **Paris. 28 September 1961.**

We have now measured with as much precision as we could the positions of the best documented sightings on BAVIC (as I now call Bayonne-Vichy) and a second prominent line. Janine has begun a list of coordinates for all the points mentioned in Aime Michel's book. We have bought stacks of detailed Michelin maps which cover the whole of France. Unfortunately we do not have the luxury of sending investigators to every location: we have to take the best information we can from the published data: Some errors will be unavoidable. To reduce their impact I am computing the great circles through a least squares fit.

I continue to write science fiction. *Fiction* magazine has just published one of my short stories, *Les Calmars d'Andromede*.

#### **Paris. 14 November 1961.**

Another trip to Vanves to see Aime Michel, who hands me a copy of the world's first sighting catalogue. It was compiled by a man named Guy Quincy, who lives in North Africa. This is a very valuable resource, even if its entries are often sketchy, because it covers over a thousand cases. It is a typescript of which only a handful of copies are in existence.

By plotting the computer data on the map I have made a curious observation: The first three great circles I have computed, **which** were

selected because they linked the most remarkable cases, happen to intersect in a single point, near the town of Cernay in Alsace.

**Paris. 25 November 1961.**

Janine and I are now doing a study of the correlation between the frequency of sightings and the distance of planet Mars, which comes closest to the earth every twenty-six months. The resulting curve is striking. Guerin tells us he had not expected such a clear-cut relationship.

If the saucers come from space, why don't we see them when they are at a great distance from the earth? Recent experience with artificial satellites has shown that even a small sphere can be detected with the naked eye at an altitude of hundreds of miles. And a simple theodolite enables an observer to detect much fainter objects. Could this mean they don't actually come from outer space? We have no answer to this puzzle.

**Paris. 7 December 1961.**

Aime Michel has given me another list by Guy Quincy which covers landing reports. Today I have reviewed all of my notes and I have updated our first catalogue. Janine has been assigned a job as a psychologist with a school near *Gare de l'Est*. After work she helps me by putting on index cards every single sighting we can find. Aime Michel believes that daily alignments of sightings are characteristic of saucer waves but he agrees with me there is nothing magical about a twenty-four-hour interval. In fact he once thought he had discovered a twenty-three-day period between the most important cases, but he could never confirm it.

Janine and I are tired. She is afraid that I will fall asleep at the wheel, so some nights she drives with me all the way to work in Meudon.

**Pontoise. 31 December 1961.**

Taking advantage of the holidays I have plotted on a world map all the landings prior to 1954 and all the significant observations before 1947. There is no pattern, no law at all. Now I am trying to find out what researchers in other countries may have done on this problem.

Last week, while browsing along the Seine among the boxes of the *bouquinistes*, I was able to purchase French editions of a book by Major Keyhoe and of *Flying Saucers Have Landed* by Leslie and Adamski. The latter is utter fantasy, with fine fake pictures of a "Venusian saucer" that is



nothing more than a dining room lampshade or some such ordinary object photographed at close range.

Seen through Keyhoe's book the attitude of American scientists and military men is quite disturbing. They behave like a well-organized insect colony whose life is suddenly disturbed by an unforeseen event. The Air Force's inability to think about the world in terms of anything other than the Air Force itself strikes me as particularly curious. For example, when they decided to study the flying disks they created a commission composed of rocket experts! And their idea of active "research" is to chase the objects with their jet fighters in the hope of shooting one down. . . .

In the whole history of the problem Aime Michel has perhaps been the only man who looked at the evidence humbly and calmly. To me this phenomenon is not simply something that should be investigated, *it is a psychological test*: the first great collective intelligence test to which mankind has been subjected. The sightings put into question both the structure of our society and the laws of our physics. Naturally we are free to run away from this test, as our scientists are currently doing.

### Paris. 21 January 1962.

I have revised the flow-chart of my great circle program. Guerin has given me some useful advice for an article about the Mars relationship. He suggested I send a copy to his former mentor Gerard de Vaucouleurs, a French astronomer now established in Texas, and to a man named Allen Hynek who is the scientific consultant to the U.S. Air Force on the problem. Both are open-minded, he assured me: they visited Aime Michel with Guerin in Paris four years ago and they were impressed with his data. I have agreed to put my real name on the article, even if it ends up being published in a magazine like the British *Flying Saucer Review*. I should have the guts to take a stand.

Since the first of the year I am no longer working in Meudon. I resigned, fed up with the pettiness of French astronomy in general and with the narrow-mindedness of Muller in particular. It is a small incident that finally drove me to quit. One evening, driving back from visiting my mother in Pontoise, I saw that the sky was quickly getting overcast and that satellite observations would be unlikely at best that night. I went to bed instead of reporting to the station. The next day Muller was very upset.

But the sky was overcast," I said. "I thought . . ."

He interrupted me:

"That's exactly your problem, Vallee, *you think too much.*"

Perhaps that was a compliment? In any case I have looked at other job opportunities and I have quickly discovered that I could earn twice as much as a research engineer with an electronics firm. I now find myself with more advanced computing tools, and more stimulating people who are in touch with reality.

A scene that would be amusing if it was not a tragic illustration of French scientific bureaucracy took place after I handed in my resignation. Muller made several valiant efforts to get me to return, stressing the fact that a retroactive raise had recently been given to astronomers on the government payroll to correct the blatant injustice of their low salaries. Naturally, this raise was due to me even if I maintained my resignation. But the administration denied it. I had the audacity to take my complaint all the way to the head accountant who had his office in the main building of Paris Observatory.

It was my very first visit to that august institution. I was awed by the impeccably polished hardwood over which the great Leverrier had walked and where a line in the floor materialized the Paris meridian. I admired the shining copper of the antique instruments on display, the magnificent rooms with their period furniture and their dignified portraits. In these glorious surroundings the accountant turned out to be a cowering bureaucrat who was not used to being challenged. He tried in vain to argue against my retroactive raise. I left the observatory with a modest check in my hand and a very bad taste in my mouth.

#### **Paris. 24 January 1962.**

Guerin has sent a draft of my Mars correlation article to Gerard de Vaucouleurs. Aime Michel has mentioned it to Yves Rocard, who is director of the physics laboratory at *Ecole Normale Supérieure*. Rocard appeared to be intrigued. He recommended that it be published quickly. Aime Michel's health is better: His worry about a brain tumor has turned out to be unwarranted.

He also told me the funny story of how he had met Guerin. After his book came out in 1958 he received a very irate letter from him, lamenting the shortcomings of his methodology. He called Guerin and came to his lab, ready for a confrontation.

## DISCOVERIES

"Why did you send me a letter full of insults?" He asked.

"What insults?" replied Guerin, genuinely surprised. "I wrote to congratulate you!"

### **Paris. 26 January 1962.**

Janine has introduced me to a statistician who is her teacher at the Psychology Institute. We need advice to evaluate the significance of the correlation we have found between the frequency of unexplained sightings and the proximity of Mars. At first, however, she ran into some strange reactions on the statistician's part.

"Where does the data come from?" He wanted to know.

"That doesn't matter," she said cautiously, since I had lectured her about keeping our study very quiet. The gossiping that surrounded us at Meudon has made me very nervous, and we don't want to get Guerin into trouble. She added: "This is a purely theoretical question."

"Not at all," insisted our friend, "these numbers look to me like astronomical parameters. Are you sure this doesn't have anything to do with Mars?"

It turns out that he has a colleague named Michel Gauquelin who has been doing his own clandestine study, an attempt to disprove astrology while demonstrating to his students the power of the statistical method. He did expose for them the myth of the zodiac, but the rest of the calculation backfired: he found a strong, unexpected correlation between certain positions of the planets at birth and the professional destiny of the individuals in his sample. There is an unexplained "Mars effect" which affects many people born with the planet just above the horizon or just beyond the zenith.

We soon met Gauquelin, and we laughed when he and his charming wife Francoise revealed their secret to us. They laughed too, when we told them where our own data came from.

Michel Gauquelin agreed that Janine's correlation computations were on the right track. He has given us some suggestions regarding the type of statistical test we should apply.

### **Paris. 28 January 1962.**

For the last two days I have been making a copy (by hand) of Aime Michel's card file, which holds a thousand cases. Each card is a reference

to a raw document, so it is hard to estimate how many actual cases the file represents. There is a huge amount of work ahead of us. On the same card I sometimes find four or five observations. Most of the time he had only noted the date, the time, the place and a reference number, so I am left very hungry and frustrated: "Bloemfontein, most interesting!"

**Later the same day.**

I have just finished copying the cards for 1958, 59 and 60, some 400 cases in all. Since there was almost no overlap with the Guy Quincy catalogue I started worrying about the stability of our correlation with Mars, so I ran a comparison between the two sets. The results were beyond my expectations: Here are two researchers taking their data from independent sources, in different countries. Three out of four cases in Michel's files are not known to Quincy, while the latter has many cases from North Africa and from America. Remarkably, not only do the peaks coincide, but the details of the distributions match too. Could this mean that the percentage of errors in each file is actually very low? Are geographic and population factors playing a weaker role than we thought? Is Aime Michel right when he says there would still be a noticeable wave in the Fall of 1954 if we threw away all the French cases?

In spite of the late hour I went out again among the produce trucks and the streetwalkers of *Les Halles* to find a public phone at the Reaumur subway. I called Aime Michel to give him the monthly sighting figures and to ask him to think about their meaning.

**Paris. 31 January 1962.**

This afternoon I went back to see Guerin in his office on Boulevard Arago. I have a lot of freedom in my new work, as well as a measure of intellectual independence that was denied to me at the Observatory. Guerin works in a long, dark, dusty office in the basement of the Astrophysical Institute. Gerard de Vaucouleurs used to work in that room, which he left long ago for the wide open spaces of Texas. By the way, says Guerin, he is looking for an assistant, preferably someone who understands computers. If I was ever tempted to move to the United States, he could be a point of contact—

We have to keep our voices low when we discuss our forbidden subject: the walls are thin. He likes my article about Mars. At no point do I

argue that the correlation is a "proof" of anything. We are simply attempting to analyze a phenomenon.

What the saucers are doing on earth eludes us completely. The type of observation that troubles me most is what Aime Michel calls the "medusa": an object comes down from the sky, hovers a dozen feet above the ground and drops a small probe that touches the earth and goes back up. This suggests that they are taking measurements, but it doesn't correlate to anything we know. As for the landings, we do not have any good explanation either: could it be that our visitors are simply trying to get used to earth's gravity?

**Paris. 7 February 1962.**

Aime Michel has given me the address of Dr. Hynek, who is director of Dearborn Observatory at Northwestern University in Evanston, near Chicago. I have mailed my Mars correlation article to him, to de Vaucouleurs and also to Richard Hall, of the civilian research group NICAP.<sup>13</sup> We have finished copying Aime Michel's cards for all cases after 1954.

Through a friend who is doing his military service at the French Air Force headquarters, I have had access to the saucer files they have been quietly maintaining since the early fifties. They are full of well-documented sightings, including a remarkable incident in which an object was tracked on radar over Morocco.

**Paris. 9 February 1962.**

A demonstration against the extreme right turned ugly last night. Mad with hate, the cops charged the crowd, forcing it to retreat down the stairs of the subway station at Charonne. The iron gates had been drawn shut. Still the policemen, many of whom belong to neo-fascist groups, charged viciously, beating people with clubs and rifle butts, pressing them against the bars. In the panic several men and women died, crushed under the weight.

I was deeply shocked as I drove home through the area of the Bastille on my way back from work a few hours after these atrocities were committed. Today the whole area remains stunned, in deep mourning. There <sup>is</sup> a palpable, terrible sense of catastrophe, of despair, a feeling that reminds me of the dark days of the German occupation as I was aware of them as a child. I am fed up with such violence, fed up with this country

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and its absurd political intrigues. How could anybody ever build something of value here?

### **Paris. 29 February 1962.**

I went to see Robert Kanters in the offices of Denoel on rue Amelie this afternoon. He is an affable man who heads up their science-fiction collection, *Presence of the Future*. He likes *Dark Satellite*, but he wants me to rework the end of the novel.

It is a strange experience to write science fiction here. The area where we live is still the Paris of Baudelaire, or of Villon. It has not changed much in centuries. On our street a dozen pretty girls stroll along the sidewalk by day and by night, wearing the shortest possible skirts and whispering sweet suggestions into my ears every time I pass them on my way home. We have agreed, Janine and I, that we would never "own" each other; that we would keep the wild freedom of love that gives a unique meaning to life in general, and to our life together. I must confess that I take advantage of this freedom: I would lie if I said that I was always deaf to my *neighbors'* suggestive invitations.

The streets are throbbing with tempting pleasures. Every night, all around us *Les Halles* celebrate life. Huge trucks bring the produce from the four corners of France. Workers unload the beef and the salad crates, the cheese and the pallets full of strawberries, lining them up on the sidewalk among the vagrants waiting for a chance to steal an apple, the poor students looking for a night job, the bag ladies rummaging in the trash, the glittering whores parading before the truck drivers, displaying their Parisian style before the farmers from Perigueux or from Brest. I watch the whole picturesque scene with lusty amusement.

### **Paris. 15 March 1962.**

Yesterday I brought back all his index cards to Aime Michel. The more we make progress in this research, the more upset I feel with the constraints under which we have to do this work, outside of normal hours, on weekends and evenings. In spite of all the improvisation, the computer programs to reduce the observations and compute the great circles are now ready, and we have a sizeable catalogue at our disposal. Isn't it time to place our work on a solid footing, instead of this suffocating, childish clandestine business? We need a stable structure within which we could

do some real science.

"I've decided to move to the United States," I told Aime Michel yesterday. "Even at the electronics company where I work, I am simply doing technical research on problems whose solutions have already been discovered across the Atlantic."

When I paid a visit to the U.S. Embassy, however, I came away discouraged by all their forms, by their haughty handling of us "aliens," by their bureaucracy. I hope this is not the real face of America.

Again Aime Michel promised to mention my ideas to Yves Rocard, who will perhaps find a way to keep this work in France. Why can't we find a discreet laboratory somewhere where I could shelter my research?

When I think about it I am fairly sure that no one in France, not even Professor Rocard, can do anything. He may be one of the "bosses" of French physics, a leader of the atom bomb program which is dear to De Gaulle, yet his rationalist colleagues might hurt him professionally if they found out he has an open mind on subjects like dowsing and flying saucers. I find this especially ironic, when the man who directs nuclear tests from the flagship of the Pacific fleet isn't allowed to spend a few francs to explore the frontiers of science with a handful of his friends.

We also speculate about the work going on in America. Aime Michel suspects they have not even gone as far as we have. He bases this opinion on the fact that "Project Blue Book"<sup>14</sup> has a staff of only three people. Their files are almost useless, judging from the overly complicated questionnaire designed for them by a team of psychologists, which breaks down every case into meaningless details. As Dr. Hynek told Aime Michel when he saw his files: "You're luckier than I am—at least when you read your documents you can find out what actually happened!"

### **Paris. 24 March 1962.**

I have obtained permission to use the computer all day, and everything<sup>15</sup> is now ready for the calculation of Aime Michel's largest "network" of alignments. I wonder what will come out of this massive effort.

<sup>16</sup>In the evening, when we do not go out for a walk or a movie, Janine and I listen to the marvellous songs of Jacques Douai:

Nos plus beaux souvenirs fleurissent sur l'étang  
Dans un lointain château d'une lointaine Espagne:

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Il nous disent le temps perdu, 6 ma Compagne!  
Et ce blanc nenuphar, c'est ton coeur de vingt ans ...

*(Our finest memories are blooming on the pond  
In a far-away castle in far-away Spain:  
They speak to us of all the time we lost, my Darling!  
And this white water-lily, your twenty-year-old heart...)*

### Paris. 26 March 1962.

Nothing of any significance has come out of the computer. I find no remarkable pattern so far in the distribution of the great circles, so our research is stalled.

My employer has taken delivery of a new machine from IBM, the first 1620 model they have shipped to France. Our engineers are astonished: it is entirely built of removable printed circuit cards bearing transistors instead of the traditional tubes; even more remarkable, many connections are wire-wrapped rather than soldered. When something goes wrong the IBM maintenance man runs a diagnostic program, removes the bad card and replaces it with a new one without troubling to identify and fix the specific device that has failed.

I picked up one of these cards from the trash, put it in my pocket, and went to see Aime Michel.

"I want you to imagine that we live in the fifteenth century. You are the learned abbot of the monastery, you have read all the available literature of Antiquity and you are familiar with the full extent of science. I am a farmer who has just found an object in his field, and I bring it to you to find out what I should do with it. Here it is, Father."

He took the card between his fingers, turned it around carefully this way and that, and finally gave it back to me:

"Burn it, my son, it is the work of the Devil!"

Indeed, what could a fifteenth-century scientist do with such a piece of technology? Even if he had been such a great chemist as to analyze the composition of the transistors, and had had the formidable insight to guess at the existence of germanium, would he have detected the *impurities* in the germanium? And if by some miracle he did, would he have concluded that these impurities had been put there *on purpose*, to create the effect of semiconductivity? And wouldn't we be in the same position



with respect to a piece of flying saucer hardware, if it happened to fall on the earth?

**Paris. 5 April 1962.**

We met with Cristian Vogt yesterday. He is one of the founders of the Argentine research group CODOVNI, based in Buenos Aires. He told us that our current research was far ahead of anything he had heard about, including official American studies. Apparently the U.S. Air Force gathers a lot of data but doesn't really do any analysis. It only looks at the sightings case by case. He also told us about some cases in Argentina that appeared to fall on my extension of the BAVIC line.

Today Guerin called me at work with an exciting observation. He had noticed that the three great circles I had computed divided the equator according to a defined scale, with a basic unit of 12.4 degrees which turns out to be related to the Martian mean time. Could we have found a genuine law? The elegant geometric pattern formed by these three great circles does seem to be more than the product of coincidence. Guerin also asked me how the contact was going with the University of Texas. I told him I was waiting for a firm job offer.

**Paris. 16 April 1962.**

Taking the bull by the horns I have decided to punch the totality of our data into IBM cards. Such a file will be essential if a serious professional study is undertaken some day. Janine is helping me set up a card format and a code for the sightings. We keep accumulating new cases from every book we can find.

We are always afraid of being overheard when I call Guerin over the telephone from the office. Our colleagues would be intrigued if they listened to us as we say things like: "There has been a landing in Normandy and a vertical cigar over North Africa..." Therefore I have devised a code which also turns out to be a good classification system for the sightings. I designate the landings as Type I, the vertical cigars as Type II, and so on.

If we moved to Texas we wouldn't have to hide this work.

**Paris. 21 April 1962.**

Instead of going to the movies or to the corner cafe we spend every

evening punching cards. After work we now make it a practice to go to the magnificent facilities of IBM France on *Place Vendome*, where the key-punch units are freely available to IBM customers. There we are able to type, sort and print to our heart's content.

The De Gaulle government, trying to protect the French language, has made it a rule that all high-level computer codes must now be in French. Fortran programmers cannot write "Go To 103" any more: They must express it as "Aller a 103." This is fine, except that there are very few experts at IBM France who are of a high enough level to understand the intricacies of the compilers (the special programs which translate the languages themselves into the code which is executed by the machine), and they now spend all their time correcting the numerous new versions that keep arriving from the United States. They do their best to translate the instructions themselves, but they don't have time to translate all the error messages.

Last night at IBM we met a disoriented Frenchman who was staring at his output without understanding.

"Look," he told us, "this says: CORRECT SOURCE PROGRAM, yet the program wasn't executed!"

We had to explain to him that the English wording definitely did not mean that his program was "correct." On the contrary it was an urgent invitation *to correct it...*

When we come out at midnight we find Paris calm and clean, washed out by the Spring rains. We drive quietly from light to light, in Janine's blue Renault Dauphine. This highly civilized city will wake up again tomorrow in its well-ordered world. From the patient stars has come a sign we are trying to decipher. At the end of our own night, we do not know what kind of awakening we will find.

### **Paris. 17 June 1962.**

Yesterday I finished computing all the alignments in Aime Michel's book. I still find no pattern. So much for the idea that the great circles might generate a regularly spaced set of intervals at the earth's equator, as we had speculated with Guerin. But we have not given up the search for patterns.

After eliminating from our data the cases that are explainable by conventional causes we now have 2,437 entries on file. We are going ahead

with the keypunching of 500 selected observations for which we are computing precise coordinates. In the meantime the U.S. Embassy has begun to process our visa application.

### **Marigny. 10 August 1962.**

A few days' rest at the home of Janine's parents in Normandy, not far from Bayeux. Through the window I see the vegetable garden, the cabbage plants and the neighbors' rabbit cages. Beyond that, a few hedges, some bushes blurry with rain, and the open fields. To my left the quiet village climbs the hillside. As I sort out my notes, new ideas come to mind. Now I think of publishing everything, even if we don't have a perfect key that will unlock the mystery. At least we can vindicate Aime Michel by proving that his alignments do exist, that he did not make up the data. But what do they mean? And could they be only the product of chance?

At last the letter we had been waiting for has arrived from Gerard de Vaucouleurs in Texas. He confirms that he is offering me a position as a research associate in Austin. Also a letter from Antonio Ribera in Barcelona. Janine is compiling new index cards based on the voluminous files of Charles Garreau, which Aime Michel first used as the basis for his book. They give us one more way to calibrate and extend his work.

Fat with the rain, this little town sleeps in blissful ignorance of our scientific puzzlement. Marigny will be my last real memory of France.

I will leave without much regret. Since the Charonne episode of last February I have felt disgusted. In vain have I expected any indication that De Gaulle would take disciplinary action against the thugs in his police force.<sup>15</sup> Wouldn't the opposition, communists and socialists, commit similar crimes if they had the chance to get into power? The hypocrisy of French society has become obvious to me, under the veneer of the convoluted commentaries in *Le Monde* or the snobbish style of *L'Express*.

In the countryside near Marigny dogs howl, furiously foraging in the hedges. Behind their enclosures, cows complain noisily. A romantic moon spreads its light over the fields. We are already thinking of what it will be like to sail to New York on the Queen Mary.

**Pontoise. 15 September 1962.**

From the little plywood dome on top of the roof I spent an hour observing the planets, aware that this may be the last time I looked at the starry sky from my old house. I followed Jupiter and Saturn with my telescope. The night was clear, the moon very bright. For old time's sake I tracked a satellite for one minute on a nearly horizontal trajectory from the West to the Southwest.

Robert Kanters tells me that Denoel will publish *Dark Satellite* in November.

**Paris. 18 September 1962.**

Pierre Guerin and I have paid a visit to Paul Misraki in his beautiful Paris apartment. An excellent musician, well-known for many popular songs (most notably "*Tout va tres bien, Madame la Marquise*"), he is also a deeply reflective man and something of a religious scholar. He has just published a book entitled *Les Extraterrestres*, in which he argues that some religious miracles, notably Fatima, could have the same cause as modern saucer sightings. And what about the vision of Ezekiel? He showed his manuscript to a Catholic bishop, who told him that he was presenting a perfectly valid interpretation of the facts. Although the Church had a different interpretation, this was not a matter of dogma, and he did not require the *Imprimatur*.

**Pontoise. 13 October 1962.**

All our books and personal effects are now locked inside three large green footlockers over which I have painted our destination in big white letters: *Austin, Texas*. Amidst my parents' old furniture, in the house on Saint-Jean street, how incongruous these words seem!

My mother has helped me dismantle my old observation dome.

*Fiction* has published a second short story of mine, *L'Oeil du Sgal*. Robert Kanters tells me that *Dark Satellite* will be in the bookstores next month. I won't be in Paris to see it.

Part Two

# BLUE BOOK

# 6

## **Austin. 29 November 1962.**

On the way to Texas we spent two days in New York City to rest from the rough sea journey on the *Queen Mary*. We stayed at the Taft Hotel near Times Square, feeling very much like tourists crushed by an alien city. We took the obligatory trip to the Empire State Building and Greenwich Village and tried to forget the uncomfortable entrance into New York harbor. We were unimpressed by the first human contact with America. Contemptuous bureaucrats kept all passengers waiting for hours, like mere cattle, at the Customs Office.

We flew from Newark to Texas on a four-propeller plane that stopped in Washington and Dallas before landing here. We have found a temporary place to rent. People smile at our halting, imperfect use of English. But the weather is magnificent, and I have become acquainted with the University campus.

Now the movers have delivered the boxes containing all our sighting files, and we are busy reorganizing them. Here I will be free to do my own research in my spare time, and to use the computer facilities whenever I want to, so we have made great plans to extend our catalogues and to punch more data into IBM cards. We don't have to hide anymore. Janine argues rightly that our coding system is too limited: it drops some important features of the sightings.

We are going to turn one room into a study, complete with file cabinets and space for specialized magazines. I have ordered several books in English to expand our library.

## **Austin. 10 December 1962.**

For \$800 in cash we have bought a real American automobile, a 1958 Buick Roadmaster. It is black, huge, a real gangster car, with a red leather interior. There are switches for everything: raising, pushing back and tilting the seats, putting up the radio antenna. The starter is located

under the gas pedal, a fact which confused us at first. Janine has already found a job as a research associate in the Testing and Counselling center. After one look at her Sorbonne diplomas the University granted her the equivalence with a Master's degree in psychology. She is auditing graduate courses in statistics to familiarize herself with American terms. De Vaucouleurs is planning to send me to MacDonald observatory in a few weeks, and there is talk of my attending a conference on the exploration of Mars in Colorado next June.

**MacDonald Observatory. 15 February 1963.**

We just drove up from Austin to the snow-covered top of Mount Locke for a series of observing sessions. My main task is to take a series of galactic spectra, starting with NGC 1741. We are using the prime focus of the 82-inch telescope, a huge monster lodged inside an enormous dome. The night is windy, fairly clear. Our target galaxy was discovered by Stefan at Marseille observatory, Gerard tells me. He even recalls that Stefan was using a Foucault telescope with a wooden mount.

Back in France another short story of mine has appeared in *Fiction*. It is entitled *The Planets Downstream*.<sup>1</sup> I was surprised to see that the same issue contained a review of *Dark Satellite* by Gerard Klein, who calls it "one of the most interesting French science-fiction novels we have been able to read in recent time," and he goes on:

The description of the Paris of the future he sets up in a few pages is a little masterpiece, as well as his precise but too fleeting images of foreign worlds, of alien machines. It seems that the language pulls the author forward, forcing him to venture to the edge of the comprehensible. His wild rush launches him into a maze where time and space and the multiple, misleading appearances of parallel worlds are mixed together.

After analyzing the various characters and situations in the novel, which he compares to *Sub-Space*, he concludes:

One could attempt a finer analysis of the themes of *Sub-Space* and *Dark Satellite* to try and elucidate (the author). But I believe he will do it soon enough himself when he emerges from his own poetic torrent, from his own formalism. He will then place the

nearly unique explosion of his spontaneous style at the service of a surer, more acute awareness.

**Boulder. 24 February 1963.**

I am spending two weeks in Colorado at the National Bureau of Standards to discuss problems of two-dimensional data analysis with a French-born senior scientist and his staff, and to learn about computational techniques which we can apply to two of our current problems: the mapping of Mars and the reduction of background data in galactic photographs. Janine came along with me.

We drove through the Sangre de Cristo mountains which were covered with snow, and offered an awesome vision of extraordinary beauty.

Yesterday I witnessed an animated discussion about Vietnam at a party attended by many staff members. After a few drinks the French scientist cautioned his American friends about getting embroiled into a full-scale war: The Vietnamese will push you out, he said, just like they threw us into the sea.

They responded angrily, suspiciously, nearly accusing him of being in league with international communism. He tried to explain:

"Don't give me that garbage, I fought in the Mekong delta long before you did, I was a French officer," he told them angrily, turning red in the face. "You don't understand the situation. You can't even imagine the conditions there."

They wouldn't listen to him. America is so much more powerful, they said with quiet arrogance, "you can't compare what happened to the French with what would happen if we went in there." These people have blinders over their eyes, dangerous blinders.

**MacDonald Observatory. 2 March 1963.**

We are back at the observatory, in a little house that looks down into the deep valley. In the evening several deer come to our door, begging for bread and apples. On our way back from the Bureau of Standards we drove through those high points of saucer history, Amarillo and Lubbock, where a celebrated photograph of lights in formation was taken in the fifties. We stopped in the latter town to mail postcards to Aime Michel and to Pierre Guerin.



**MacDonald Observatory. 4 March 1963.**

Last night we succeeded in taking two spectra, respectively of IC 4296 and NGC 4258. I worked with Gerard and Antoinette, again using the prime focus, but tonight we suffer from poor visibility. There were low clouds to the North of us, and the moon was bright in the sky. It was 9:30 p.m. when we were able to start on our main objective, NGC 4258. The temperature on the telescope bridge was barely above freezing. The sky became overcast again, so we had to stop half an hour after midnight, after less than three hours of exposure, a duration which is too short for the deep-space objects we are trying to capture with the spectrograph. From our position high inside the dome I had a fine view of the Texas landscape through the wide opening, all the way down the mountain.

**MacDonald Observatory. 6 March 1963.**

We are aiming the telescope at NGC 4258 again. It is a galaxy where Courtes has reported a hydrogen spiral we are trying to confirm by taking a series of spectra under different angles. We had to stop after only a one-hour exposure, but we returned to it later and remained on target until 5 a.m.

After I close the dome and park the telescope I go into the darkroom to develop the film, having rehearsed every gesture so many times I am able to execute it in complete darkness. The result of all this work is a tiny blur striated with spectral lines, half the size of my fingernail.

**Austin. 30 March 1963.**

I am expanding my library, reading UFO books in English by Keyhoe and by Waveney Girvan. In spite of my interest in the subject, I *find* this kind of literature incredibly boring. The style is atrocious, many of the ideas are childish. The data alone may have some value. As for Charles Fort, author of *The Book of the Damned*, he is witty and well documented but sometimes he tries to prove too much.

**Austin. 9 April 1963.**

Along with the staff of the Astronomy Department I recently attended a seminar on models of the universe. The approach to the problem is fairly simple: you begin by assuming that the universe is uniform and iso-

tropic (with the same properties in all directions), and from this hypothesis you begin to write down the equations of its past, present and future behavior. Then you consider the body of observations that "prove" that the whole thing is expanding. By drawing the light cone which represents the path of the radiation that reaches the earth you can show that an infinite region of the universe is not observable. Next you "infer" that the entire thing is rotating, although you have just demonstrated mathematically that you were observing approximately zero percent of it. What kind of a joke is this?

The astronomer who gave the lecture was profoundly Christian and saw in cosmology the ultimate proof of the existence of God.

I came away very unsatisfied. Wouldn't the universe be large enough to have some parts that expand while others contract? Aren't we confusing local statistics and universal laws? And what about those fundamental, everyday phenomena that are still a puzzle to our current science, such as gravitation and consciousness? Shouldn't we try to find a reasonable way to account for them before we generalize? Until human thought has a theory of itself, what is the value of cosmology? The answer is that it is a useful and necessary exercise but it may not tell us very much about the real nature of the space around us.

The same criticism, of course, applies to the very idea of God. From some unexplained local phenomena that were reported at the dawn of history (the burning bush, the pillar of fire) people have erected the convenient model of an omnipotent creator. This is silly. The correct conclusion, in my opinion, would be to acknowledge that an unrecognized form of life and consciousness exists close to our earth. It is not necessarily cosmic in nature any more than our astronomical observations are necessarily telling us the shape of the entire universe. In fact, if there is a God, it is an information anomaly that would prevent the universe from being isotropic: It would violate the premises of the whole argument.

**Austin. 11 April 1963.**

A letter has arrived from Guerin, who is talking about spending his vacation in Alsace, at the point where three of our great circles intersect. Aime Michel wants to send his own Martian statistics to astronomer Shklovski in Moscow, who is rumored to be interested in extraterrestrial life. I don't think that will do any good. Tired of the term "flying saucer,"

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which sounds overly sensational and already implies a certain answer, I now refer to the problem as "the Arnold Phenomenon" after a celebrated witness, businessman Kenneth Arnold.

I am drafting a complete report on our work. I wonder what impact its publication would have. I also realize now that I need a doctorate if I want fellow scientists to listen to me, whether I talk about UFOs or about computer science, which I now find more interesting than astronomy.

**Austin. 15 May 1963.**

**To** what insights can higher consciousness take us? In my attempts to see more clearly where traditional teachings lead I find that the Rosicrucian tradition is still the most attractive source, the one to which I return again and again, perhaps because it is so humble and does not pretend to have all the answers.

What is that feeling, that attraction of an unseen presence that seems to be speaking to us across centuries of darkness? The elements of esoteric history I found in the book by Sedir given to me years ago by a friend at the Sorbonne continue to resonate for me. Behind all the jargon and the lofty words there is something to be discovered, a truth about ourselves that reveals its grandeur in the glow of candlelight, in the vague nuances of the night reflected by mirrors.

This is not the silly black magic that invokes devils or deities to gain riches and power. Nor is it the enchantment that comes with orgiastic dance and possession. It is a more subtle and demanding search; a personal, solitary investigation through one's own life, a confrontation with the very mind of the world.

As I study these texts it is also becoming clear to me that whatever else these old hermeticists were doing, they should be credited as the real founders of modern thought. In spite of its occasional obscurity this material represents a profound body of work. It may be the only significant and successful effort to preserve the intense spirit of earlier ages within the knowledge of modern science, even if I find it simplistic to invoke a "Supreme Guide of the Universe." Anyone who has looked at remote galaxies, as I did, through a deep-space telescope, should realize how far we are from understanding the higher levels of consciousness, how low we stand on the scale of cosmic things.

**Boulder. 3 June 1963.**

We are back in Boulder for a conference on the exploration of Mars, where two kinds of scientists who do not speak the same language are trying to argue with each other: the astronomers speak of the reflectivity of the green areas and the spectral features of the red areas, while the space engineers want to know if the green areas are edible and if the red areas will corrode the tracks of their vehicles.

I have just heard a talk by Dr. Franklin Roach at the National Bureau of Standards. He has recently served as a consultant to NASA in the scientific training of astronauts. Given the narrow field of view visible from the Mercury capsule, scientific opportunities are severely limited, he told us. It is even difficult for the pilots to recognize any constellations.

Roach also mentioned that Gordon Cooper was the first astronaut to see the Milky Way from space. He observed the zodiacal light, but he was unable to see the solar corona because of the scattering caused by the thick windows.

He also mentioned several phenomena reported by the astronauts, which did not immediately have explanations. Thus John Glenn saw some strange particles outside the cabin. It turned out they were attached to the capsule: He could make them go away by hitting the window with his fist. An observation made by Gordon Cooper has not been explained yet. It turns out that during the day the astronauts see the earth surrounded with a blue halo. At sunset, for a brief instant Cooper saw an intense orange luminosity which is not fully understood. The appearance of the sky, too, poses some problems. Cooper saw something like "haze" underneath his capsule.

Psychologically speaking, the worst fears of the experts about the negative effects of space have not been verified. Cooper kept his good humor throughout his flight, taking frequent short naps. The dreaded "split-off" effect (it was speculated that the astronauts might not want to come back to earth) did not take place. What did happen is a harmless illusion called "capsulo-centricity": the astronaut has the impression that he can make the whole universe move around his cabin and that he can bring the earth into view just by pushing a button, while he remains perfectly motionless in space.

**Austin. 10 June 1963.**

Central Texas is a paradise. There are woods and hills here, big oak trees, huge butterflies and endless miles of shoreline along the artificial lake where the Astronomy Department holds its Sunday picnics. People are relaxed and friendly, they seem to have all the time in the world. A small white wooden house on the edge of campus shelters our love. Squirrels run around the shaded roof, playing hide and seek with us. There is white dust on the dry roads. Behind us a wooden bridge goes over a creek.

Janine tells me she is pregnant. She is radiant, more beautiful than ever, calm and sweet. We miss nothing of France. If I had remained in Paris I might still be spending my nights tracking satellites, and I would have to watch every word I said.

The science work, however, is not as exciting as I had hoped, although I greatly admire Gerard de Vaucouleurs. He left France in 1946, incensed at the rigid attitudes of the astronomers of the time. He wanted to study planets and galaxies. The former were too close, and the latter too far away for traditional French astronomy, which concerned itself primarily with the fixed stars. When he was told that nothing new could be found about galaxies, since "Hubble had done it!," he became infuriated at such an absurd statement: Hubble, a great American pioneer of galactic research, was the first to acknowledge he had barely scratched the surface of the immense new domain he had discovered. Gerard went away from France, first to England and Australia (where he specialized in the little-known galaxies of the Southern sky) and later to Texas. He works very hard with his wife Antoinette, but there is no future for me here without a Ph.D., although I am busy with several large and interesting projects, including the preparation of their catalogue of galaxies for publication.

I was hoping to work on planetary problems, but this expectation has not materialized, except for a few efforts related to the Mars mapping project we have undertaken for NASA. Washington provides little support. It is as if planetary scientists were afraid of seriously tackling the real questions before them.

With the approaching Summer comes a stifling, muggy heat that slows down the thinking process.

**Austin. 14 July 1963.**

We went over to de Vaucouleurs' house for lunch today. We spent a pleasant afternoon with him and his wife. He brought the conversation to the subject of flying saucers. We talked about the questionable quality of the photographic data and about the Air Force's equivocal position. Clearly the problem is not as frozen here as it is in Europe. I remain confused, however, about de Vaucouleurs' own outlook. He is open and encouraging to me in his comments, but he always approaches the subject from a skeptical viewpoint, and he has not drawn out my deeper thoughts. I am reluctant to remain in Austin, in spite of his support and the friendly hospitality Antoinette and he have extended to us. I am anxious to move on with my research and perhaps to pursue a Ph.D. elsewhere. I have decided to contact J.Allen Hynek, in search of a more definite approach to the observational material.

The folks from *Fiction* have written to me: They will be printing one of my stories, *The Artificial Satellite*, in their special Anthology of French science fiction for 1963.

**Austin. 4 August 1963.**

We are awaiting Hynek's answer to my suggestion of a meeting in Evanston. In the meantime I am printing out our entire catalogue. I miss the good long talks I had in Paris with Aime Michel, that extraordinary mind, and with Pierre Guerin.

**Austin. 27 August 1963.**

We went to Mexico City for a week of vacation, and upon our return we did find an encouraging letter from Allen Hynek. I am answering him with the suggestion that we travel to Evanston to see him on September 7th.

This week away from Austin has given me a new perspective. We should not stay in Texas, beautiful as it is. Professional astronomy is a held with only a small number of scientists who hate one another and fight over tiny budgets, ignoring the giant forces that are reshaping the world around them. I admire de Vaucouleurs' insight and dedication, and I do not have any quarrel with the older generation of astronomers who did so much pioneering work, but I am discouraged at the atti-

tudes of many among the younger ones. They show little interest in other scientific disciplines, and they have no exposure to the wider industrial reality around them. They underestimate the real potential of computers, a fast-changing technology to which I am increasingly attracted.

I think it is criminal not to put more emphasis on planetary studies. In this respect I find it ironic to be working with de Vaucouleurs on the Map of Mars project for NASA. Our map will be ten times more accurate than any previous one, suitable for the reduction of the photographs that will soon be taken by the Mariner probes.<sup>2</sup> But in the process we are part of the last chapter in the history of classical Martian astronomy. From now on, all the important studies of Mars will be based on space probes. The great tradition of earth-based Mars observations which comes to us from Galileo and Kepler through Schiaparelli, Antoniadi, Maggini and de Vaucouleurs himself, ends in our machine and in this printer spitting out the coordinates of hundreds of fuzzy spots at 400 lines per minute.

### **Austin. 18 September 1963.**

Last Friday we flew away from Austin. We landed in Chicago at night, after a stop in Dallas. Janine was especially happy to make this trip: since her childhood she has been fascinated by the very name "Chicago." She has long had the intuition she would visit the city some day. Chicago and Samarkand.

On Saturday Hynek called us at our hotel and we had quite a conversation for a first meeting. It lasted all day and into the whole of Sunday: we had so much to talk about! He is a warm and yet a deeply scholarly man, with much energy and a great sense of humor, an open mind, and a deep culture that comes from the sophistication of his Czech ancestry.<sup>3</sup> It is hard not to be impressed by his sharp ideas and his eagerness for action. He has a lively face where piercing eyes are softened by a little goatee that makes it hard to take him completely seriously. He took us on a tour of Dearborn observatory, a charming old building among the trees of the Northwestern campus, at the very edge of Lake Michigan.

I had planned to take time out to look for a job in Chicago, but Janine and I were both tired on Monday morning and found no energy to go downtown. However, a new possibility has come up: Hynek has found out there is an opening for a systems programmer at the Techno-

logical Institute, right at Northwestern University. I will look into it seriously.

After reviewing all our results in detail for two days Hynek said it was imperative to publish them as soon as possible. This implies a series of decisions. First I will need advice to find a publisher. Perhaps my French literary agent will be able to give me the name of an American house he knows. It will be easier to pursue these contacts from Chicago rather than from Austin, which is outside the mainstream. Second, I realize that this book will be an unprecedented statement by an astronomer arguing for the reality of the saucer phenomenon. If I remain in Austin professional pressures are inevitable, since the department works closely with Harvard Observatory which is directed by arch-skeptic Donald Menzel, who has just published his second debunking book, *The World of Flying Saucers*. I do not want to put my friends there into such an awkward position.

Above all, I want to work closely with Hynek. There was an immediate bond between us. I am struck by the balance he has achieved between theory and practice; he understands solitary research but he is also a very good team builder. He has a genuine understanding of science but his sense of culture saves him from the pitfalls of specialization. His house in Evanston is full of books on all kinds of subjects. There are classical records everywhere, and current issues of the cartoon-filled *New Yorker* on the coffee table. He is an ethical man, with a realistic view of the politics of science and the role of the military. After our very first meeting he tried to call Colonel Friend to suggest that he join us in Evanston right away. I like this impulse, it comes from a man of action who can make quick judgments about people. Warmly, his wife Mimi offered to help Janine when the baby comes.

### **Austin. 21 September 1963.**

We are packing again, ready for Chicago where I did get the systems programming job. I will leave astronomy behind without much sorrow, to start a new career in computer science. A Pakistani friend is just returning from a tour of East Coast observatories, including Washington and Harvard. He shares my impression that the field is becoming dull: "All the people I saw," he told me, "seemed engaged in pedestrian work, without much passion, without fervor, without the driving ideas that could lift



their lives above the routine drudgery of academia."

We will take the big black Buick, driving through Oklahoma, across the Mississippi and the plains of Illinois. Janine is six months pregnant, as beautiful as ever, as enthusiastic as I am about moving to another unknown city, another life.

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### **Chicago. 19 October 1963.**

Barely one month after our first meeting with Hynek, we are settled in this furnished three-bedroom flat we are renting on the North side, halfway between Evanston and the Loop, as people call the downtown area. We have a large living room with wood everywhere, a bay window, and bookcases with glass doors on either side of the fireplace. Next to the living room, my little office looks over Bryn Mawr Avenue. At the other end of the apartment are the large kitchen, the porch and the back stairs. There are people living on the floor above us, about whom we know nothing. The owners, a cheerful Greek couple and their children, live downstairs.

On the Northwestern University campus in Evanston I audit Hynek's astronomy class when I am not developing computer programs for the Biomedical Department. My work at the Technological Institute—a large modern building which faces Lake Michigan—gives me access to the main campus computer and a more comfortable salary than the measly \$600 a month I was earning in Texas. Yet I have not recaptured my spiritual balance. I count on this weekend to renew contact with my friends in Europe. An exciting letter arrived this morning from Aime Michel: he is giving up free-lance journalism for a job with the French radio research center, where he will work under musical genius Pierre Schaeffer.

### **Chicago. 2 November 1963.**

Hynek and his wife came over for dinner Monday night. They were surprised, they said, at our fast transition from Texas. Things are also

moving rapidly on the research front. During his class on Thursday Hynek mentioned his relationship with the Air Force: just the previous day he had visited the Project Blue Book office in Dayton. He was going to need some research assistants, he said, to analyze the accumulated files and to test the investigators' conclusions. Having said this, he stroked his goatee thoughtfully and added that he had already discussed the idea with the Air Force, and that it had been approved.

I asked if citizenship or a security clearance would be required, since I am still a French citizen. Hynek answered that not only were the files unclassified, they might well end up in the trash if some general suddenly decided that the whole phenomenon was unworthy of study. He welcomed my involvement. Another member of the class, Nancy Van Etten, asked a pragmatic question: she wanted to know how much we would be paid. Not much, answered Hynek; and he gave her a copy of *Special Report #14* to read.<sup>4</sup>

Chicago. 9 November 1963.

My major concern now is to create a small elite team around which a real scientific effort could get organized. This brings my thoughts back to Michel and to Guerin. Aime is a remarkable and dangerous man. His imagination, coupled with a sharp sense of humor and a powerful brain often propel him too fast. Guerin is very different. He is an island of intense thinking, but he has to preserve an image of conformity inside the old astronomical citadel where he works. He cannot afford to let his colleagues guess what lofty thoughts are burning through his mind when he passes them in the hall. Actually, very few of them put a high priority on real science. They worry much more about their pensions, who will get elected to the Labor Union Committee, whose budget will be slashed next year.

Chicago. 15 November 1963.

The French manuscript of my monograph about the "Arnold Phenomenon" is on its way to a Paris literary agent who has promised to look actively for a publisher. Hynek has suggested that we hold the first meeting of our new UFO Committee at his house on Sunday. He told me frankly that he counted on me to be the driving force of this group, because he had practically no freedom of action with the Air Force. That

is a crafty way to use me without his taking any personal risk, but I am willing to play the game. I have resolved to come to every meeting with a written list of specific points to be discussed. My objective is to pull the problem out of the quagmire where it is stuck. I want to try and convince the Air Force that this is a serious scientific question, that it isn't limited to the United States, and that it isn't necessarily just the business of the military to investigate it. Current American attitudes about the issue are based on shaky statistics whose only strength is the appearance of authority of the electronic machine that produced them.

As I become familiar with the files, I am struck by their backwardness, compared to the level of sophistication we had reached in Europe. There is not even a general index of Air Force data, and no compilation of cases comparable to Guy Quincy's list. Even Hynek still talks as if landing reports did not exist at all.

Chicago. 17 November 1963.

We have just had our first meeting of the UFO Committee at Hynek's house in Evanston. Janine was there as well as Nancy Van Etten. We spent a great deal of time on organizational issues and on the definition of the problem itself. I still would like us to call it the "Arnold Phenomenon," or any such term that would break with the sensational and somewhat pejorative term "flying saucer," or the overly used and abused acronym "UFO." But the Air Force terminology is hard to change, and we are stuck with those awkward words "Unidentified Flying Object."

Mimi Hynek, who thinks the whole subject is utter nonsense, argued emotionally against me. She did not believe there would ever be a time when the topic would be seriously studied at a major university under the term UFO, Arnold Phenomenon or any other name. Hynek watched us fight, cleaned his pipe and refilled it in silence. Wisely, he avoided getting into the dispute.

My next objective is to develop a formal classification of sightings. Hynek finds the idea interesting, so I have started to read and to analyze the Air Force files, working from his own carbons of the Dayton documents. I extract the significant data, and I put them into our own file after translation into French. This will enable us to compute new, independent statistics.

**Chicago. 19 November 1963.**

It is practically on a full-time basis that I now work on the saucer problem. Janine, who is resting at home since her delivery is approaching, does a huge amount of work calculating the longitudes and the latitudes of the best sightings. This often leads her to a critical re-appraisal of certain cases and to correcting old errors in our documents. Now we need to test the very existence of Michel's *orthoteny*. The question is no longer to verify that his alignments exist, since I have already validated them. But I need to test whether or not they might simply appear as a result of chance. I have convinced myself that the appropriate methodology for this is simulation, sprinkling fictitious sighting points over a geometrical shape representing France, and systematically looking for all possible alignments. Without a computer, of course, this would be out of the question.

While my programs are running I spend long hours at the library at Dearborn Observatory, catching up with the recent literature on the physics of the solar system or making notes for our UFO study. I often hit theoretical snags, as I did in the correlation between the frequency of sightings and the distance of Mars, and I need to discuss such issues with Hynek. Fortunately his office has two doors, one of which opens up into the library itself. Thus I see him practically every day, even when we do not have a formal meeting scheduled.

**Chicago. 23 November 1963.**

The assassination of President Kennedy yesterday is one of those events history uses in order to show us how little we have progressed away from barbarism. It deprives us of a sincere man who gave the world a remarkable lesson in genuine democracy. Beyond this it puts a tragic halt to the acceptance of new social concepts, from civil rights to the conquest of space. Certainly the forward march of the Western world will have to be resumed sooner or later—science itself makes this unavoidable—but who will expand the framework of our political system, as Kennedy had started to do?

I discover with surprise to what extent I had judged and admired my new country through Kennedy's actions. This brutal death reminds me of the existence of a volcano of violent realities underneath the orderly unfolding of our best plans.

The real Beyond is not that which follows death but that which stretches underneath life itself. In this sense the saucers are a potential source of cultural and strategic upheaval, just like yesterdays killing in Dallas. Just as in Dallas, we are dreadfully unprepared.

**Chicago. 28 November 1963.**

The computer simulations have now produced some remarkable and unexpected results. Every evening I have been bringing home the statistics generated by my latest simulation program, to study them with Janine. There is no longer any doubt about the distressing fact they demonstrate: Aime Michel's networks *can indeed appear as a result of chance, with the same precision as the actual sightings*. This throws into question everything I have said and written before about orthoteny. The problem with the earlier tests was simply that they did not take into account the peculiar topology, the geometric shape of the French territory.

Accordingly I may have just destroyed much of what I was going to publish. I will know it for sure when I recompute the totality of the patterns Aime Michel had constructed by hand with his maps of France. I now expect to see the whole theory collapse. Of course a few exceptional alignments will remain ... and also the saucers themselves! The key to the mystery escapes beyond our reach once more. However I refuse to be discouraged. Perhaps it is even a good thing for us to go back to the proverbial drawing board. I am anxious to finish these calculations and to take a trip to France in a few months to discuss the results with Aime Michel himself.

Mimi Hynek, whose son Ross was born just a few months ago, has been very generous with us, giving Janine some practical lessons in the handling of babies. They have four other children: Paul who is about two years old, Joel who is a teen-ager, and two older kids who have already left home: Scott and Roxanne.

**Chicago. 3 December 1963.**

Tonight Janine sleeps at Grant Hospital, our son Olivier next to her. He was born at 6:48 this morning. They are both doing very well. While waiting for them to come home, I continue to read the UFO cases from the Air Force, some of which are as fascinating as the most sensational cases in the published literature. Captain Hector Quintanilla, the man who

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currently heads up Project Blue Book, has promised to visit us next month.

### **Chicago. 4 December 1963.**

This afternoon I saw Janine and my son at the hospital again. She is happily resting. We are both recovering from the tough period before her delivery: the move from Texas, the uncertainty, the trauma and the psychological upheaval that accompanied Kennedy's assassination, **our** intense work on the UFO files, and her increasingly disabling pregnancy. Our son is an angel: I am going to be crazy about this little fellow. Janine has a very pleasant, cheerful room: she is impressed with the way the hospital runs.

Our doctor, who was recommended by our landlady, is a Greek. So are most of the staff and the patients at the hospital. Since Janine's black hair gives her a Mediterranean look the nurses frequently burst into her *room* happily giggling in Greek, and they wonder what is wrong with her when she doesn't respond.

The first snow has fallen on Chicago. There is a festive mood about everything. The world is fresh and clean. In Evanston the snow-laden branches around the quaint old building of Dearborn Observatory give a picture of perfect peace.

In the used bookstores of Clark Street I have found five more volumes about flying saucers. I am now thinking of restructuring my manuscript into a more complete overview of the problem.

### **Chicago. 22 December 1963.**

Janine is home and we are getting used to life with a baby. I admit I am fairly helpless when Olivier cries in the middle of the night. As soon as I brought him back to the house with Janine, our landlady put honey on his lips, according to Greek tradition, "so he will be sweet all his life."

Hynek flew to Dayton again this week to visit the Air Technical Intelligence Center (ATIC) that has responsibility for Project Blue Book.<sup>5</sup> He reported on the results of his visit during our sixth meeting, which was attended by Stanley Roy, a new astronomy student. The staff of ATIC, he says, regards us very pragmatically as a group of nice young enthusiastic scientists who might eventually provide them with a convenient alibi if Congress, or the Air Force brass, ever ask pointed questions about the

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handling of the problem. By using us to verify their statistics they will be able to appear open-minded and genuinely concerned with proper scientific methodology. At the same time, however, they did not count on the amount of real work we have already accomplished. Captain Quintanilla and another officer will be here on January 16, and they have invited us to visit Wright Field. They told Hynek they would be delighted if the phenomenon turned out to be a massive, genuine scientific mystery: the Air Force would rush before Congress to get a multi-million-dollar boost in its budget!

During our meeting I stressed the need to inject new methodology into the field. Sooner or later I believe the Air Force will be forced to make such a change. Janine and I now find ourselves in a unique position: no one else has had access to the private documents in Europe and to the French and the U.S. military files as well. There are still many holes in our catalogue, of course, but the trip to France we are planning to take in March will give us a chance to complete the study of Aime Michel's files, which will fill most of the gaps.

Chicago. 23 December 1963.

In recent discussions with Hynek, I pointed out that the saucer question may well be part of a complex series of scientific realities, but it also plunges deep into mystical and psychic theories. I found him very receptive to this idea. We must also ask ourselves if an extraterrestrial intervention might have been a factor in man's early history, specifically in the early development of civilization and of biblical events. As Paul Misraki has shown in his book, the immense machinery of the Angels and the divine messages delivered by Jehovah amidst lightning and thunder could be interpreted as a celestial manifestation rather than a divine one. The return of such phenomena today could be explained by the need for some "unknown superiors" to boost our religious vaccinations. Some super-scientific group of cosmic origin, considering mankind as its own creation and seeking to experiment on us, or to guide us benignly towards galactic status, might behave as the saucer operators do.

Another question then arises: Has the future spiritual state of man already been achieved by some individuals? Have certain gifted men already achieved contact, on some plane, with those who may be guiding our psychic evolution?

**Chicago. 1 January 1964.**

The latest book by Serge Hutin, *Journeys to Elsewhere*,<sup>6</sup> does not fulfill any high expectations of occult revelations. In fact I am fairly disgusted by all the esoteric apostles he quotes, along with mystics of the left-hand, right-hand or middle path. I am growing out of my earlier fascination with the hermetic literature, where the truly important ideas are obscured by occult mumbo-jumbo. Every fragment of every bizarre document left by earlier ages is eagerly seized upon and commented on, with assorted hints of deep and mysterious meanings that are never brought out to be critically examined in the harsh light of day. Hutin's book is replete with such false wisdom, throwing into the same stew the science fiction of Maurice Limat and the esoteric writing of Zozimus!

What about alchemy? Its Adepts are said to have reached a total, definitive vision of the world. Let us set aside the minor questions of information theory this would imply. Hutin gives the names of medieval occultists and philosophers who indeed left their mark on the culture of their time, but they certainly did not reveal to us the final truth about the cosmos: why didn't they write, if they were so smart, about the structure of galaxies, or the atmospheric pressure at the surface of Venus?

Such mastery one attains exclusively through perfect asceticism and total abstinence, according to Serge Hutin, but wait! It can also be reached by sex, through tantric union, possibly combined with orgiastic ecstasy. Or simply by leading a healthy, sane life. All these recipes are contradictory, yet their advocates present each one as the only choice, out of which there can be no salvation.

There are hundreds of manuscripts and grimoires that are said to be filled with the details of the ultimate secret. They describe the Philosopher's Stone, the preparations for eternal life, the potions that enable man to lead multiple simultaneous lives, and to see God in all His glory. But all these published revelations contain deeply hidden secrets: In other words, you will need a Master to reveal what they mean. But no two Masters ever agree on anything\_\_\_\_Now consider the Adepts, who could change the course of history merely by raising their little finger, but who refrain from such intervention. They could live blissfully in eternal cosmic beatitude, yet we are told that they choose to be among men to appease their suffering. If they are so powerful, why don't they use their



powers to break the misery of everyday life, hunger, illness, war and slavery?

My impression is that what we have here is simply an interesting assemblage of ideas, images, visions and dreams gathered from disparate sources. Their authors extrapolate from a few interesting facts and interpret them hastily into eternal truths. To me there is indeed a fundamental reality of hermetic science, but it is not based on such visions. Everyone must find his or her own expression of it. *The spiritual path I have chosen is that of intelligence tempered by the fire of love, but always applied to accessible, solid, consistent, calibrated facts.*

Another concern for me is the degeneration of the esoteric domain into a variety of branches and sub-fields that stand in glaring contradiction to each other: Thus Alchemy is said to be a branch of Tantra, which is widely sub-divided. There are some twenty kinds of magic. The Kabala contains many domains, while astrology is split between several disciplines and methods that have nothing in common. All the fundamental principles are said to be absolute, inviolate, pure and true of all eternity, but they are also perfectly incompatible with each other and impossible to test against everyday reality. Thus Hutin tells us that everything comes from the Egyptians, who got it from the Tiahuanaco man, who was an Atlantean. Good. We finally have a solid base. But wait, if you go to the North Pole you will find a race of telepathic blond giants who also have all knowledge, while the truly supreme magicians reside in Tibet, where they hide inside the mountains, as everybody knows. The Ultimate Truth, in the meantime, is buried in the Amazon, while the Extraterrestrials mingle among humans on Main Street, going in and out of their base inside Mount Shasta. Is this clear? In the end such books read like the brochures of a travel agency, not genuine mystical texts.

I am willing to accept the hypothesis that there is a higher plane of perception, because that is confirmed by my own everyday experience. The "great initiates" are exceptional men who are driven by excellent intuition, but they do not have total understanding of the world and are subject to the same weaknesses as other people. A true Adept, asked about his knowledge, should only say that he is a "mere student." Those who brag about their wisdom are crooks and phoney. As far as Unknown Superiors, Superior Ancestors and other orders of Superiors in the cosmic barracks, their existence is not proven by any real facts. It does seem that

the history of Antiquity contains instances of contact between men and other races or alien cultures. But the knowledge about this contact may have been misinterpreted by priests and self-styled masters down through the ages.

Hermeticism contains many beautiful truths, so I would go too far if I said it is worthless and irrelevant to the modern world. What bothers me is that esoteric knowledge gets lost in its contradictions and in the contempt it implies for the concrete observations of modern science, the very source from which it should draw support.

**Chicago. 2 January 1964.**

When we go back to France I am afraid we may find nothing but an old people out of breath, with a wily old man at its helm, an elderly general who lives only for History. For me the recent history of France remains summarized in the Charonne massacre: what I saw there after the police riot will always be in my memory. The worst hours of Stalin, the worst crimes of the Nazis had a name: Tyranny. The killings at Charonne have no name.

Charonne is worse than the deepest horrors in the tales of Lovecraft. Mad policemen threw dozens of men, women and innocent children down the stairs against the steel gates of the locked subway station, and they beat them up with their weapons. Eight people died. The culprits were never punished. We cannot escape that simple fact. Such is the France that claims to hold up to the world the torch of freedom and civilization: What right does it have to speak of such things?

**Chicago. 5 January 1964.**

The seventh meeting of our UFO Committee took place this afternoon at Hynek's house. I am beginning to understand the expectations and also some of the frustrations of the Air Force. I also think it may be possible to get them to create a new scientific commission to review the cases. It is clearly towards the creation of such a commission that I should work. The first step is to convince Hynek of its utility.

The absence of research seems attributable to lack of interest among scientists as much as to censorship by the military. We cannot seriously accuse the Air Force of neglecting scientific research, since that isn't their job. It is the scientific community which is guilty of neglecting its duty to

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the public, by refusing to consider the observations for what they are, namely sincere, genuine reports of unexplained facts.

### **Chicago. 6 January 1964.**

My manuscript is finished but I despair of having it published by such academic houses as Dover in the U.S. or Dunod in France. Those people will not take a chance on a controversial subject, even ~~with~~ such a technical approach to the problem: What we are doing is *Forbidden Science*.

### **Chicago. 12 January 1964.**

At the eighth meeting of our UFO Committee we re-investigated the Mitchell case of 1963, when two pilots saw an. object over Nebraska. We called up the witnesses on the phone to get first-hand data. Their sighting is similar to that of the BOAC "Centaurus" that flew for nearly an hour in view of a cluster of dark objects. Genuine team spirit is building up among us. Hynek himself is now drawn into it. The major new fact is the realization that we are in a unique situation, as the only civilian scientists who have had access to the basic documents.

### **Chicago. 16 January 1964.**

We have just had an excellent meeting with the officers in charge of Blue Book, Captain Hector Quintanilla and Sergeant Moody. I come away from it with a clearer sense of how they perceive their role. Neither one of them has any serious training in science, and they make it plain that knowledge is not their business:

"The mission of the Air Force is *to identify, intercept and destroy* any unauthorized object that violates U.S. air space," Quintanilla told me. "In other words, if an unknown object is detected by ground radar or by a pilot we ask it to **identify** itself. If it doesn't we chase it. And if it tries to escape interception we shoot it down. It's as simple as that."

"What about the global nature of this phenomenon?" I asked. I brought up the French cases, the landings, and the humanoid sightings. They were not interested.

"It's none of our business if a Martian shakes hands with a baker in Brittany. Our responsibility is limited to reports from U.S. citizens. What we are looking for? Enemy prototypes, spy craft, anything unusual that we can understand in terms of technology."

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He gave me two examples from Blue Book files:

"About 1951 several technical observers and ground radar personnel saw a strange object that flew very high and did not match any published pattern. It was reported as a UFO. The sighting was immediately classified, because it was realized that the observers had only seen a U-2 on its way to the Soviet Union. Another time, a simple fisherman saw a strange object in the ocean close to shore. The report was checked as a matter of routine: the thing in question turned out to be a Russian submarine!"

In their minds every report must have a classic explanation. Accordingly they see no reason to deny us free access to all the files: we might even find interesting data on some rare forms of globular lightning. But not one American scientist has ever suggested there might be anything there, other than purely natural phenomena.

"Besides," pointed out the Captain, "we brought together five top scientists in 1953 to review the whole problem. They concluded that all the cases had conventional explanations."

"Wait a minute!" said Hynek, "I attended that meeting of the so-called Robertson Panel.<sup>7</sup> These men had been taken away from their regular work for a few hours only, they examined a handful of cases selected by the military itself. What could they do except to re-affirm their beliefs?"

I jumped in: "It would have been a lot more meaningful for the Air Force to bring together scientists who had witnessed the phenomenon themselves, such as Clyde Tombaugh or Seymour Hess."

"We always call experts," answered Quintanilla. "For example, we send all the mirage cases to Menzel, all the meteors to Dr. Olivier...."

I felt disgusted with that comment: "With that kind of approach, how do you expect to ever learn anything new? You will always find that the phenomenon is composed of meteors and mirages! What about the unexplained residue, which you claim is only a few percent? Who will be your expert for the residue?"

Perhaps the residue can be explained in classical terms, too."

What about the Loch Raven case?"

They agreed they couldn't explain Loch Raven, which they still carry in their own files as unidentified. Or the Levelland case.<sup>8</sup> Yet nobody is following up on such observations, which are unexplained although they are

very detailed and come from multiple, reliable observers. Since they do not fall into any of the explanation categories, these cases are closed. In the triumphant words of Sergeant Moody: "We have identified them as Unknowns!"

**Chicago. 17 January 1964.**

For the second day I met Captain Quintanilla, Sergeant Moody, Hynek and Nancy at Dearborn Observatory. We went to a restaurant in Skokie for lunch. The conversation revolved around the role of scientists in this problem. Hynek remains very prudent. He is clearly afraid of antagonizing the Air Force and of losing his contract, hence access to the files, if he makes any waves. I have no such reservation, so I speak up against the Blue Book approach.

"You underestimate the level of interest which exists among scientists in private," I tell the two officers. "Even if such scientists deny it in public."

"Carl Sagan himself is more interested than he would admit to his colleagues," adds Hynek. "At a recent astronomy meeting he walked up to me and told me privately that he had learned of my association with the Air Force."

Grumbling among our guests. But there is more.

"Why is Blue Book rejecting all the landing reports?" I go on. "Why ignore Aime Michel's well-documented accounts of humanoids simply because they seem fantastic? That is not a scientific criterion. Comets seemed utterly fantastic in the Middle Ages. Artists have left us engravings that show comets as the hand of God holding a bloody sword in the sky. Yet competent scientists took the trouble to study them. If they had rejected them just because the report sounded weird, where would astronomy be today? A scientist is supposed to be able to go beyond the report to the phenomenon itself."

Hynek is impressed by this argument, but I can see I am not getting through to Quintanilla, who has made it clear he wasn't concerned with science, or to Moody, who keeps looking at his watch.

**Chicago. 20 January 1964.**

A New York house is reading the manuscript of my book, which I will call *Challenge to Science*, and my Paris agent has given me the address

of one Mr. Henry Regnery, a publisher in Chicago, advising a visit. He met him many years ago, he says, and he was impressed with his business sense. Aime Michel writes that he has mentioned my work to Frank Salisbury, a plant pathologist and Mars expert from Colorado.

**Chicago. 25 January 1964.**

Waveney Girvan, who edits the *Flying Saucer Review* in London, sends me a text by Menzel that responds critically to one of my articles,<sup>9</sup> although it is written on a strict scientific level, and Menzel's tone remains civil throughout. He is the director of Harvard College Observatory and his only interest in flying saucers is to deny their existence. I appreciate the fact that he does not "pull rank," he does not use intimidation and scorn against me, as a senior French scientist, a Danjon or a Schatzmann, would undoubtedly have done. After all, I do not even have a doctorate yet. I am small potatoes next to Menzel.

**Chicago. 26 January 1964.**

Now I am reading the book by Gray Barker, *They Knew Too Much about Flying Saucers*. It describes the alleged intimidation of American investigators by mysterious "men in black," or MIBs. If such terrible beings existed, wouldn't they have neutralized Hynek a long time ago?

We only have vague theories about the nature and origin of the saucers. One could speculate that they may be coming from a temporal rather than a spatial source. Some of the objects change shape. Others disappear on the spot. The saucers observed on the ground do not seem adapted to long-term interstellar flight as we understand it. These are the facts that the believers like Keyhoe are sweeping under the rug because they contradict their preconceived ideas about UFOs. I begin to appreciate the predicament in which the Air Force finds itself. Furthermore, the objects are often invisible to radar.

**Chicago. 31 January 1964.**

Eleventh meeting of the UFO Committee. Stanley Roy did not attend. I showed Hynek the FSR article by Menzel, together with my answer. This is a touchy situation, since Menzel was Hynek's mentor at Harvard during the artificial satellite days. We have begun the systematic review of last year's cases.

**Chicago. 1 February 1964.**

Doubts. I am tired and upset, not seeing clearly where I am headed. I need to talk to someone about the data and what it means, but Hynek always remains on the fence, cautiously.

"All this is very interesting, but there isn't any evidence here that I could take before the National Academy of Sciences," he keeps telling me.

"What about these patterns? The time distribution, the behavior classes?"

"That's not as good as physical evidence. A genuine photograph, a piece of hardware: That's what we need. We have to wait for a really good case to show up."

For many years he has been watching, waiting for the one incontrovertible case that would make a sudden dramatic **difference**—the single big event that no one would be able to deny because the evidence would be overwhelming. That is the way the French Academy of Sciences was forced to admit that meteorites existed, when so many fell at once on a single little town that it became impossible to deny any longer that "stones fell from the sky." But most **scientific** discoveries do not happen that way.

I try to convince him that we may not recognize that "really good case" if and when it comes, unless we begin in-depth research, right now. My arguments move him but he still does not want to challenge authority. It is not so much that he needs the money he gets from the Air Force. Rather, he is afraid of losing access to the data, of being cut off. Secretly I also suspect him of enjoying the mystery of it all, the opportunity to fly periodically to Wright Field as a scientific consultant, while his astronomical colleagues are stuck in boring, routine jobs. Blue Book puts adventure and intrigue in his life.

Vaguely, I do think that Menzel is wrong, that there are flying saucers out there. The earth is only a dark cave far from the exciting places of the universe. We live in ignorance of the Champs Elysees and the Broadways that are glittering all around us in the cosmos.

All great changes have come through incredible facts, through the fantastic: the lives of **Tesla**, Newton, Kepler, Paracelsus are examples of it. I believe in the higher dimensions of the mind.

The wind blows over Chicago. The sleepy town listens to the rumble

of an engine that drones on somewhere to the North. The deserted streets are wet and mindless. The world dies, the world lives. The city is sad and heart-wrenching.

**Chicago. 28 February 1964.**

Meredith Press is sending back *Challenge to Science* with a rejection notice: "We appreciate the chance to read your manuscript but we do not see how to sell it profitably on the American market." Back to square one. Should I publish in England? To issue the book in France only would mean to doom my work to oblivion: Aime Michel's friends would surely applaud, the rationalists would reject it summarily, and nothing more would ever happen.

It is sad to realize that most Americans ignore the richness of life in Europe: the Spanish spirit, the Italian soul, the splendor of medieval mysticism in every French country church, the agony and the creative force of the intellectuals confronted with a changing world from Berlin to Dublin and from Stockholm to Prague. But what is Europe itself doing with all that wealth?

Some eras of history seem completely colorless to me: the eighteenth and nineteenth century in particular, except for the American revolution, leave me cold. The Sun King was boring, his Versailles a display of empty vanity only relieved by Moliere's gentle wit. The Philosophers created a form of rationalism that put humanity into a bureaucratic cage for two centuries in the name of "progress." Where is the great enlightenment they promised?

The last twenty years, on the contrary, have seen a remarkable shift of all our cultural systems under this rigid framework of historical rules. There is a powerful, dangerous, exhilarating feeling in the air. Today culture must meet reality, and vision must perish unless it leads to practical, effective actions. All of this was beautifully captured by the anonymous American schoolboy who wrote: "Schubert earned very little money; if he hadnt composed music we would never have heard of him."

**Chicago. 29 February 1964.**

Those achievements that are beyond the capabilities of a single country like France could be within reach of a unified Europe. The reason for this is simply mathematical: a single equation with ten variables has no



solution, but if you construct a system of ten equations with ten variables there is a single well-defined answer. If there had been a real chance for the emergence of Europe we would never have left Paris for Texas and Chicago. To create this great economic empire, however, will take more than merging the holdings of a few conglomerates under the control of big banks. It will have to be a human adventure, not a purely financial or political one. The current apathy of Europe is not making this happen. In France political parties and special interest movements are only yearning for the illusions of yesterday's stability and privileges, denying the need for change as long as possible.

The Left could have become the driver of Europe as early as 1950. On the contrary, its lack of imagination has made it a passive, idle mass. The problem is men, courage, will. On the other side of the political spectrum De Gaulle has launched France on a bizarre journey towards what he calls *Grandeur*, building symbolic monuments like the *Plan Calcul*, an effort to build computers for the captive Government market which will concentrate all the decisions about French electronics in the hands of a few technocrats<sup>10</sup> instead of capitalizing on the real potential of the young people. The country will find it hard to come back to reality.

### **Chicago. 1 March 1964.**

Fourteenth meeting of the committee. I am so fed up with the fanciful "explanations" given in the Air Force files that I am tempted to write a satirical book entitled *The Universe According to Sergeant Moody*. This would be a universe in which globular lightning lasts over twenty minutes, where meteors routinely make ninety-degree turns and where Venus rises in the North! Hynek agrees that such conclusions are an insult to scientific truth, and he continues to give me support, but when it comes to the Air Force he always takes an opportunistic course of action rather than staking out a scientific position and defending it.

It is at his request, however, that the Air Force has now declassified the final report of the old *Project Sign*<sup>11</sup> to enable us to review it. I find the document remarkable and open-minded. After such a fine beginning, why did the research sink into the quagmire we see now? Hynek is happy to be able to read the conclusions of this report to which he contributed, but which he was never cleared to read: it takes twelve years for such a report to be declassified, he told us.

**Chicago. 7 March 1964.**

Andre Gide wrote about religious belief in *Les Faux Monnayeurs*:

As a soul sinks into devotion it loses the sense, the taste, the need, the love of reality.... As for me, I care for nothing as much as seeing clearly, I stand in astonishment before the thickness of the lies in which the true believers love to wallow.

The true believers I am thinking of are not the bigots he was attacking but another sort of zealots, the UFO groups which clutter the intellectual landscape, eagerly awaiting their Venusian friends. Their influence is far from negligible: they release bubbles of irrational imagery through the social fabric.

**Chicago. 12 March 1964.**

Frank Salisbury, the botany expert from Colorado who is a correspondent of Aime Michel, called me up on Tuesday evening: He is preparing a new study of the Mars-saucer correlation. I promised to send him my data. Janine and I are now moving fast in screening the Air Force cases. During the day she checks all the index cards we have selected together the previous evening.

At Northwestern I have begun work on an interesting pattern recognition problem, getting the computer to "learn" letters of the alphabet even when their shape is altered. Some day computers will be able to read printed text and even handwriting. Naturally if we can do it for letters we could extend the program to other figures. This is the fourteenth computer program I have written for the Biomedical Department since last November. The main problem I have worked on is the analysis of respiratory cycles, with automatic identification of physiologically significant pauses. That program is now in production. I work closely with an Australian scientist named John Welch, who collects the data from human patients at the hospital.

**Chicago. 15 March 1964.**

Yesterday we had our sixteenth meeting of the committee in my office at the Technological Institute, after a quick dinner together at the cafeteria. Hynek told me he wanted me to join the staff of the observatory to man-

age the computer applications. Indeed I am tempted to return to astronomy, but I cannot leave my current job right away.

Our committee works hard on last year's cases, some of which are remarkable. The attitude of the Air Force in the face of the phenomenon remains consistent: open and motionless, like a lazy schoolboy yawning in the back of the class.

Janine, who has returned to full activity after Olivier's birth, continues her statistical work. We began our catalogue with 1,062 cases from Guy Quincy and we added forty-five reports from the French military files. After sifting through the documents accumulated by Aime Michel and Charles Garreau and after rejecting all the marginal reports we reached 2,864 cases in August of 1962.

Since then we have added another 700 cards based on the reading of various books and magazines. The Air Force cases we are screening now will be merged with this body of data: out of 3,318 Blue Book reports made between 1957 and 1962, we are only keeping 726 sightings as truly unexplained.

Not only has the catalogue expanded, but it has greatly gained in quality. New sources often overlay the old, doubtful cases as well as contributing new entries. Thus we have wiped out almost a third of the Guy Quincy catalogue from which we started. Many cases mentioned by Leslie were only bad quotations of Charles Fort: in such cases, naturally, we always return to the original source.

Chicago. 17 March 1964.

The Air Force files range from the dramatic to the grotesque and the utterly comical. Thus an old lady has reported a luminous thing in the sky. She was sure it was a spaceship. An investigator pointed out to her she had been looking at the planet Jupiter. Undeterred she wondered, as a good American should: "Does it belong to us, or to the Russians?"

Another witness came out of the building where he had his office and saw a spinning luminous disk in the sky. The remarkable fact is that the man was an eye specialist, who had been in utter darkness prior to his sighting, examining a patient's retina with an instrument that uses a spinning, luminous disk—

Next week we will be seeing France again, for the first time since we boarded the *Queen Mary*. We will breathe the air along the Seine, the

fragrance of oranges in *Les Halles*, the taste of the Chateaubriand at Doucet s. Aime Michel is likely to take us to some impossible restaurant in an unknown corner of Paris. In Pontoise, long-stemmed blue flowers will be covered with dew on Easter morning. Maman writes that life in France has become even more complicated than before, that prices are rising everywhere. The newspapers she sends me tell the story of the Bull computer company, mired in a series of absurd decisions in the name of a mythical plan for greatness: De Gaulle is killing innovation in electronics while trying to promote it.

It is difficult to make a choice between the two cultures. America is tough, greedy, egotistical, and truly magnificent in spite of all its faults. If only it were softened with a little humility, a little tenderness, it would easily reach to genius. It could become a beacon, a blessing for humanity everywhere. But I see mostly cold efficiency around me, and little recognition of those demanding desires described by Marcel Aym  in *La Jument Verte*:

In my imagination the call of luxuriousness raised heavy burning dreams, a priapic tumult.

American midwestern society does without such embarrassments as "painful concupiscence" and priapic tumult. The passionate man has no place, it seems, in this hypocritical, puritanical land. Feelings of indignation, anger or desire are regarded as abnormalities of behavior meant to be repressed, hidden away, banished to the couch of the Freudian psychiatrist. Americans strive to show themselves as cool, virtuous citizens. But in everyday life this attitude often denies the warm and tumultuous reality of the human heart.

## 8

### **Pontoise. 24 March 1964.**

The *Mens Magna* is an absurd machine I built years ago to amuse Eric and Denis, my nephews. It is a black tower of blinking lights and whistling sounds, impressive and useless. Yet I found it again with plea-

sure, here in the attic where I played as a child and where my brother's children now run their games. Pontoise is marvellous. Downstairs Olivier laughs with his grandmother, babbling happily. This attic is my watchtower. Up here every memory is a magic mirror.

France is like the *Mens Magna*: useless and pretty, absurd and surrealist. Yet I am tempted to live here again. This temptation is like a recurring death wish.

### **Marigny. 29 March 1964.**

We are back in fat, rainy, luscious Normandy, laughing with Janine's sister Annick, playing soccer with her little boy and eagerly reviewing the files of Aime Michel which cover the period from 1946 to 1954. They consist of press articles, notes typed by physicist Rene Hardy, a pile of letters from Aimé's readers and a series of observations volunteered by readers of *Le Parisien Libéré*, of more mediocre interest. These documents have never been screened systematically.

### **Marigny. 30 March 1964.**

France looks gray and complicated. For the first few days of our stay the Latin Quarter seemed strange to us, as if the proportions were suddenly all wrong. We must have become used to the vertical lines and the bold architecture of Chicago.

Guerin told us bluntly that what was left of French science had become a nest of snakes. There is so much jealousy and acrimony among his colleagues that some astronomers do not dare go away, even to use their hard-earned observing periods at the major telescope of Haute Provence, for fear that their budget might be cut back and their office re-assigned while they were away. Guerin himself has many enemies, not only because he is interested in our forbidden subjects, but because he does not follow the Marxist party line which has become *de rigueur* in French science.

### **Paris. 2 April 1964.**

We left Olivier in Pontoise and we came back here to make copies of Aime Michel's documents prior to returning them to him. I continue to feel depressed by what I see here. The people met in the street seem exhausted, disoriented. The weather doesn't help. Everything seems gray,

the rain is cold and miserable. People bark at each other in the streets, the shops, the subway.

I have met with Guerin again. He has read the manuscript of *Challenge to Science* and made a list of points to be worked on. I am too tired to attack them tonight. I will wait for Aime Michel's own reactions. Unfortunately he has inexplicably skipped town to spend a week in the Alps, where he owns a retreat away from the world.

#### **Pontoise. 4 April 1964.**

Still no news of Aime Michel. Yesterday morning we met with my agent, a retired gentleman who keeps a hand in literary matters by representing a few authors. He owns a grand apartment in an expensive section of the city. We discussed the French title. I decided on *Phénomènes Insolites de l'Espace*. He encouraged me again to pay a visit to Mr. Regnery in Chicago. Regnery's family, like his own, has roots in Alsace. Regnery understands the European mind, he argues.

Everything I see here leads me to think that France is headed towards economic and intellectual bankruptcy. The wind has gone out of the sails of scientific research. Publishing only survives by issuing masses of translations from the American and by jealously protecting a few islands, a few intellectual circles that are little more than tiny cults around people like Sartre, or the *Nouvelle Vague*.

#### **Paris. 6 April 1964.**

Occasionally I see and feel another world. It is a realm of colors, shapes and beings outside of us, entities foreign to our lives; a world most men cannot or will not recognize. It is our own universe I see, but through the eyes of another time.

In those moments I also perceive (more like a memory than a perception) the shadow of a vast new wisdom that has nothing to do with today's science. I dream of a body of knowledge that would encompass our emotions. Current thought is narrowly based on cognition, with the assumption that perceptual errors must be subtracted from the observed universe to yield the "true" picture. To my mind, without the perceptual errors that inject emotion and meaning into life there would not be an observed universe. Thought, consciousness, science itself: those are only big names thrown around by high priests trying to impress the

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common citizenry. They are mere labels stuck on the tiny emerging portion of a giant octopus which goes on swimming, undetected, unrecognized by our great thinkers.

I have dived below the surface. I have glimpsed the eyes of the octopus.

### Paris. 7 April 1964.

Another day lost to science and knowledge, and dedicated entirely to happy, idle walks through Paris. "Flâner," "promenade," are two French terms that have no accurate translation in the English language. "Flaner" gets translated into "loitering," which almost suggests criminal intent. In the United States one can actually get arrested just for walking aimlessly around.

Today, then, I have loitered from one end of Paris to the other. Along the Quais I bought *Letters on Astronomy* by Albert de Montemont, and Humboldt's *Cosmos*. I found Koestler's *Sleepwalkers* next to *Les Diaboliques* by Barbey d'Aurevilly. I happily confess I indulged in the mixture of scholarship and lust which is the only authentic Parisian pleasure.

The sun has finally come out, putting a more pleasant face on the irritations of the city. There is a thrilling, superficial force rising in the air, but it is not genuine change. I suspect that it comes from the mere passing from one fashion to another, from one set of meaningless words to another, words to which no deep idea is attached, giving rise to endless symbolic battles that have no contents.

I hear the television set blaring next door. For the last half-hour two idiots have been arguing about their definitions of poetry.

"The poet is, above all, an erudite ..." yells one expert.

The other fool disagrees:

"Poetry is a spark which ..."

Such useless fights, such pointless life.

### Pontoise. 9 April 1964.

The temptation remains strong to engage in comparisons, to measure Europe by American standards. I must reconcile myself to the fact that we are dealing with two different planets, among which such comparisons are meaningless. Yet I find the urge irresistible.

Americans are disappointing when they fall into dull conformity. At Northwestern the students themselves refuse to consider any idea that

isn't blessed by a book or stamped by some "authority." Within our own committee for example, Nancy and Stanley can't understand why I feel the need to invent a classification system for UFO sightings. It is as if I were asking them to break some terrible taboo, as if such new knowledge might make them different from the bland mass consciousness to which they aspire to belong.

Hynek shares my frustration: young men and women come to the campus expecting to find all knowledge in the books, he says. Their parents are in real estate, in accounting or law. The kids conform to the expectations of their families. In their eyes, various academic degrees simply measure one's level of acquaintance with the library. As soon as you set aside the books to ask them about the future, about new ways to observe the stars, about the possibility of life in space, they feel lost, scared, threatened to the very core of their being.

Aime Michel has come back to Paris. He is experimenting with telepathy, a domain where he argues that everything remains to be discovered. As he hobbles along, forcefully hitting his cane on the uneven stones of the streets in Vanves, he tells me:

"Even hypnosis has never been studied seriously. About thirty or forty years ago a few people—amateurs!—did some interesting experiments, then little more was said about it, the major issues were never resolved. We are sitting on masses of explosive discoveries that could go off at any moment."

Unfortunately an idea for which nobody has the courage to fight cannot win by itself. Europe is full of new ideas that die, stifled by the bureaucracy, by the intellectual mediocrity of its self-appointed cultural elite.

Last night in the house of Serge Hutin, my fellow Seeker of Truth under the Rose+Croix, I had the feeling of having reached one of those points of singularity from which an entire spiritual universe can be probed and explored. He showed us admirable paintings by the shy Belgian artist Rene-Maria Renner, who has no "name" in the art world, and seems happy to remain hidden in obscurity, pursuing and perfecting his vision. His work is a vast tapestry of humanoid beings floating in blue space, directing hordes of humans who carry spheres on their backs, or staring across infinity from the top of large boulders. They are as majestic as Mesopotamian priests. Their women have large, beautiful eyes on the



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sides of their heads, and lips on their nipples. They smile inscrutably as if reminiscing about pleasures unknown to mere humans.

What good is our technology, all our space research, if it does not encompass that experience of the heart, that inspiration, that hope of worlds beyond? And who are the strange beings under Rener's fantastic brush?

### Chicago. 14 April 1964.

Our mailbox was overflowing when we returned to the United States. Frank Salisbury has discovered a remarkably close correlation between our data and his new sighting frequency curves, based on the files of APRO, the Aerial Phenomena Research Organization in Arizona.<sup>12</sup> No one is arguing that this correlation proves that the phenomenon comes from Mars, but the internal consistency of the data is very striking. Another letter has arrived from Waveney Girvan, the editor of the *Flying Saucer Review*. He proposed one of his colleagues, a man named Gordon Creighton, as a translator for *Phénomènes Insolites*. Creighton is an accomplished linguist, retired from the Foreign Service, a great traveller, a former British consul in Brazil and expert on China.

Seventeenth meeting of our committee: more work on the Blue Book cases, which we continue to discuss in detail, one by one.

Following up on my agent's advice I went to see Mr. Regnery in his downtown office. We had an amusing exchange.

"What's your book about?" he asked me.

"Unidentified Flying Objects," I replied, already on the defensive.

"You mean Flying Saucers?"

I objected: "I am a scientist, Sir. The correct term is UFO."

He seemed to accept that, then he asked me bluntly:

"Do you ever ride in them?"

I was shocked. "I beg your pardon?"

"I mean, do you fly to Mars and Venus like George Adamski?"

"Certainly not!"

Now he seemed genuinely puzzled.

"Let's see, you're a scientist. So you must explain them away: Mirages, clouds, atmospherics?"

"No Sir, I don't explain them either."

"Then that's final: You don't have a book about Flying Saucers!"

It took me another half-hour to convince him that there was a third way to think about the problem, the way of science which looks at both sides of the evidence and tries to weigh it. In terms of publishing he is absolutely right, of course: the only books on the subject that have been successful with the public at large were either written by contactees like Adamski or by arch-skeptics like Menzel. There has not been a serious hardcover book on the subject since 1957, when *The Great Flying Saucer Hoax* was published by APRO leaders Jim and Coral Lorenzen. Even that book was issued by a small regional press, with very limited distribution and no impact on the public at large. As far as publishers are concerned the subject has been dead for ten years.

"Well, that offers us the opportunity to re-open it," I argued valiantly.

Mr. Regnery remained skeptical, but he has a daughter who speaks French: He will give her my manuscript to read.

### **Chicago. 19 April 1964.**

How could I help my friends in France get more support for their work on the borders of science? The truth is, they do very little to break out of their routine. Serge Hutin is a wise and quiet man who lives in Fontenay-aux-Roses with his aging mother. He "awaits the decisions of the Cosmos." Michel and Francoise have a small apartment behind the Pantheon, with no heat in the place, and they write on a freelance basis. Aime Michel may be the happiest of the group, reading the Greek classics in the original text in his little village high up in the Alps.

I don't have such luxuries, but I am acquiring solid computer experience at Northwestern, where I now make my living as a programmer for the Physiology Department. In one typical experiment we run simulations of the cardiovascular system. Columns of numbers express ventricles pumping blood, vessels expanding, lungs breathing inside the mathematical "model" that tabulates blood pressure and volume throughout the system. The doctor with whom I work puts on his white coat and begins his operation. I remain at the console and I keep my hand on the switch that will feed the real data into the machine.

Above the racks of electronic equipment is a glass panel. On the other side of the glass is an operating room with a surgical table. I see the doctor walking and talking with an aide who connects a tube with a measuring device to the aorta of our "patient." On the table, restrained by

leather straps, is a large dog with its chest open. The instruments provide readings that are converted into digital impulses. Cables go through the partition and into my side of the facility. A hundred times a second, pressure and volume data from the dog's heart and lungs are fed into the program and compared with the model, which recomputes all the parameters in time for the next data point, one hundredth of a second later. The project is designed to uncover new properties of the human cardiovascular system. Dogs, in this respect, are very close to humans, since the physiology of their circulation and respiration approximates ours.

### **Chicago. 25 April 1964.**

Hynek and I have just spent two days at Wright-Patterson Air Force Base with the Blue Book staff. It was a curious, eye-opening trip. Quintanilla and his wife met us at the airport as we landed in Dayton on Thursday night (April 23rd). The consensus was that it was too late to begin work. Accordingly the evening was devoted to drinking and dancing at the officers' club. I didn't feel like joining them, so I spent a few hours slowly getting mad, staring at my coffee cup and wondering what this festive beginning was designed to prove, or to hide. I had made the trip to work on one of the world's greatest research puzzles, not to listen to a syrupy record in some stuffy night club.

I did get into a funny argument with Quintanilla and his wife about "today's youth" and the strange music kids were listening to, notably the Beatles, which they find objectionable and violent.

"What do you mean, violent?" I asked. "Their songs say things like *I want to **hold** your hand...* You call that violent?"

"Well, you know what I mean, they encourage the wrong kind of behavior."

We never resolved our disagreement. We went to bed at one in the morning and I woke up painfully to begin work on the Base by 8:00 a.m. Only then were we introduced to the rest of the team, which occupies a few offices at FTD, the Foreign Technology Division.

The FTD building is a long aluminum structure with no windows. A model of a MiG fighter hangs down from the ceiling in the lobby. In the bathrooms are prominent signs, reminding the staff that classified conversations are prohibited in all public areas. Whenever we needed to use the facilities a staff member came with us and stood outside the cubi-

cle to make sure we did not engage in some act of espionage.

Quintanilla proudly showed me the UFO files and several metal cabinets full of "evidence" which had turned out to be chaff, random pieces of metal and ordinary junk. These are the items which the Air Force chooses to display in front of visitors who ask questions about the status of Project Blue Book. No wonder Congressmen and scientists always go away convinced that the whole subject is garbage! They have never been shown any of the really puzzling data. The whole thing is a joke, or a charade.

We had lunch at the fancy officers' mess, after which Quintanilla took us on a tour of the Base, notably to the edge of the very impressive section that belongs to the Strategic Air Command. Back in the office we continued to study selected files until 4:30 p.m.

The evening was spent at Quintanilla's house on the Base. Soon the lights were turned down, and dancing resumed. By the time the music stopped we had missed our plane, but never mind, there was another one later. The whole group agreed we should go into town for dinner. The women demanded more cocktails and more music, in an atmosphere of forced gaiety which drove me crazy as it became more and more dull and dreary, a parody of carefree happiness. I had expected to find many things at the Foreign Technology Division, but certainly not this absurd form of giddy entertainment.

It all ended with a race to the airport. We nearly had an accident on the way and naturally we missed the plane again. That new delay provided an opportunity to visit a downtown Dayton night club for more drinks and more dancing.

As I watched the entire staff of Project Blue Book wasting this precious time, my mind went back over the other things I had seen that day: the immense empty runways of the Strategic Air Command, with the rows of huge B-52s ready to take off on either side of the tarmac, and the openings of ominous tunnels nearby, where the crews were waiting day and night for a signal indicating that World War III had begun.

What kind of life is this? What have we done with the human soul? I thought about France: What did De Gaulle's *Grandeur* mean for these bombers whose wings were trailing menacingly in the grass like giant night moths? To them France was little more than a few minutes of flight time over a few acres of terrain. I did feel a breakthrough, a new under-

standing of the scale of things. When our party finally staggered out of the night club I had to look up at the sky, at the stars, to find a little reassurance.

### **Chicago. 28 April 1964.**

This morning I was working at the downtown medical facility when a technician from the Physiology Department ran to fetch me: Janine was trying to reach me on the telephone. There was an emergency, but no bad news, he said right away. I rushed to the nearest phone. Mimi Hynek had called Janine to relay a message from Allen: *A flying saucer had landed in New Mexico!* Captain Quintanilla had called him and he had jumped into the first available plane.

I turned over control of the computer to one of the assistants and drove home along the Lakeshore Drive as fast as I could. The observatory staff only knew that Hynek had flown out to Albuquerque on the 12:45 flight. Finally I reached Mimi: A saucer had indeed landed in full view of a policeman. It rested on tripod legs, she said, leaving traces on the ground. Hynek wanted me to join him at Kirtland Air Force Base, where he had alerted Security to expect me. However he also said he would be back in Chicago tomorrow, so there was really no point in my flying there. Although the case only came to Hynek's attention this morning, the landing happened four days ago, on April 24th, last Friday at 5:45 p.m., *when Hynek and I were just leaving Project Blue Book headquarters*. If this observation can be verified, it certainly comes at the right point to support the proposal for new research I presented to Quintanilla when we were all in Dayton.

### **Chicago. 30 April 1964.**

Are we at the beginning of a new wave of sightings? Newspapers are widely commenting on the New Mexico events and on Hynek's trip there. The witness in Socorro is a very sincere man, a highway patrolman named Lonnie *Zamora*. Hynek tells me he did not want to be interviewed before talking to a priest, because he thought he might have seen something diabolical. Now another landing is reported in Montana.

**Chicago. 8 May 1964.**

Things have calmed down a bit. On Tuesday night we convened the UFO Committee. Hynek described the Socorro landing scene in detail for us. There is no doubt in his mind that the policeman is telling the truth, but we still argue about the nature of the object. An electronics engineer named Bill Powers, who works for the observatory, has taken an interest in the ground trace measurements made by the police and the FBI. If he is right the egg-shaped object seen by Lonnie Zamora rested on a very sophisticated landing gear with four legs (not a tripod as initially reported) which was capable of equalizing pressure on its points of contact with any rough terrain.

I have developed a mathematical formula that tracks the probability of being visited by a space civilization as a function of its distance from us. The most probable origin of such visitors would be a solar system only a few hundred light-years away from us. That optimum distance is a compromise between the number of civilizations expected to exist (which grows like the cube of the distance from earth) and the likelihood that they can acquire knowledge of our existence. Such knowledge is expressed by an integral function which decreases as one goes farther and farther away from our Sun.

**Chicago. 10 May 1964.**

The committee is scheduled to meet today for the twentieth time, at Dearborn Observatory. Hynek is writing his report on the Socorro landing and he wants us to critique it. He can no longer deny that there are indeed unexplained landings, accompanied by humanoid sightings. The Socorro case reads like an incident right out of the French files of 1954, except for the insignia seen on the craft. Our discussions on this point are intense. Unfortunately the Air Force analysis of the Socorro soil samples is not turning up anything of interest, other than eliminating rocket or jet propulsion as a flying mechanism for the strange craft.

I have given Hynek a copy of an article I wrote about the 1946 Swedish reports of "ghost rockets," a series of events of which he was unaware. He said he would like to quote it in his forthcoming interview with CBS on Tuesday.

**Chicago. 24 May 1964.**

Plenty of new ideas have been inspired by the Socorro landing and its sequels. I save them for a new book I have undertaken, which will be called *Anatomy of a Phenomenon*. I will write this one directly in English, because I do not want to repeat the mistake of having a manuscript languishing in the drawers of publishers in Paris, at the mercy of their capricious decisions, and then having to go through the agonizing process of yet another translation.

Gerard de Vaucouleurs wants me to go back to Texas in July as a consultant on the NASA Mars project. In Chicago the weather is magnificent. Janine and I often walk through the park or along the shore of Lake Michigan, pushing little Olivier in his carriage. I feel very happy. On weekends we find a spot in the grass for a picnic in the park, we play and laugh together.

**Chicago. 28 May 1964.**

Screening Aime Michel's old files I stumble across these wonderful words of professor Andre Danjon, the pompous director of Paris observatory: "Our good old earth is made exactly for us and it would be very wrong to hope to find something better elsewhere."<sup>13</sup>

Every day my work still takes me to the Northwestern medical school where I continue programming our model of the cardiovascular system.

**Chicago. 4 June 1964.**

Bill Powers is becoming a close friend. He is a tall, energetic fellow with a fine sense of humor, a maverick cybernetician trained in psychology, a great reader of science fiction. In recent discussions with him and with Hynek I stressed that our UFO Committee had come to the limits of its usefulness. They concurred: we need a new study group, a real working team, staffed by scientists rather than by students. We have just had an informal meeting at his house to consider the idea further. I brought Janine and two of my own colleagues, an engineer and a mathematician named Carl De Vito. A couple of astronomers and Bill Powers completed the group.

**Chicago. 28 June 1964.**

Janine, who has been staying home with our son for the last six months, starts work again tomorrow at a downtown data processing consulting firm. We have hired a warm and homely girl named Hélène to come from France and take care of Olivier. Next to my typewriter a pile of pages is mounting: The first draft of *Anatomy of a Phenomenon* is practically finished. Gordon Creighton writes from England that he has already translated half of my earlier French manuscript. I am beginning to understand that I will never be able to run my own research projects here if I do not obtain a doctorate, so I have reluctantly decided to embark on a Ph.D. program at Northwestern, although that means going back to school and taking again some mathematics and engineering courses I passed years ago. Chicago is stifling hot.

**Chicago. 10 July 1964.**

I no longer find the calm retreat into myself or the concentration I need in order to write. Since our return from France I have gone through a period of dry, helpless disappointment, even though Chicago is as magnificent next to its big open lake as Paris is frustrating and bureaucratic.

In a couple of weeks I will go back to Texas on a consulting assignment. It should be interesting to see how the place has changed since last year.

**Chicago. 18 July 1964.**

Since I finished writing the first draft of *Anatomy of a Phenomenon* my mind seems to have dried up. I float in little eddies at the foot of the waterfall. What will happen when both of my books are published? I have to expect many misunderstandings. The believers in spaceships and cosmic brothers will hail me as a supporter of their bigotry, and the Rationalists will attack me without taking the time to examine my arguments. My only long-term hope is to nudge Hynek and Blue Book towards some fundamental changes and more research.

The clarity of vision which the true seeker gains through solitary work also applies to spiritual attitudes. It endows him with the right to go forward in life without fear. While he gives up the gratification offered by membership in some recognized movement or church, he gains a far



greater privilege, the freedom to inquire into the nature of his own soul.

The notion of the "good yet frightful God" of the Bible and of the Gospels seems like a swindle to me: it is the biggest, most cruel confidence game in history. Are we really supposed to be scared of some plaster god surrounded with little blue angels? Simple human dignity should make us reject all that with indignation. That does not mean we should be ashamed to kneel on the earth and cry like children when we contemplate the evils of mankind and our pathetic weakness.

It is man I fear, his lack of respect for himself and his lack of reverence for everything sacred around him. I do not fear him as the creature of some unknowable God but as an unreliable entity, biased and unpredictable. I fear man's stupidity and violence.

**Austin. 25 July 1964.**

Nothing has changed here. I spent a happy evening in the poorer part of town with a friend who grew up in this sleepy capital of Texas. Old cars drove by, kicking the dust from the dirt streets. It settled on the trees and the dry bushes. The glowing twilight lingered. Doors were slammed, people sang, phones rang. Thankfully, a breeze was felt throughout the wooden house. As we made love, drapes shuddered in the hot night.

**Austin. 26 July 1964.**

Between two computer runs, I daydream about things to come. I have fun writing a novel of our future life in my head. It would begin in a blue bedroom, sealed from the world by blue drapes. The bed would be in one corner, next to a purple chest of drawers and a blue lamp. The carpet would be gray, with vulgar white furs carelessly thrown over it. A big sofa of black leather would face two tiny chairs, lilac and blue, near a small table where we would take our coffee and our ease.

In an oval-shaped golden frame set on a bright red background I see our private map of Tenderness. Mirrors, oil paintings, drawings, small fantastic motifs in black and purple are everywhere. Black shelves too, with golden nails along the edges, supporting a vial in which a bloody heart swirls ominously.

In another corner of my vision is a statue of a green faun wearing a hat of fire. Around his neck is a collar adorned with rectangular red stones.

Like the Genie of the Bastille he is stepping up on one foot, holding an orange ball in his upraised hand.

The entire house is dripping with sumptuous things. Along the staircase foxes hang by their tails. As an additional note of bad taste, the upstairs rooms are bathed in red light. A huge color television set brings a note of stark reality into this oppressive atmosphere. A large window opens on the vast expanse of the attic, made visible at that level by the subtle play of mirrors. Twenty suits of armor stiffly stand at attention there, under the ceiling beams. They resound with an astounding noise when they are hit by all the bats who live there.

Stravinsky's *Petrouchka* and Rachmaninoff's *Second Concerto* are heard over this tumult. Occasionally a blond girl of great talent but little virtue is brought over. She takes off all her clothes, keeping only one ornament, a wide belt which bears the Arms of Zagreb University. Sitting on the skin of a rare blue tiger, she improvises on the harp, inspired by the random motion of two Chinese fishes swimming in a large sphere which is lowered from the ceiling for this express purpose.

In the depths of our park three alchemists spend their time in prayers and dangerous experiments in a large brick house. Silent assistants, their faces hidden behind masks of black velvet, use marble stretchers to bring to their masters a steady supply of cadavers. Ignoring their sinister traffic, we wallow for hours in the delights of intimate conversation. And woe to the mischievous angel who would steal a single strand of your hair, woe to the bird who fails to sing, woe to the toad who doesn't jump out of the pond to greet the dawn with us, as we prepare for another long day of dreaming.

### **Chicago. 1 August 1964.**

From the *Guide of Mysterious France*, published by Tchou, under the heading "Jasseron":

In Tharlet Wood some spirits called *Senegouges* used to gather around an oak tree. One day a farmer fired at them with his hunting gun, and he heard a human cry. The *Senegouges* have seldom been seen since 1815.

**Chicago. 13 August 1964.**

Harvey Plotnick, a young editor with Regnery, called me last week at the Technological Institute. He had read my manuscript, liked it, and wanted to discuss publication. We have reached an agreement: They want to issue it as *Anatomy of a Phenomenon* (the title was Harvey Plotnick's idea) but I must restructure the book. Yet I cannot work on it until I complete my report on the Map of Mars.

**Chicago. 22 August 1964.**

I am practically rewriting the whole book from the beginning, sorting out the figures and eliminating the passages that are too technical. I have improved my style as I compiled the book: "All you have to do now is to rewrite the beginning like you wrote the end," says Harvey with humor, remarking on my progress.

We are living through a curious period. We have never had so much freedom to organize our life. We have never been so happy. But we both feel very tired. Janine works long hours downtown while I drive to Evanston every day. We have temporarily solved our financial problems thanks to the money from my consulting project in Austin, but we are still unsettled. Life in this country creates such an energy imbalance that it is imperative to define wide cycles of consciousness in one's own life, or be swept away.

**Chicago. 24 August 1964.**

Dr. Hynek and I agreed over lunch today that I would start working for him at the observatory in September. I gave him a draft copy of *Anatomy* to read, asking him to keep it confidential for the time being.

**Chicago. 27 August 1964.**

I had a shock when I received the latest copy of the *Flying Saucer Review* from London yesterday. The cover bore a big red title: MENZEL versus VALLEE! This made me very angry at the editors. And today, a letter arrived from Menzel himself, dictated from Portugal. The tone is not so triumphant any more, since I have shown he was wrong by a factor of two when he tried to disprove Aime Michel's alignments. I am gaining some respect for Menzel through this exchange. He does prove what he

states, namely that many UFO reports are explainable. We already knew that. But his books push the evidence too far. They are like those manuals on Natural History, as Jonathan Swift says, where the elephant is always much smaller than reality, and the flea much bigger!

**Chicago. 5 September 1964.**

As I negotiate with Regnery, I have managed to keep the presentation of the book on a sober and straightforward level. Before UFOs can become a valid object of research for science, a wide public needs to get used to the idea that the phenomenon is the product of an intelligent force.

I do want to call attention to the facts, but I want to do it with dignity, and I want to remain behind the scenes. Regnery would like me to create a media sensation, to promote the book aggressively. That would be a mistake. I am not an entertainer, or a missionary calling for some new crusade. Perhaps it is my childhood in wartime Europe that has convinced me that the most important changes occur in secret, behind the curtain, undetected by superficial observers. Also, I continue to question my own objectivity. I am finishing *Anatomy* with an odd mixture of joyous anticipation and fearful trepidation. Koestler found the right word when he spoke of scientific researchers as "Sleepwalkers."

**Chicago. 9 September 1964.**

All joy has disappeared. What remains is fear, unsettling solitude. My arms try to reach for shapes that vanish. I have slipped my moorings. Something has broken. I drift away.

The fervent hopes of my search are tainted by low discouragement, a vague notion that all desirable things are suddenly beyond reach. Life sometimes appears unmanageable. The hot nights make things worse. I reel angry, without knowing why. Janine comes over with a kiss, she wants me happy: everybody wishes me well. But I feel like a jet fighter helplessly grounded in a beautiful field of violets and primroses from which it cannot take off.

**Chicago. 18 September 1964.**

My work is changing. The Astronomy Department will now cover a quarter of my salary under a new project on stellar evolution, Hynek's

scientific specialty. At the biomedical center I continue the computations of respiratory mechanics I have started with John Welch.

Last Sunday I finished retyping the manuscript of *Anatomy*, and this morning I had another argument with Mr. Regnery and with Harvey Plotnick over control of the book's presentation. To begin with, I do not want the dust jacket to mention that I work at Northwestern University.

"How will the public know that you're not just another crackpot like George Adamski?" asks Plotnick.

He is a smart fellow with a very abrasive way to ask direct questions.

"If you understand my book you must know the answer."

I am probably too careful, too conservative, but I do like to stay behind the scenes. Regnery, on the other hand, has to sell books, so I understand their motivation and I feel like a fool.

As a result of our work together, Hynek's position is finally beginning to change. He took my draft manuscript with him when he went on vacation in Canada, as he does every summer. He liked it and sent me a note of encouragement. Today he read to me a Foreword he had composed while reading the book, but eventually he decided not to release it: He is still afraid it might compromise his position with the Air Force and his colleagues. But the gesture was significant: Hynek had declined to write a Foreword for Menzel's book.

There is almost a mystical atmosphere on this little campus in Evanston, in the sweet rains of Autumn.

#### **Chicago. 24 September 1964.**

Today is the tenth anniversary of the sightings along the now-famous Bayonne-Vichy line, and my twenty-fifth birthday. Long gone is the time when I wrote, after reading Aime Michel's book, "I hope the events will wait for me . . ."

#### **Chicago. 27 September 1964.**

Lunch at the Orrington Hotel today, with old Mr. Regnery and Harvey Plotnick, to resolve the last marketing issues. I introduced them to Hynek. He was impressed, as he told me later, by my firm stand against any fancy promotion.

Menzel has just written to Hynek about Socorro, in his usual absurd way. "It sounds like a hoax, or perhaps a hallucination." In the *mean-*

time the Air Force continues to look into a curious fact I have uncovered: the insignia seen by patrolman Zamora looks very much like the logo of *Astropower*, a subsidiary of the Douglas Aircraft Corporation. I found the logo in an ad they recently published in an engineering journal. I am suspicious of this aspect of the sighting. To my knowledge there has never been a genuine report of a saucer with an insignia painted on the side. Could the Socorro object be a military prototype?

### **Chicago. 29 September 1964.**

Hynek and I eagerly discuss Socorro at every opportunity. We disagree about the relevance of the landing, which he often rejects in frustration by saying:

"What can we do with it? It's a single-witness case. How do they say in Latin?"

"*Testis unus, testis nullus*. Single witness, no witness."

He laughs good-heartedly. "I always thought it meant having one testicle is like having no testicles!"

"If we fail to follow up the single-witness cases, how are we ever going to discover if there were in fact other confirming witnesses?"

He brushes my objections aside. "I can't exactly rush over to Carl Sagan or to Donald Menzel with this case in my hand and tell them, look fellows, here is the proof you've been waiting for!"

He would like to be able to explain away Socorro because of its implication that the diminutive pilots are a real factor in the phenomenon. It is true that there is something absurd, even ludicrous, about the humanoids. Yet they are real.

For me, I would also be tempted to discount Socorro as an isolated observation of some secret prototype, perhaps a test of a moon landing module, if it weren't for the fact that a genuine wave of sightings is actually under way, all over the world. Hynek called me again yesterday: he had *just* received a package of recent sightings sent to him by Blue Book, snowing a significant rise in the number of reports. We will have lunch tomorrow to compare our data.

### **Chicago. 11 October 1964.**

The Hyneks came over for dinner at Bryn Mawr on Friday night. We had a very relaxed evening. I showed them the illustrations I plan

to use for the book.

Yesterday, Bill Powers and I drove to Wisconsin with Mimi and Allen for a meeting of midwestern astronomers. I had noticed that the witnesses in a sighting that took place in Monticello were living in Madison, so I insisted that we should take the time to meet with them in their home.

This excellent case may force Mimi Hynek to change her mind about the phenomenon. She continues to be firmly opposed to my pleas that UFOs should be considered in the mainstream of science. A strong-willed person, she is a militant member of the League of Women Voters, and *an* ardent follower of Chicago politics. She considers herself a realistic modern woman, and realistic modern women don't believe in all that UFO nonsense.

I do think the witnesses impressed her: they are a young couple working in the anthropology department of the University of Wisconsin. The wife's mother and sister were with them as they drove between Monticello and Argyle. They clearly saw a huge formation of lights that simply cannot be explained as natural phenomena.

#### **Chicago. 18 October 1964.**

Hynek has mentioned my forthcoming book to Quintanilla. He will take the manuscript to Dayton on his next trip. I want the Air Force to read it before publication, out of courtesy if nothing else. Yesterday the mail brought Gordon Creighton's translation of *Phénomènes Insolites*.

The end of the week has seen some remarkable world events: Nikita Khrushchev has relinquished power in the Soviet Union, while China tested its first atom bomb. I spent this Sunday afternoon and the whole evening thoroughly reorganizing my files in preparation for the voluminous correspondence and the new data my book is sure to bring. I am entering a new phase of simplification, of alchemical distillation. I feel winter coming near, my winter.

# 9

**Chicago. 24 October 1964.**

What is happening on the moon? Aime Michel has written to me that some of the unpublished photographs from the Ranger missions showed well-defined formations which looked like large artificial domes. His information was based on the fact that Dollfus, the current president of the Planetary Commission of the International Astronomical Union, has been urgently called into conference by Kuiper, the astronomer in charge of lunar photo analysis for NASA. The purpose of the meeting was to discuss, as he put it, "whether or not it is appropriate to release the photographs." The press has been kept in the dark, and so have the Soviet members of the commission. What does it all mean?

For the last few days Hynek was in Boston, attending a conference on balloon astronomy. I caught up with him on Wednesday. We had lunch at his house.

"Do you know who I met in Boston?" he asked. "None other than Audouin Dollfus. I was frankly surprised at his demonstrations of friendship. He showered me with his attentions."

"Perhaps there is something to those stories of mysterious formations on the moon, and he is having second thoughts about the possibility of UFOs in the solar system ..." I suggested.

Hynek shook his head. He was not as excited about the lunar imagery as Michel and Guerin, he said. He reminded me that Bill Powers had already *noted some peculiar formations on the* photographs that had been published much earlier. They contradicted the common geological theories about the moon, but they certainly did not show any spacecraft or any artificial structures.

Now comes a letter from Guerin. He writes that Boyer, a Pic-du-Midi astronomer, was at Meudon observatory when Dollfus received Kuiper's letter. He was literally "decomposed" when he read it. Before Guerin could learn anything more Boyer had gone back to his eagle's



nest in the Pyrenees and Dollfus was on his way to the United States. In Boston we find Dollfus suddenly being very friendly towards Hynek, who wonders about this abrupt change of attitude. From Boston Dollfus flies to Tucson, Arizona, for reasons unknown.

Armed with this latest information from Guerin, Hynek acted quickly. He called Dick Lewis, a reporter friend of his at the *Sun-Times*: "Find out why the press is being kept away from the most recent Ranger findings," he told him. "The photographs may be censored." Lewis got excited, picked up his telephone and called Shoemaker, head of the Ranger project. "I don't have time to speak with you," Shoemaker said, "because I'm flying to Arizona to meet with Professor Kuiper."

This confirmed everything we suspected, so Dick pressed on with his questions. Shoemaker eventually confessed that about twenty photos showed "peculiar formations" similar to those Bill Powers had noticed. They could be just large round rocks. They could also be artificial domes, or even landed spacecraft. In a later conversation with Dick Lewis, Professor Urey stated that he did not have a natural explanation for them.

#### **Chicago. 31 October 1964.**

Returning from Wright Field today, Hynek tells me that Quintanilla has made only a few comments after reading *Anatomy*. The Air Force does not want to either endorse or criticize my book officially. That is fine with me, of course. But he conveyed his personal opinion that the work was outstanding. He added that he was impressed by the extensive research it represented.

Guerin and Aime Michel *continue* to be very excited about the Ranger affair. They think we may be close to having the proof that there are extraterrestrial intruders in our solar system.

For the last ten days I have been trying to convince Hynek to prepare a research proposal that would bring an exclusive contract to Northwestern for the analysis of UFO files. Under the terms of the contract, we would pool our experience and our documents, making them available for the first time to the scientific community at large.

#### **Chicago. 2 November 1964.**

The UFO Committee has completed its work on the Air Force statistics. Stanley and Nancy have gone on to more lucrative pastures. Our

new group, composed of scientists and professionals, met today for the first time at our Bryn Mawr apartment. We drank four pots of Janine's coffee, ate a big strawberry cake, and proposed tons of ideas, some of which were new and exciting.

As I kept talking critically about "the Air Force" and its misguided policies, citing recent reports from Major Keyhoe's *National Investigations Committee on Aerial Phenomena*, Hynek laughed and rebuffed me:

"Jacques, you'll have to understand something those UFO believers over at NICAP have never, never wanted to get through their heads: *There is no such thing as 'The Air Force'!* There are various clusters of military brass within the structure of the government, and they have many diverse interests and conflicts among themselves. What you call the Air Force policy' is only the attitude of one group which happens to have the upper hand against the others for a little while."

"There must be some authority to which they respond?" I said.

"Everything works as if the only thing the Pentagon reacted to was a little old lady in Nebraska who happened to be the widow of some stockbroker who left her 51% of General Motors and 60% of General Electric when he died. If that woman ever sees anything alien in the sky, or if she reads in the *National Enquirer* that a dozen Martians have just landed in Florida, she gets very scared and she calls her Congressman. Since he counts on her to put up most of the money for his next election campaign, he gets very excited himself and he writes a terse note to the Secretary of the Air Force, demanding to know what they're doing to protect his constituents with all their billions of dollars!"

We all laughed, but Hynek went on more seriously. "The only occasions when I have seen the Blue Book staff working really hard were when they had to answer such inquiries from Capitol Hill. The Congressman in question, if they antagonize him, could well cast a vote that would affect the Air Force budget at the next hearings. So public relations are a big concern, a very high priority for them."

#### **Chicago. 7 November 1964.**

The Ranger affair is becoming more clear, thanks to Dick Lewis, who published an article about it in today's *Sun-Times*, cautiously discussing rock formations that are visible in some craters and whose origin is unexplained." Neither Lewis nor Hynek have been able to confirm the fact

that *some* of the pictures taken with the P-camera of the spacecraft may have been kept secret.

In the midst of all this speculation I feel tired and morose. All of a sudden the idea of going back to France permanently seems less absurd. Here there is no time to think. Once my two books are published perhaps I ought to drop all flying saucer research and try to complete my doctorate in two years. Leave Chicago. Get back to other pursuits. A good computer scientist can find a job anywhere in today's world.

#### **Chicago. 8 November 1964.**

All weekend long I have worked on the card file. My mother has mailed to me Aime Michel's documents for 1957 and 1958, which add to the mass of data. A strange fervor is raised within me when I engage in this slow, painstaking work. I feel like a Benedictine monk translating the Fathers of the Church. Will this be useful to someone, some day?

The UFO problem keeps changing. Many pioneers like Ruppelt, Wilberr Smith and Carl Jung<sup>14</sup> have passed away. Yesterday I learned of the death of Waveney Girvan, editor of *FSR*. I will not forget that he kindly published my first articles and supported me in my early efforts. I am sorry I never met this Celt with the curious, mystical mind. The world is a vast, changing place where I feel lost.

#### **Chicago. 11 November 1964.**

On Monday night we had another meeting with Bill Powers, Carl Hynek, Harry Rymer and his wife, and concert pianist Samuel Randlett. They form an original, stimulating group. They offered me many useful comments on *Anatomy*. At Henry Regnery, Harvey Plotnick has now annotated the first half of the manuscript. He has shown me several mock-ups for the dust jacket and we have begun the tedious process of requesting permissions for all the quotes and figures.

#### **Chicago. 23 November 1964.**

This evening my desk is clear and my time is free, for the first time in many months. Bill was right about the moon photos. It turns out they show nothing but natural formations, although Guerin has correctly surmised that new theories of lunar evolution will be needed to account for them.

Janine and I will probably not stay in the United States. I am afraid of something here, a dark force, a profound emotional sickness that I cannot quite define. Our specific decisions will depend on the public reactions to my book, which cannot be predicted yet. I may find myself confronted with a hostile barrage not only from skeptics, but also from the believers in extraterrestrial beings who expect imminent salvation from the stars. American society scares me because it is capable of producing terrifying movements based on lies, religious fanaticism and other aberrations that nothing can stop. I am thoroughly disgusted with the sensational presentation of the UFO subject in those American tabloids that do not hesitate to make up sightings out of whole cloth. Bill Powers keeps reminding me that what I see as flaws in the American mind are really defects and weaknesses of mankind in general: Americans did not invent religious *fanaticism*, nor do they have a *monopoly* on yellow journalism.

Chicago. 29 November 1964.

The Bouffieux photograph, which was published by Aime Michel and by Jimmy Guieu in their books, has turned out to be a hoax. I have finally decided, out of caution, not to use any such alleged saucer photograph in *Anatomy*. A few pictures do seem trustworthy, but I am not able to document their origin with enough confidence.

While perusing the French files I have come across a letter addressed to Raymond Veillith, one of the most dedicated private researchers in Europe. It was written by Colonel Poncet, chief of the 'saucer' bureau of the French Air Force, in answer to Veillith's offer of information sharing:

The scientific office of the Air Force staff is concerned with much more important subjects. It finds itself forced to leave to amateurs the task of spreading the truth about the interplanetary ships and of greeting our space brothers.

He added: "It would be pointless for you to go on sending us your reports." Thus the door was neatly closed to any disturbing fact.

Chicago. 2 December 1964.

In response to a recent report I sent him on my current work, Aime Michel describes the trials and tribulations of a French physicist, Olivier

Costa de Beauregard. This man is pursuing top-quality work on the theory of gravitation, yet he is unable to get any support in France. Nobody will take him seriously in Paris until his work is duplicated in the United States. Aime Michel adds:

In the mental system of our contemporaries there is no framework for ufology. The compilation of your catalogue, like that of my own files, is already a violation of this mental framework: we are studying a phenomenon without having the definite proof of its existence.

**Chicago. 3 December 1964.**

Today our son is one year old, already!

I had breakfast with Hynek and I told him, point by point, about Costa's work.

**Chicago. 9 December 1964.**

Despair. I feel we are making no progress. A strange disenchantment hangs between Janine and me, or is it my own failure to cope with the stress of our relentless work? It seems that nothing encourages me, nothing smiles at me. Torture: I am exhausted at the memory of the happiness which was here, within reach, just a moment ago.

**Chicago. 19 December 1964.**

*Anatomy of a Phenomenon* will be ready for the printer next month, thanks to the excellent editing work done by Harvey Plotnick. The dust jacket design shows a fiery meteor tearing through a dark blue grid. The next step is to prepare *Challenge to Science* for publication. Harvey has read Gordon Creighton's translation and likes it. I will have to work on both the American and the French version at the same time. But my first priority now is to bring Costa and his assistant to the United States for a series of lectures and high-level meetings.

**Chicago. 20 December 1964.**

Returning from a cosmology conference in Texas, Hynek called me tonight to say he had met in Austin with Wennersten, the top physicist for the Air Force. They had no trouble contacting two leading rel-

ativists, Papapetrou and Lichnerowitz, to get some references about Costa's credibility and scientific status. What they heard satisfied them. So why shouldn't the Air Force pay his way over here? Costa would be able to see my UFO files (Aime Michel writes that he is dying to read them) and he would be able to talk seriously with Hynek and me about physics.

This Journal is a constant lesson for me: regret for the mistakes I made, terror before ignorance and human weakness. But those feelings also turn to tenderness and pity. When I close my eyes I often see a country cemetery in Pontoise. It is raining. When my father's coffin was taken to its resting place I walked behind the car with my brother, as we climbed the street that leads towards Gisors. A *man* in working clothes who was pushing a wheelbarrow put it down as he saw us, removed his beret and respectfully crossed himself.

My mind is full of shadows, my consciousness thrashes a hundred questions I cannot research. I must follow my own way, I must select between bifurcating roads. The choice has always been easy for me. Whenever I found myself facing two paths, one of which was smooth and predictable, the other burning with questions, problems and potential revelations, I have always taken the latter.

### **Chicago. 22 December 1964.**

We had a disappointing meeting at Bryn Mawr yesterday. Bill Powers spent almost an hour on the phone with a witness, telling him about our group and how great we were instead of focusing on his sighting, on what the man had to tell us. I feel we are wasting precious time.

Today Hynek and I had lunch in private. I gave him a complete file of Costa's publications. I told him about a new possibility: should we think of the *phenomenon in terms* of artificial intelligence? I pointed out to him that many observations, including Socorro, Vauriat, Toledo, showed behavior that resembled that of automata rather than that of intelligent beings.

We just had a ~~run~~ Christmas party at the Observatory. Dearborn was ~~full~~ of laughing kids. The ~~staff~~ had assembled odd musical instruments in the library. Bill Powers played the accordion, Hynek came in wearing a red hat and a red vest to direct the orchestra with his baton, and Santa Claus arrived mysteriously through the slit in the telescope dome, carrying a big

bag full of toys. Olivier watched all this and went on to play in Hynek's office, which was littered with bright boxes.

A fruitful year is coming to a close. The publication of *Anatomy* is imminent. I have fulfilled that deep desire I felt to "respond" to the mystery triggered by my own observation of 1955. I have acknowledged that I saw the problem, that I understood its scope. But when it comes to solving it, as the French expression goes, that is quite another pair of sleeves\_\_\_\_

**Chicago. 26 December 1964.**


We spent a warm Christmas around Olivier; a little work, much play. We think about our families left behind in France. Maman will come over this Summer.

Hynek called me this evening, looking for advice. He was thinking of sending a forceful letter to the Air Force, he said. They have asked his opinion of the annual Blue Book report, in which he is cited as scientific consultant. For the first time he is tempted to rebel. He understands that his signature is only a formality. The work we have done together for the past year has made him aware that there are no scientific contents to the Air Force report: "I don't want my name associated with this piece of trash," he said, "especially when *Anatomy of a Phenomenon* is so close to publication." We discussed the way in which the book might make people aware of the depth of the subject, even awaken scientific opinion. He told me he didn't want *to* be tied to the Air Force party line *any* more.

I have received a very interesting letter from an Italian military officer I will call Luciano:

I am a Captain in the Italian Air Force and I am employed at the Ministry of Aeronautics in Rome.... My interest in the UFO problem began many years ago when I had occasion *to* speak for the first time with direct witnesses of whose sincerity I had no doubt. Previous to that I was very skeptical on the matter.

He described his sources of documentation, his research, and his files of over 6,000 index cards containing the details of sightings since 1947, about 800 of them from Italy itself. He went on:



like every-  
body else, and pursue only those researches whose results are already  
known. Do not raise unsettling new questions that could bring Theory to  
its knees. Such is the unfortunate setting for much of modern science.  
Chicago. 10 **January** 1965.

Another letter has come from Luciano in Rome, thanking me for my  
quick answer and expanding on the state of UFO research in his country.  
He deplores the fact that the few public advocates of the phenomenon  
have turned out to be crackpots who brought ridicule to the subject.



Things changed a little last year when, after a flurry of sightings in the Rome area during the summer months, I was asked to provide my opinion on the matter. I have been appointed a UFO consultant to the Italian Air Force. In some cases I participated in the investigations carried out by our authorities. Naturally I have been given full access to the Air Force files.

In return he asks me what I regard as the most interesting U.S. observations of 1964.

**Chicago. 13 January 1965.**

I have been working on the text of *Challenge to Science* at the Regnery offices. The title was suggested by Bill Powers at one of our Bryn Mawr meetings. My editor is an energetic young woman named Betty McCurnin. Formerly at the *Encyclopedia Britannica*, she is obsessed with always finding the right word, the short phrase that completely captures an idea. Gordon Creighton, who is retired from Her Majesty's Foreign Service, has translated my long, beautiful French sentences into long English sentences. Betty tells me:

"Jacques, sit down, I want you to listen to something."

She reads to me one of my long sentences, shakes her head as if to scare off some annoying flies, then:

"What does all that mean?"

I give her a few words of explanation, summarizing the arguments, weighing the evidence. She leans back in her chair, holding her blue pencil lightly between her fingers:

"What you just said makes perfect sense. So why didn't you write it that way?"

We cut, we restructure, we trim. She is patiently teaching me to write with the concise precision afforded by the English language, rather than the flowing, evocative, visionary sense of French.

I am adding a whole section to the book, proposing a reorganization of UFO research. It shouldn't be up to the Air Force to decide if the unsolved "residue" in the data contains new scientific evidence. Blue Book should only be a filter, and the unexplained reports should go to a permanent civilian office staffed by scientists in charge of the analysis.

**Chicago. 15 January 1965.**

Taking me aside in confidence, Hynek has shown me some extraordinary notes he took a few years ago from a classified report written by Colonel Robert Friend, one of the most competent officers ever to be responsible for Blue Book. Friend was still a Major at the time. The report concerns a meeting that took place at a CIA office on Fifth and K street in Washington, on 9 July 1959, under the direction of a man named Arthur Lundahl. Seven CIA officers and one from the Office of Naval Intelligence completed the team.<sup>15</sup>

It appeared from Lundahl's statements that five years earlier, in May 1954, a certain Mrs. Guy Swan who lived in South Berwick (Maine) had contacted the Navy through a retired Admiral. She was able to "channel" outer space entities such as Affa and Crill, she claimed. She would pose a question, relax with a pencil in her hand, and soon an unknown force would take over and provide meaningful answers. A Naval Intelligence officer, Commander Larsen, visited her and tried without success to establish a psychic contact under her guidance. On 6 July 1959 (three days before the meeting related here) he had discussed the case with Lundahl and Neosham at the CIA. They encouraged him to try again and this time he was indeed successful in receiving messages from the Affa entity, who lived on the planet Uranus.

After several exchanges of platitudes typical of psychic communications ("there will not be a third world war, Catholics are not the chosen people," etc.), they requested to see a flying saucer. Affa told them *to look outside*.

All three rushed to the window, the assembled officers were told. *And suddenly there it was!* Lundahl, Larsen and Neosham had seen what they described as a circular object, the edges lighter than the center. Neosham had called the Washington airport radar and had been informed that electromagnetic signals were unaccountably "blocked" in the direction in question.

**Chicago. 18 January 1965.**

Hynek is back from Washington, where he has seen many people. The Air Force chief scientist is getting translations made of all the papers by Costa which I had transmitted. There is some new movement in the

UFO field, and I must recognize that this time the credit goes to good old Major Keyhoe, who is stirring things up with his many articles and television appearances. As for Hynek, he keeps telling the Air Force they should undertake a true scientific study—but nobody listens to him.

"What do you think of the rumors that Blue Book is only a cover, that there is another research project somewhere?" I asked him recently.

"Well, *with the enormous budget this country is spending for classified work, it is almost unthinkable that there wouldn't be a secret project on UFOs.* But I wouldn't jump to quick conclusions," he added with a chuckle, "remember what they said about military intelligence being a contradiction in terms!"

"Still, we may be wasting our time, while some group of scientists working behind the fence has the real data\_\_\_\_"

Hynek thought about it for a while, and shook his head.

"No, I have a hard time believing this. I would have picked up something along the way somewhere," he said. "If there is a secret project, Ruppelt certainly didn't know about it, and Friend didn't know about it.... Yet it would have to get its data from the same sources we have, wouldn't it?"

"It could have its own detection systems."

He shrugged impatiently.

"Look, I've had a secret clearance since the war. And it is true that what I was working on, the proximity fuse, was kept strictly secret. I didn't even know what the next guy was working on. Sure, the same thing could be going on here. There may be secret projects looking at esoteric propulsion schemes, I'll grant you that. They could be hidden away in the Four Corners, or in the middle of Australia. We would never hear about it. But it's utterly unlikely."

### **Chicago. 21 January 1965.**

Edward Teller gave a lecture at the Technological Institute today. He is a dry, humorless man. His goal is not the betterment of mankind but only science for pure science's sake. He wants to blow up "clean" nuclear bombs. He doesn't speak of understanding nature, only of taming her, of dominating her, of conquering her. His science is a big, precise machine from which man can learn nothing that will enrich his value system, only more facts and more numbers.

**Chicago. 27 January 1965.**

We saw Major Keyhoe on television last evening. His performance was rather pitiful. A pretentious reporter cracked a few easy jokes before introducing the panel. A skeptic had been recruited among local astronomers. He kept using irrelevant, insulting arguments like "you cannot speak objectively of the reality of the saucers because you make a lot of money with your articles."

The Major gave me the feeling he was a nice, nervous man, way over his head, ill-prepared for this kind of debate. He allowed himself to be easily maneuvered.

**Chicago. 28 January 1965.**

Serge Hutin has sent me his book *Mysterious Civilizations*. He writes:

It is said that some spiritual centers exist . . . hidden to profane eyes, protecting the world with their invisible influence. Some locations (like certain parts of California and the city of Lyon) have been magically prepared by great initiates in ancient times to serve as gathering points for qualified magical researchers in centuries and millenaries to come.

I muse that this passage could well apply to Evanston, where a strange convergence of paranormal research is taking place, even though the University shows no interest in supporting it. Not only is the observatory directed by Hynek, a man with mystical insights, but Evanston is home to *Fate Magazine*, that popular standard of occult lore, and the campus seems to attract people with a private passion for higher truths.

**Chicago. 4 February 1965.**

Major Quintanilla, Sergeant Moody, Hynek and Carl de Vito have just left our Bryn Mawr apartment after three hours of intense discussion. I feel tired but pleased with our progress. Quintanilla now understands how we work. He saw our card file, the original documents, and evidence of Aimé Michel's sources, dispelling his last lingering suspicions that the French reports were vague or had been invented out of thin air. We discussed the goals and the methods of the Air Force in detail. I gave the Major several examples of Blue Book sightings that should have been

placed in the *unidentified* category, and for which he had no answer. I have no quarrel with the Air Force and no axe to grind, but I believe I demonstrated to Quintanilla that we should not think in terms of isolated cases any more: instead we ought to think of UFOs as a global phenomenon. This would lead to entirely different methods, new classes of evidence.

Janine created a congenial atmosphere for these talks, a sense of working together even if we disagreed on some points. She put everyone at ease with her charm and her practical hospitality. Thanks to her, Bryn Mawr is warm, comfortable, a pleasant retreat from the world.

### **Chicago. 6 February 1965.**

As Hynek drove our guests back to their hotel, Moody made some dumb remark and Quintanilla said with a sigh: "Sergeant, sometimes I wonder if you understand the problem!" Everybody burst out laughing.

Moody deserves a Nobel Prize in fudging for his bold UFO "explanations." Thus he is the discoverer of a new species of *birds with four blinking lights*. It was also Moody who once decided that a certain observation was without merit because "the reported object did not match any known aerial maneuvering pattern"!

Quintanilla has no scientific curiosity but he shows conscience and integrity. I hope that I have convinced him that he stands before a serious problem, *not just some routine* military job, and that the reports from the public he handles every day deserve to be treated with some care.

At noon, as Janine and I were about to have lunch, the doorbell rang and a telegram was delivered to us. It came from my French agent, who has been whipped into action by my recent threats of going around him: Table Ronde, a Parisian publisher, is sending us a contract for *Phenomenes Insolites de l'Espace*. If all goes well, both of my books will thus be published within a few months of each other. Many important facts about the phenomenon will be on record at last.

### **Chicago. 8 February 1965.**

This evening we resumed the regular meetings of our research group, which Hynek jokingly calls "The Little Society," by contrast with President Lyndon Johnsons "Great Society." At today's meeting I described our classification system in detail.

**Chicago. 10 February 1965.**

In preparation for a new biomedical study I recorded my first electroencephalogram today. I converted all the data to digital form in order to run a computer analysis. In spite of my interest for this work I would be thinking of leaving the University and of returning to industry if it weren't for Hynek, and for the expectation of working with him once the new observatory is built. The positive side is that my department *leaves me free to do as much* personal research *as* I want, as long as *my* normal work gets done.

The Vietnam war is getting worse. The U.S. Army communiques about recent victories sound eerily identical to the French news I was hearing as a child. They speak of "police operations," of routine rounding up of suspects, of mounting casualties among the "rebels," of their imminent defeat. Speaking of the Geneva Convention about Vietnam, the U.S. Government says there would be no war if the enemy did not violate this agreement. This is like saying that houses could be built without any roof at great savings to everybody if only rain did not fall from the sky. Once again, with Vietnam as with UFOs, American decision-makers seem incapable of thinking in terms of historical and social phenomena, of global strategy. They keep dealing with superficial symptoms and parochial interests. Even Hynek is a conformist at heart. He looks at me with embarrassment and suspicion, and he soon changes the subject if I dare voice my opinion that Vietnam is a stupid war. Other staff members shrug when they hear my arguments: "What happened to France cannot possibly happen *to* America, we are *so much more* powerful! There will never be an American Dien-Bien-Phu. The war is only an economic issue, we will drive the cost so high that little North Vietnam will never be able to afford it. We will never fall into the same trap as France did."

**Chicago. 11 February 1965.**

There must be other levels of consciousness, and other lives than ours. I dream of the transfiguration of man into a conscious being with full access to his world: beyond borders, beyond everything that is absurd and arbitrary in our narrow social, moral, sexual rules.

Some evenings we still manage to break away from the tensions of Work. We go out with friends in the bitter cold of the Midwestern win-

ter, for a hot cup of coffee, or a bowl of soup and a movie. The icy weather brings glorious purity to the tall buildings of the Loop, the streetlights reflecting off sheets of white everywhere. Petula Clark seems to explode with all the power of Chicago as she sings "Downtown," lustily expressing what we feel inside: "Forget all your troubles, forget all your cares—Downtown!"

**Chicago. 16 February 1965.**

Hynek wants to learn about computers: every morning I give him a one-hour programming class. From the conference room in the biomedical department we can see the landfill area where barges are bringing sand from Indiana. A few trees have already been planted there, and trucks have cut a road towards the site of the future Lindheimer observatory that will house a 40-inch telescope.

Every Monday Hynek and I have our regular lunch to compare notes on the status of UFO matters. I have two hours of classes in the afternoon as I build up credits towards the Ph.D. qualification. And in the evening about 6 p.m. the Little Society convenes for dinner at the cafeteria.

When he returned to France after a trip to the United States in 1961 my French astrophysics teacher Vladimir Kourganoff lamented the lack of cultural density from which, he said, he had greatly suffered here. The contrast was even greater when he later travelled to Holland: on one side he only saw American mediocrity on a grand scale, while on the other side was evidence of what he called the warm European soul. Such judgments are biased and superficial. It is true that in Europe every stone has been painted twenty times by classical masters, it has served as a seat for a hundred disconsolate *poets*, and dozens of philosophers have rested their foot on it to tie their shoelaces. But Kourganoff, like most European tourists, saw nothing of the new culture which is bursting out of the ground everywhere in North America, because it is not made obvious by museums and plaques on prominent buildings. The media only reflect the most vulgar level of life. The casual observer cannot see what I am slowly learning to see: America's deep and secret beauty, its buried emotions.

**Chicago. 23 February 1965.**

It is becoming very hard for the Air Force to ridicule the UFO subject now that *Anatomy* is circulating in manuscript form and that Hynek has

begun to change his tune in public. In fact Moody is in danger of losing his job in the process. His universe is a world where neatly organized catalogues of Rational Events furnish well-behaved *Models of Authorized Phenomena*. Hynek jokes that the Air Force must have sent him to an elementary psychology class, from which he seems to remember only that the world is made for "normal" people like him and that anyone who reports an unusual phenomenon is simply "nuts."

In December 1964 a huge craft is said to have landed in a field near Harrisonburg, Virginia, after flying over a car whose engine stalled. Some local college teachers measured strong radioactivity at the site. A few days passed, and a report was duly *sent to* the Air Force. More *days* went by. Quintanilla eventually sent two sergeants to investigate the case. They came back to Dayton and stuffed the report in the "psychological" category ("the witnesses were nuts").

This neatly explained everything except for the radioactivity readings. One of the college professors, one Dr. Gehman, got angry and mailed his own observations to NICAP. He also reported on the peculiar investigative techniques used by the two sergeants, who had arrived on the scene no less than three weeks after the events. They carried a Geiger counter, but all they did with it was to sweep it over the field, which was now covered with a thick blanket of snow. Yet they seemed to be detecting something. Every time the needle of the counter hit the top, they would reset the calibration, saying reassuringly: "That's all right, it does that all the time! There's no radiation here!"

In his most recent letter to me Aime Michel is reporting on a curious change he believes he is detecting among some secret services in Europe which seem to have taken an open interest in the subject. Thus a former agent of the British Intelligence Service has leaked the news that Great Britain was now swapping information on UFOs with the Soviets, both having reached the conclusion that the objects were real. Another agent, an American, has assured Michel that the FBI took the whole issue very seriously. Finally, Colonel Clerouin, whom he had not seen for years, suddenly invited him for lunch and told him there was a lot of interest in the subject among the French military staff.

Aime does not trust any of these shadowy people, whose very business is lying and cheating in the first place. He thinks that all these rumors are manipulated, but by whom? And why? Someone is using us to "snow"



somebody else, he thinks. But the sudden renewal of interest in the topic, the fact that they even talk about it, is very curious indeed.

**Chicago. 25 February 1965.**

Yesterday Hynek and I called Wennersten to discuss Costa's upcoming visit. Today I spent the morning correcting the galleys of *Anatomy* with Harvey Plotnick and Betty. I had lunch with Hynek and Richard Lewis of the *Sun-Times*, whom I have known since the Ranger affair. He is thinking of writing an article based on my book.

Among Aime Michel's papers I find a remarkable letter from an Englishwoman living in South Africa. Her house sits high on a hill above a wide river valley. One night in February 1957 she saw a "moon" that moved and emitted multiple flashes of light. She got up from her bed, went to a room which looked over the valley and she opened the window. The "moon" came towards the house with a fast zig-zag motion, changed to the shape of a golden football and flew over the roof. As it did so she smelled an odor like that of an overheated radio. At one point the light went behind a tree and part of the object was then visible on either side of the trunk. It disappeared at high speed.

If it was a saucer, if it had a crew and if they saw me at the window I must say they probably went away with a strange impression of an earth creature: I am blonde, almost six feet tall; on the night in question I was naked because of the heat and the humidity, and my hair was in metal curlers! It is not surprising that they left and did not return\_\_\_\_

**Chicago. 28 February 1965.**

Running through the library of the Technological Institute in Evanston and at the Midwest Exchange Center in the Loop I have found a wealth of old sources mentioning either "intra-mercurial" planets or dark bodies crossing the sun. Such objects were seen repeatedly during the nineteenth century.

I relax from computer work and library investigation by painting a series of fun and wild scenes on the walls of Olivier's bedroom: butterflies and monkeys and animals of every description. We play together every night. He babbles happily. Whenever music is heard in the living room he

insists on pushing his playpen into the door frame to sing along with the melody in his own fashion.

**Chicago. 4 March 1965.**

It is snowing over Chicago today. In the light of State Street or the canyons of Lake Shore Drive my windshield wipers cut through a marvellous whiteness where the world seems delicately suspended. In the glowing night warm happiness passes, so close, so strong! Pools of eternity glitter in the darkness, reflecting other galaxies. Pools, I want to kneel next to you and drink you.

Dream, lead my furious life to the great blue roses of stained glass, to the kingdom near the sea where died the Fair Annabelli.

**Chicago. 15 March 1965.**

A strange movement is sweeping me along now, like the eruption taking Jules Verne's visitors away from the center of the earth, propelling them towards an unknown surface. I feel new forces awakening; they clearly show me who I must become: a man who is free, without fear, dedicated to the old-fashioned search for naked truth. The planet should not belong to boredom.

Another morning spent revising the galleys as *Anatomy* nears publication. I would have to be stupid not to feel terrified, but this very terror forces me to look for new sources of energy within myself. I have conquered these fears, except for one: I am afraid of being wrong, not so much because it would reflect badly on me, but because I might draw others into my mistakes.

The last line in the book reads: "The sky will never be the same again." But I will never be the same either. I feel like the little boy who had been told that he would be hit by lightning if he ever said ~~naughty~~ words, when one day he happens to get uncontrollably mad, yells "Shit!" at the heavens and discovers that he has miraculously survived after all.

**Chicago. 22 March 1965.**

We have been leading a very cloistered life that was becoming heavy and drab. At last we left all our various projects behind last week, and we roamed through Chicago with new-found friends who do not belong either to the academic community or to the UFO circles. It was refresh-

ing to be able to talk about ordinary subjects, to discuss movies and life in general, to forget science and its narrow rituals. And it is fun to be guided through this formidable city by people who know it well.

**Chicago. 30 March 1965.**

Hynek has come back from a trip to New Mexico. In the plane he wrote a statement praising *Anatomy*, and he has authorized me to publish it. At the same time Carl Sagan denies me permission to quote him. As for Quintanilla, he demands that I refrain from mentioning that Hynek is consultant to Blue Book if I publish his statement of support: those are reminders that the fight is far from being won. My book is simply changing the rules of the game towards a more scientific debate where the Air Force now must alter its tactics and its language. Eventually it may even be forced to open the door to new ideas.

**Chicago. 9 April 1965.**

*Anatomy of a Phenomenon* will be out in the bookstores within a month. An article by Dick Lewis based on the book has appeared in the *Sun-Times*. It was picked up by other papers from Florida to Maine. Now I am beginning to receive some friendly letters and phone calls, principally from members of NICAP who are open-minded enough to challenge Keyhoe's party line.

Yesterday the contract for *Phénomènes Insolites* arrived from Paris, with an advance on royalties that will barely enable me to pay Gordon Creighton for his translation work.

I had a confidential talk with Hynek today. I briefed him on my correspondence with Luciano, the Italian engineer who analyzes UFO cases behind the scenes for the Italian military, and who has given me permission to disclose our correspondence to him.

**Chicago. 13 April 1965.**

Life has become too heady, like a strong wine. Janine and I have left our research aside for a while. We are both changing jobs. She could no longer stand the pettiness and the greed of the contracting company where she worked until now. And I have entered the engineering graduate school full-time, planning to take the qualifiers in June.

A funny scene took place today when Regnery sent two men to pick

up my model of the Hopkinsville humanoid: they want to put it in the window of Chicago's largest bookstore *to* promote my book. This model is an exact replica of the goblin seen at the farmhouse in the famous 1955 incident when an entire Kentucky family shot at the intruders until they ran out of ammunition. The creature was four and a half feet tall, dressed in silver, with a large chest, a big head, huge ears, long arms. My model of it is so life-like, with its two red eyes on the sides of its head, that the burly truckdrivers backed off in some apprehension when they first saw it. Realizing that it couldn't bite, they eventually picked it up, folding its ears carefully for the trip down the stairs.

Luciano has sent me information about a near-landing that took place on 20 August 1963 at 9:32 p.m., and which he investigated with a secret service team under a special clearance from the Italian government. The witness was the trusted chauffeur of the Italian President, driving his official car. The site was the hunting reserve of the President, not far from Rome. A disk-shaped object resembling an upside-down saucer with a turret on top hovered at low altitude above the car. The case created quite a stir among the Intelligence services, understandably. The report was communicated to the U.S. authorities in Washington, who never followed up with the Italians but gave assurances they had passed it on *to* Hynek for evaluation. Yet Hynek has never *seen the* report, never heard of it! *I have used this case to point out to him again that he didn't see all the reports, that there must be another study somewhere, using Blue Book as a mere front.*

### **Chicago. 20 April 1965.**

I am finishing several tasks related to Hynek's project on stellar evolution. Since the rule at Northwestern demands that I be a full-time student for three consecutive quarters I *will* receive no pay for my work on the second edition of his Bright Star Catalogue, but I will complete the job anyway.

Hynek has now read my whole correspondence with Luciano, which goes back to December 1964. He emerged from this reading visibly shaken. He had never suspected the existence of a serious UFO project behind the scenes in Italy, a national project whose files were not shared with Blue Book, and now he wonders what else he doesn't know! He has requested information about Luciano from his Intelligence contacts in

Washington. He is also asking the Air Force to find out all available information about his European counterparts: He wants to find all the scientists who might be consulting, as Luciano does, for the various military forces.<sup>16</sup>

Surprisingly, Dick Lewis' article about my book has earned me congratulations from the engineering faculty at Northwestern. This open-minded reaction gives me new hope.

Spring is coming back. All over this little planet men talk and fight, fear and fight, hope and fight and die. The Vietnam war lingers on. Poor folks on both sides are crushed under the crumbling ambitions of stupid leaders. Will the world always stagger blindly from war to war? Nobody seems to care. The level of international news we get from radio and television media in Chicago is pathetic. What little data we glean is ridiculously short and biased, smothered in local politics and "local interest" anecdotes that are meaningless.

Chicago. 4 May 1965.

Hynek has just told me on the phone that Menzel has suffered a heart attack, but survived.

I think I have found the right topic for my doctoral dissertation: how to build an artificial intelligence system capable of answering English questions about a database of bright stars. Such a program would be driven by a linguistic analyzer. My adviser, Professor Krulee, doesn't think the system can be built successfully, but he believes it would be interesting to try anyway. The only question-answering systems in existence are little more than toys, like MIT's *Baseball* program designed for simple-minded inquiries like: "Did the Red Sox beat the Dodgers?" The technique has never been applied to a full-blown scientific problem.

Chicago. 6 May 1965.

Michel Gauquelin and his wife are continuing their pioneering work on their revisionist approach to astrology, with great statistical success. I brought their results to the attention of Regnery, with my recommendation to have their book, *Cosmic Clocks*, translated and published in the United States. Regnery's daughter Susanne is reading it in French.

I spent the morning with Harvey Plotnick reading the early reviews of *Anatomy*. This evening the first box of books arrived from the printer.

Janine picked them up *on* her way home. The *very first copy, of course*, is for her. Two others for Hynek (who will forward one to the Air Force) and the fourth for Aime Michel. Now the rocket is launched.

# 10

## **Chicago. 8 May 1965.**

A good old Chicago-style heat wave has struck. The muggy weather is draining. I feel exhausted in spite of the soft tepid wind and the blue sky above. Janine has gone shopping with Helene. My son is supposed to be sleeping, but he *is* just as uncomfortable as I am and cries in his room. I feel helpless, dispirited, unable to work as hard as I should on the art-work for *Challenge to Science*. Part of my discouragement comes from the irritation of having still more examinations ahead *of me*. I thought I had left all that behind a long time ago.

## **Chicago. 17 May 1965.**

A curious incident recently took place during a conversation with a Martin Marietta engineer who says he is compiling a reference book on UFOs. As he was spending the evening at Bryn Mawr with our Little Society the conversation came to sightings in the Soviet Union. He told us he had written (in Russian) to their Academy of Sciences, and had received the reply that no study was being made of the subject. He showed us the Russian reply, held in a thick black binder. As the conversation continued the binder was passed around and it came to Sam, who read the letter and innocently turned the page. The engineer leapt out of his chair like a tiger and tore the binder away, tersely spitting out, 'the other papers have nothing to do with that!' We were left fairly shocked at the violence of his reaction. Of course we began to wonder what else might be in that binder. There are rumors that major aerospace companies are conducting their own secret studies of UFOs.

Today Hynek invited me to have lunch with a fellow from the Illinois Institute of Technology, an engineer who thinks he knows how to build a flying saucer. It was a waste of time.

**Chicago. 21 May 1965.**

Last evening I met with Hynek and an NBC television reporter. The network has decided to shoot a documentary on UFOs. I see this as a further sign that my book comes at the right time. However I have not gotten over my discouragement of the last few days, which may correspond to the "decompression" that follows my intense work on the two books. This realization does not make it any less painful: I feel fragile and vulnerable.

**Chicago. 26 May 1965.**

We had another meeting with the NBC man last night at Hynek's place. Allen greeted us informally, dressed in a sweater and slippers, his face drawn and wrinkled, with signs of age and a great tiredness in his eyes, reminiscent of some pictures of Einstein. Underneath his exhaustion, what we saw was a genuine humanist, caring and vulnerable.

A strong wind was beating against the screen-enclosed porch. It made the lampshade swing over the crude wooden table. Our maps were always threatening to fly away.

When I look at Hynek, at his gentle and ironic way of contemplating the world, at his conscience and his simplicity, I see a rare example of a scholar who is also capable of inspiring great projects, visions of new values, of freedom beyond the ivory towers. Others often consider him a second-rate scientist, because he never tries to impress, he never pretends to know more than he actually does. He loves to share his sense of mystery, of puzzlement.

**Chicago. 29 May 1965. Noon**

Janine and Helene have gone out with Olivier. I am working at my desk and *my* thoughts drift again *to* France, to the sweet France *of the* meandering rivers, of the fiery sunsets behind the tall cathedrals, the gentle prairies, where cows walk slowly in the morning dew while the angelus rings from the tower of the country church, and baby-face clouds waste their time in the sky above.

But what about the French people themselves, their vision and their appetites? I see them clearly enough through newspapers and books, through biased news published in perfect good faith and impeccable

style. What comes through all that is technical incompetence, an unfocused view of international events, a vaguely liberal self-image that is no longer connected with any genuine love of humanity at large, a pretense of caring for others that is denied at every page by the glossy pictures of luxury cars, extravagant perfume and expensive toys.

It is the ads that count. They show men in their thirties, supremely elegant, handsome as Alain Delon, coming back from the tennis court with some vapid debutante at their side. This generation represents a wave of odious upstarts with empty dreams and vacuous conversation. They were born around the time of a World War that was fought and won by others, in a world invaded by *forces* their own parents could *not* fathom, broken and decimated as they had been by the previous war, the "Great War."

When I was seventeen I had many angry arguments with my father and with my uncle around the lunch table every Sunday in Pontoise.

"It's your generation that made all these mistakes," I would tell them. "You went through the First World War and you failed to understand what was happening. You went ahead with your colonies, your so-called French Empire, your old bourgeois values. You created this *mess*, all this injustice we see in the world."

Perhaps I was right. But what I failed to see was the imminent replacement of what I called the "old bourgeois values" by the young bourgeois values of my own generation. How could I imagine that they would be even worse? More unjust, more ugly, because they would not even rest on any tradition of cultural continuity? In the meantime the most solitary, pitiful generation of men and women is now dying in France, forgotten by all the magazines, ignored by the publicity men and the chroniclers with the elegant pen and incisive style. It is a decimated generation that tried to preserve its obsolete culture amidst the most sordid display of cruelty and terror ever seen by man. The promising young poets, managers, inventors, doctors and statesmen who could have led Europe to greatness were turned into bloody pulp by the cannons and the machine guns and the poison gas. One only needs to take a look at the age pyramid of France and Germany to see where the genius of Europe has gone, why it can't solve its modern problems, why the great leaders and the great thinkers are missing.

When my father was at Verdun as a liaison officer, he was often the



only thing that moved in the whole landscape, and the physical and psychological skills he developed to survive in that terrible environment were extraordinary. One day he came back from a mission under the fire of German batteries to find that his whole company—the two hundred men with whom he had lived and fought and faced death in the mud of the trenches—had all been killed or maimed by a direct hit from the enemy guns.

That generation, or rather what little was left of it after the carnage, is now dying. It is being replaced by a wave of arrogant fools who are only concerned with the size of their apartment, their little nest egg in the anonymous coffers of Switzerland, their shiny new Citroen.

### **Chicago. 8 June 1965.**

For five hours I have worked with the NBC television team filming a new documentary. As a result the Toulouse multiple-witness sighting and the case of Vins-sur-Caramy, with its remarkable observation of the vibrating road signs, have been duly put on record for future audiences.<sup>17</sup>

### **Chicago. 13 June 1965.**

Helene flew back to France yesterday, taking with her the records she is instructed to return to Aime Michel in person. My mother arrives tomorrow: we have spent two days preparing her room and generally putting the house in order. There is a strange atmosphere here because of the uncertainty in my life. Fortunately Olivier is an angel and lets us work in peace. I am right in the middle of my written examination. Letters from readers of *Anatomy* are flowing in. They express the happy feeling that someone is now working seriously on the UFO problem at last. I wish I could be as enthusiastic \_\_\_\_

My bitterness about these silly examinations leads to discouragement. The University is not measuring our scientific merit, only our dogged obstination. They are not selecting the best researchers, but the ones who are the most pig-headed.

### **Chicago. 14 June 1965.**

Maman arrived safely this evening, bringing me another batch of original documents from Aime Michel. A nice letter came from Guerin, who rightly pointed out one of the flaws in my book: I don't spend

enough time on the physical problem itself. After all, relativity does not admit of any speed faster than light, he says, and dematerialization belongs in science-fiction novels. *There is no adequate physical framework for flying saucers.* I am hoping that Costa de Beauregard, the French physicist who is a friend of Aime Michel and plans to come here next January, will provide us with some new insights.

Today McDivitt and White, the Gemini astronauts, are in Chicago for a big parade. So is Hector Quintanilla, more discreditable, being interviewed for the NBC documentary.

### **Chicago. 3 July 1965.**

Lecky's book, *Rationalism in Europe*, which I am currently reading, is a fascinating study of the changes in public opinion that led to the discarding of medieval ideas about sorcery. I find in it an obvious parallel with our current quarrels. Aime Michel and Guerin have trouble understanding why scientific minds do not embrace the concept of extraterrestrial life with enthusiasm. But the problem is not one of logical reasoning. Public opinion is evolving in spite of anything we or the skeptics can say and print. That is why the heavy debunking efforts of Menzel have so little effect, and why the impressive "evidence" compiled by NICAP has no impact either.

Regnery has decided to take my advice and to publish the Gauquelins' book on astrology. I called Michel and Françoise to give them the good news, and I told them they should come over to America to continue their research. I had planned to talk to Hynek about their work, but I wanted to wait until there was something definite about their book, since he does not read French. I did tell him about it last Wednesday on the way back from lunch. This devil of a man surprised me again, revealing that he had long been interested in astrology since his early student days, although he didn't buy all the false science that came with it. He had even done some statistical calculations along the lines systematically explored by the Gauquelins. Naturally that is the sort of thing he would never admit in front of his colleagues.

A remarkable sighting has just taken place in the Alps, in a field near the small town of Valensole. A farmer named Maurice Masse saw an oval object land, with two small occupants that stepped outside.

**Chicago. 6 July 1965.**

Janine's brother Alain has joined us from France. He is a pastry chef who was wasting his time *and* his health working under abominable conditions in a bakery shop in a Paris suburb. We helped him get a job with a Chicago restaurant, and now he is adapting rapidly, although he speaks almost no English.

I had a long phone conversation today with Gerard de Vaucouleurs, who congratulated me for my book and told me I must not give up this work. These encouragements are important to me. They were quite unexpected, even though I have long understood that de Vaucouleurs, like Hynek, had reached personal conclusions about the subject.

**Chicago. 9 July 1965.**

Last night Carl de Vito and I discussed mysticism. Lecky does a magnificent job of describing the disappearance of the belief in witchcraft and the obsolescence of the miraculous. He demonstrates the following principle: it is not through a **campaign** of logical arguments that these ideas evolved in the minds of people. Instead, public opinion changed by a barely detectable "shift" or "drift" which was not reflected in the writings of any scholar of the time. The books about sorcery written by the best thinkers of the seventeenth century deplored the increasingly skeptical attitudes of the masses on the subject. The Bible, after all, gives clear and precise instructions to exterminate sorcerers: "Thou **shalt** not suffer the witch to live," in obvious contradiction with "thou shalt not kill." Practically no authoritative book dared to argue in the other direction. Yet, according to Lecky:

In 1660 the majority of educated men still believed in witchcraft, and in 1688, the majority disbelieved it.

Lecky adds that by 1718 those Englishmen who still believed in sorcery had become an insignificant minority. We are probably in the same position today with the belief in alien visitors. The new concept of extraterrestrial intelligence could be studied like a spreading epidemic, using a stochastic model of growth. Citing these facts, I pointed out to Carl that the Church hierarchy had always been forced to follow the movements of society: once public opinion shifted, the fewer witches

they burned and the less witchcraft was actually practiced in the countryside.

Carl said he noticed in me a strong desire to go beyond mere scientific knowledge, but he *did* not think I had a true “mystical” mind. That is a matter of definitions. To me, religion has nothing to do with mysticism. Therefore he is absolutely right if he measures the weakness of my mysticism according to my lack of faith in the common images of God. In any given era faith is determined by opportunistic social factors that have no relationship with reason and intelligence, or with profound mysticism.

***If there is a God, then the most important thing he has given us is our brain. I believe he would want us to use it to question him, rather than throwing away our wonderful inquiring abilities to wallow in blind faith in front of him. To question the divine plan would be the greatest compliment one could pay a Creator.***

As Lecky demonstrates in his discussion about rationalism, the evolution of ideas seldom takes the form of a visible debate leading to reasonable change. Instead mass thinking slides unnoticed underneath the professional thinkers who firmly believe they are planted on unshakable axioms and dogmas. To a mystic, space and time are only appearances secreted within the *neighborhood* (in the topological sense) of a given individual and a given epoch. Therefore mysticism is not a doctrine or a belief but only an orientation of consciousness, a direction of thought away from ordinary space-time reality. Monge used to say that in mathematics the shortest path between two propositions in real space often went through the imaginary domain. I think the same is true here.

If an Adept thinks he perceives the shadow of God and his prodigies beyond the plains and the mountains of the fantastic country of consciousness, that is his business. Since the mystical attitude transcends the everyday world it is natural for it to become associated and confused with the idea of a superior being responsible for all creation. But in my view this identification is not necessary.

**Chicago. 13 July 1965.**

Yesterday I had the bad surprise of receiving a very nasty, threatening letter from a New York attorney. A woman named Isabel Davis who did a study of the Hopkinsville landing a few years ago wanted to sue both me

and Regnery because I had quoted two short passages of her report without specific permission. She didn't have a very strong case: short quotations with full credit customarily can be used without written authorization. My good faith is obvious since I printed a full reference to her work in my book, including her name.

Things improved a little today. Hynek, who is spending the summer at Harvard, knows this lady, so he phoned her to find out the reason for her lawsuit. One of her close friends, it turns out, is a lawyer who pushed her to start litigation. Perhaps he thought that my youth and inexperience would bring them both a lot of easy money.

Not only had she not realized that she had everything to gain by being quoted in *Anatomy*, but it has now come to light that her report itself was based entirely on somebody else's work, an investigation conducted by one of Hynek's former assistants, who has now granted me permission to mention his own extensive findings in the case. In fact she had even signed an agreement at the time not to publish anything! Not only did she not suffer any injury or loss, but she is the one who could be sued. Now that this has come out she is anxious to make peace with me. But she has also angered my publisher, so any mention of her is being removed from future printings.

This little affair does illuminate an aspect of American ufology to which I had not yet been exposed. These people may claim they are acting for the greater good of mankind in disinterested fashion, but their private behavior belies that: their bickerings and back-stabbings are unworthy of true researchers.

This little unnecessary crisis has made me lose two days. I have also wasted time in Evanston convincing the University that they owed me \$200 in back pay. Eventually I won my case, so that our bank account has gone back up to \$249, a veritable fortune.

While we are wasting our time in such fashion, the phenomenon seems increasingly active: the level of sightings which had remained high since Socorro (April 1964) is climbing to a peak. New cases are reported in Chile and in Portugal. Public opinion has become keenly aware of the problem again, and the media are reacting accordingly: *True* magazine has decided to reprint a condensed version of my book in October.

**Chicago. 23 July 1965.**

For the last two days Chicago has been hot, ugly and muggy. There is news of intensifying war in Vietnam, even rumors about possible mobilization in the U.S. Something is very wrong in Washington. And I have failed the oral qualifying examination. I will have to try again. Janine brings me a cup of coffee and comforts me:

"A life without problems, that would be really boring!" she says with a wonderful smile.

Yet this rotten summer gets to me, this crisis summer. I have always liked Fall and Winter best.

Man is pathetic when he fails to perceive his own nature, when he passes his own true self in the dark on his way to oblivion.

**Chicago. 25 July 1965.**

I don't find myself here. I long for a smaller world, for a retreat. I want to get down from the merry-go-round for a while. I have had my fun, I have screamed and laughed with all the others. It may soon be time to leave the fireworks of America behind.

I long for prairies that smell of rain. I would like to be among poor simple people again, laborers, the folks of the old country, arguing about the soccer game at the corner bistro. I have had my fill of watching the big American circus, I am fed up with the tinsel on clown costumes, the strong men breaking fake chains with gusto, the trembling feathers stuck in the asshole of the showgirls. Perhaps I will buy another ticket and come back to be amazed by all that again. But for now, give back to me the deep silence of our walks through the woods; the quiet pursuit of science; and the little kisses Janine used to give me when I came home to her.

**Chicago. 28 July 1965.**

Tonight Janine's brother Alain called me from a Rush Street coffee-shop at 9:45 p.m. He was with five sailors from the French destroyer *Bouvet* which is docked at the entrance of the Chicago River. They had just seen a yellow luminous disk over Lake Michigan, with bright lights around the periphery. It flew South and disappeared in the distance. It had the apparent diameter of the Full Moon.

I called the tower at O'Hare right away. I was told categorically that no advertising plane was flying over the lake. So here is one more case for the unidentified file.

**Chicago. 30 July 1965.**

We are having difficulty finding a trustworthy person to take care of Olivier. To make matters worse our neighbors are not helping us in this predicament. They resent the fact that Janine works downtown, wearing smart outfits, operating expensive computers. They clearly think her place is at home taking care of the cooking and having more babies instead of using her brain. It is curious how women blame so many of their problems on men, while at the same time they display so much envy and jealousy among themselves.

A friend of my publisher has come forth with a private revelation. He was one of the people in the radar room when the famous 1952 UFO flyover took place above Washington, violating restricted airspace—the same case Donald Menzel explained as "temperature inversions." When the abnormal returns showed up on the screens an officer ordered two men to go out with a camera to take pictures of the source. They soon came back, the photos were developed on the spot and they were immediately confiscated. All men in the room were told to remain silent and never to mention the photographs, which showed perfectly clear luminous objects.

But where did those photographs go? And why doesn't Hynek know anything about this?

**Chicago. 5 August 1965.**

We took my mother to Evanston today: lunch at the Orrington hotel, then a walk through Shakespeare Gardens and the campus. She was impressed by the site, amused by the squirrels that run everywhere and even cross the busy streets by performing acrobatics on the telephone wires.

Aime Michel writes that a deluge of sightings is taking place all over Europe. "How come nothing is happening in the United States?" In fact we are witnessing a similar explosion here. There are so many new observations that the Air Force has pulled Hynek away from Harvard to send him to the Southwest, where a wave is in progress.

## BLUE BOOK

The *Medical Tribune*, one of the most exclusive papers in the U.S., has published an excellent review of *Anatomy*. This triggered interest by a reporter from the *Sun-Times*, who will interview me tomorrow. I am pleased and surprised at how objective and serious most of the reviews are. The book was initially ignored by the major papers like *The New York Times* which pride themselves on assuming that any phenomena unexplainable to Midwestern peasants would be easily within the rational understanding of the superior minds in Manhattan. . . . But there is now an avalanche of local reviews, from California to Maine. They show a genuine groundswell of interest which is forcing the big city papers to consider the book seriously. Clifford Simak, writing in the Minneapolis *Star*, called *Anatomy* "fascinating in its detached approach" and the New York *Daily News* mentioned it as "the best book we've seen yet on this intriguing subject, a brilliant effort."

Maman leaves for New York and France on Sunday. The time has come for Janine and me to consider our future. Aime Michel tells us we would be crazy to go back to Paris.

### **Chicago. 6 August 1965.**

Gerard de Vaucouleurs has sent me a letter of congratulations, together with a curious newspaper clipping indicating that Leonard Marks, director of the U.S. Information Agency, was fascinated by the saucer question and wished to see the government organize a program to study "the probability of intelligent life on other planets."

### **Chicago. 8 August 1965.**

Crazy schedule: We accompanied Maman to the plane bound for New York. The day was rainy, stormy, gray, rumbling, sad as a Midwestern summer. Afterwards I drove Janine and Olivier home. They were both exhausted. I went to Evanston to catch Hynek, who was just coming back from Texas and was leaving again, so that I ended up driving him back to O'Hare airport just to spend more time with him. I am glad I did, because I learned a great many things during our discussion.

To begin with, it is not the Air Force which sent Hynek to Texas. He insisted on going there himself. The Blue Book people were concerned that his trip would give even more unwanted publicity to the numerous local sightings. Generally speaking, the Air Force behaved



## FORBIDDEN SCIENCE

very shabbily. They issued silly public statements such as "The witnesses must have seen Jupiter *or other such stars*" ! One would expect Air Force officers to know that Jupiter is a planet and not a star. Besides, that explanation cannot even remotely explain the reports.

The most interesting fact is that Hynek did not even decide to go to Texas by himself: it is de Vaucouleurs who requested that he make the trip! He called Hynek on the phone and started talking about *Anatomy*, asking what he thought of it. Always the crafty politician, Hynek expressed some reservations at first. De Vaucouleurs countered him: "But it is excellent!" he said. "How can you say otherwise?" Hynek told me that he quickly agreed with Gerard, and they came to the main subject of the local sightings.

It turns out that one of the observations was made by radar operators at Tinker Air Force Base. To get quick help they called ... the Highway Patrol! Indeed, a number of cops saw the objects independently and another radar tracked it as well.

Two pictures have been taken during the Texas flap. Hynek extracted them with some difficulty from his bulging briefcase to show them to me. They seem genuine. One of them shows a luminous source moving up and down over a constant background where the stars have left characteristic trails.

In Austin itself one of de Vaucouleurs' students saw an object which looked very much like the disk reported to me by Alain over Chicago. Gerard grilled his student for two hours, covering the blackboard with figures in front of an amazed Hynek, whose investigations rarely get into such a level of painstaking detail.

One of the satellite tracking stations set up by Hynek in South America observed three objects and sent the data to him. I wish Muller had done the same at Paris Observatory in 1961.

But the most memorable thing is a conversation Hynek had with a pilot who told him about an incident that had taken place last July 3rd, at about 7:30 p.m. while he was flying over Canada on his way to Montreal. It was already dark on the ground when one of the passengers knocked on the cabin door and asked:

"Do you mind telling me how high a dirigible can fly?"

"Not above 10,000 feet."

"And how high are we flying now?"

"About 35,000 feet." The businessman seemed taken aback, then he pointed out the left window and said:

"In that case, can you tell me what that object is, over there to the left?"

What the pilot saw was an immense cigar-shaped craft about to penetrate a large cloud 10 kilometers or so away. The front of the object emerged from the cloud while the back had not yet entered it. With his binoculars the pilot saw five rows of windows over the entire length of the object, which he estimated at twice the size of an aircraft carrier.

Two Canadian jet fighters came into view at that point, looking very tiny as they climbed towards the craft. The cigar seemed aware of them. It accelerated in a smooth, continuous manner and disappeared in a few seconds with a fantastic display of blinking red lights in multiple rows along its whole body.

The same witness stressed that saucer sightings were common among his colleagues but that nobody would report them for fear of being sent to a psychiatrist and losing his job. "Why don't you conduct a survey of retired pilots?" he suggested.

Hynek confided to me he now realized that the time had come to do something, perhaps by contacting Leonard Marks or even J. Edgar Hoover. As for me I feel rather tired at the moment. I am supposed to prepare another silly examination that covers the history of industrial engineering and the tricks of good management. But I am thoroughly disgusted with this fellow Taylor who had defined cooperation between managers and workers by the fact that "the workers must agree to do everything they are told to do, without asking questions and without making suggestions!"

Janine, Olivier and I have reached a turning point: For the first time in a year we are alone together. This will mean a lot of work for Janine, also much happiness at our intimacy. In spite of my tiredness I am becoming myself again. In a few weeks Autumn will come, bringing delightful rain, the freshness of the air, the rich smell of dead leaves.

### **Chicago. 16 August 1965.**

This year's wave of sightings has already brought two new facts: first, a truly accurate description of a small occupant seen at close range, thanks to the clear and specific account given by Maurice Masse in Valensole.

Second, the fact that the saucers are now allowing themselves to be seen on radar and fly slowly enough to be tracked over populated areas.

**Chicago. 19 August 1965.**

Hynek has forwarded a copy of a letter from Colonel Spaulding:<sup>18</sup>

I have recently discussed with Bob Wilson of the National Academy of Sciences the possibility of that organization responding to an Air Force request to evaluate all known information on the subject of UFOs. I would like to have your reaction to this proposal.

Hynek asks me to comment. What's the Air Force up to? I smell a rat.

**Chicago. 22 August 1965.**

Most of my energy now goes into staying focused, memorizing facts and figures for my next exam. I earn a meager salary by writing computer programs for Children's Memorial Hospital and for the department of Material Science at Northwestern, but I am wasting long hours I could be spending at home with my precious wife and my little boy.

A young man from Chicago has called me after reading *Anatomy*. His name is Donald Hanlon. He wanted to tell me about his research on some old local sightings and on the Fatima miracle. We will be meeting on Tuesday night. I hope he will prove to be a good recruit for the Little Society. There is no hope of finding another Aime Michel here, but a group of compatible minds, comprised of specialists in different areas, could make faster progress than Michel alone. It is for that reason that I keep the Little Society going.

Hynek acknowledges that our group has already played a major role in allowing him to voice ideas, hypotheses or scenarios that he could never have articulated before a dry and formal scientific gathering. When we meet in the evening around one of Alain's special pastries (Hynek's favorite is strawberry cream cake), a cup of Janine's strong hot coffee in hand, we can let our imagination soar. Allen's current dialogue with Spaulding was ignited as a result. Carl also says that by taking his mind off his dissertation our evening sessions have helped his own research. He has even begun to work with Harry Rymer on the problem of quasi-periodic functions. As

for Sam, who is endowed with a photographic memory, he is always ready to supply a mine of amazing bibliographic references.

Chicago. 28 August 1965.

Don Hanlon has surprised and impressed everybody. He turns out to be a twenty-year-old printing worker, with a deep mind, very well-versed in the literature of the paranormal. Not only has he read a lot of books, but he has corresponded with Carl Jung and Ivan Sanderson.<sup>19</sup> His views on Fatima and the apparitions of the so-called "Blessed Virgin" are strikingly similar to those of Aime Michel. It is for people like him that I wrote *Anatomy*.

**Chicago. 2 September 1965.**

After lunch I came home, tired and vaguely disgusted, leaving behind the book I am supposed to be studying. It is still on my desk at the Technological Institute, open at the page entitled "Definitions of the Function of the Personnel Director."

Hynek has written to me again to explain his answer to Colonel Spaulding, in which he suggests forming a panel of civilian scientists "for the express purpose of determining whether a major problem really exists." He adds:

If the preliminary survey of the problem should bear me out, namely that there exists the possibility of new scientific information in the UFO phenomenon, then definitely let the recommendation be made to have the National Academy of Sciences, or some other civilian group of recognized stature, undertake a longer-term study of the problem. I would offer a strong opinion at this juncture: even the preliminary panel should be a working panel.

The success of *Anatomy* has triggered new interest for the topic in the media. The producer of every significant radio or television show in the country has received a copy of the book, at a time when the recent wave of sightings in the Southwest is forcing a reappraisal of the reality of the objects. Many new articles and television appearances by believers are fanning the flames. We speculate that is why Spaulding is now proposing a study by such an august body as the National Academy of Sciences. But are they serious?

**Chicago. 6 September 1965.**

We stayed up until 3 a.m. to watch the UFO program on the local Norman Ross show entitled "Off the Cuff" in which I participated with Bill Powers, Dick Lewis and several local astronomers. We had asked Don Hanlon to come over for dinner. Our first impressions were strengthened during this long evening. He has a powerful mind. He is intellectually alive. His interests and his passion for learning range over a wide array of topics.

**Chicago. 11 September 1965.**

A very typical debate has taken place on a local radio station, pitting Menzel against two Chicago NICAP men. The public was invited to ask live questions and these were, by far, the most revealing: The average American now knows as much about the subject as the NICAP members, and displays a better ability for rational thought than Menzel, who was emotional throughout the program. He kept wrapping himself in his professorial toga while the two NICAP representatives fell into his traps "like supporting actors setting up funny lines for a great comedian," as Harry Rymer remarked. All the cases they brought up were ridiculous. For instance, when they mentioned the Brazilian sightings at Trindade, Menzel jumped all over them:

"Now, did you personally research this specific case?"

"Not really..." they said.

"Well, I did, and let me tell you what I found out," continued the director of Harvard Observatory, flattening them mercilessly.

Hynek came home yesterday. He leaves again for Great Britain on Tuesday. Since I put him in touch with Gordon Creighton they have become great friends. Creighton is organizing a dinner in his honor next Thursday. Today I will introduce Don Hanlon to him, to give Don an opportunity to show Hynek his excellent research on the 1897 wave. I felt very proud to have unearthed several forgotten cases from that era, which I published in *Anatomy*, trying to re-awaken interest in

the UFOs of the American past. But this pride was soon deflated when Don arrived at our house, smiling mischievously as he presented me with roll upon roll of newspaper facsimiles showing many old sightings of which I had never heard. He has shown me his notes on dozens more.

Three important things have happened since Hynek's response to the Pentagon:

1. Spaulding got very mad at Hynek, yelling that "all his plans were going to go up in smoke." The reason for this may be that Hynek has demanded that the committee be an effective working group, not a phony setup like the Robertson Panel, which worked superficially for only two afternoons and signed a statement which was merely a rubber stamp for the Air Force decision to squelch the whole subject.

2. The Chief Scientist of the Air Force, Dr. Markey of MIT, has made the remark that Hynek could always send a copy of his letter to the Secretary of the Air Force if he didn't like the way it was handled by Spaulding. Of course, he added casually, such a move might lead to ending his contract. Hynek thinks that if he pushes too hard the Pentagon will purely and simply dissolve Blue Book, which appears more and more as an expendable escape valve, not a genuine research project.

3. Hynek had a conversation with Sagan, who suggested the following: If the saucers exist physically they must be detectable by radar. Therefore it should be possible to get information from the aerial defense system to find out if they have such targets on their screens, and how often. Hynek took the suggestion seriously and he forwarded it upstairs as a formal inquiry. This triggered a whole upheaval. There were meetings high up in the Air Force hierarchy, and finally Winston Markey was again sent to tell him that the whole thing was unthinkable. His very words were "*We advise you to pursue this UFO matter no further!*" As Hynek remarks with some bitterness, that statement is rather remarkable, coming between the Air Force and an observatory director who has served as its consultant for eighteen years!

Now he wonders if he has not been duped all those years, if there isn't indeed an astute plan afoot to hide the truth from the scientific community and from the world at large. Perhaps Project Blue Book has been a cover from the beginning, operating thanks to the collaboration of a few scientists who go along with anything the military tells them to do.

When Hynek examined the files of photographs gathered by the tracking stations he had set up around the world, he found entire series of UFO observations from all twelve of them, some of which he saved for me. Something very grave is going on, but it is impossible to put a finger on it.

**Chicago. 13 September 1965.**

A French television team is in town. We have shot a long interview that consumed most of the day. Tonight they took us to dinner. I managed to keep the interview on a scientific level. I even quoted Danjon, the director of Paris observatory, who has said "a subject is only scientific by the manner in which it is approached."

Aime Michel has sent me a disappointing letter dealing entirely with the sequelae of the Valensole landing. The witness has confessed that he has indeed received from the 'little pilots' a message that was not simply composed of gargle sounds but had a specific meaning. He refuses to disclose what it is when Michel interrogates him. Since I am studying the apparitions of the Virgin Mary at Don Hanlon's suggestion, such contact stories with attending messages do not surprise me any more. We may be dealing with the same class of phenomena, masquerading through various types of entities adapted to each culture.

**Chicago. 14 September 1965.**

The rain and the fog are back, thank Heavens! Harsh grass is growing over the land reclaimed by the campus from the wide and menacing expanse of Lake Michigan. Furious waves kicked up by the storm crash on the rocks around the site of the new observatory. I love this powerful lake; it gives me a new measure of things.

**Chicago. 15 September 1965.**

Another good letter from Aime, who sends me a whole stack of documents. It seems that farmer Maurice Masse has indeed had a contact with the operators, who are supposed to have told him they would come back in October, but he doesn't want to give any details. There are indications that the object was hovering around on the nights preceding the landing, presumably waiting for the propitious moment to make itself seen.

**Chicago. 17 September 1965.**

Harvey called yesterday morning to report that Ace Books, the mass paperback publisher in New York, is interested in the rights for *Anatomy*. This would raise the distribution of the book to the hundreds of thou-

sands. And last night I found a copy of *True* magazine at the newsstand, with the condensed text in it. I have trouble believing that this is going to be read by three million people.

**Chicago. 19 September 1965.**

A book I just finished reading seriously at Don's urging raises the possibility of other physical dimensions interacting with consciousness. Under the title *A Woman Clothed with the Sun*, this work by John Delaney is a summary of alleged apparitions of the Blessed Virgin Mary, otherwise known in the literature as the BVM. The reports are extraordinary, very troubling. One is tempted to imagine there is indeed some sort of power that follows, and intervenes into, human affairs. I want to study these observations in more detail, and to compare them with the events related by Charles Fort, whose complete works I am also in the process of reading. I am spending the weekend compiling more sighting index cards. There is so much to learn! The model of previous waves continues to be observed: large cigars, landings, and so on.

**Later the same day.**

The radio news drones on, spewing out the endless nauseous garbage of human stupidity: some nation "will seek the adoption of a resolution to condemn\_\_\_" The world is bloody again. This is a miserable planet, mutilated, covered with scabs and bruises. The men with whom I work pretend, as I do, to be virtuous scientists in one of the great civilizations in history. Indeed, tomorrow I will go back to my well-ordered campus, with its nice lawns, the wealth of business well-done, the easy gymnastics of serene brains well-taught.... But around us are agonizing masses, fierce beasts. A Marine pilot comments that it's a real pity the Viet Cong don't have airplanes, because it would make the war so much more fun if we could shoot them down! An Indian soldier swears he will die at his post rather than give up one inch of national territory. A Pakistani crowd yells slogans: They will fight to the last man, says the translator, to make Indian blood flow like a river. And the object of all this heroism is a narrow band of useless terrain no one has ever heard of, between two precipitous ridges where nothing can grow.



**Chicago. 24 September 1965.**

The flu keeps me in bed again and my head is crowded with new ideas. I have spent the week revising my notes for the next examination. To take my mind off my studies I have been reading *L'Express* and it has made me angry at the flood of confused verbiage that keeps coming from France. There is an economist, for instance, who has discovered that "the flexing of the *conjoncture* belongs to the past!" In the meantime *Planete* sends me a brochure written by Louis Pauwels, announcing a new Encyclopedia of Human Civilization in twenty-three volumes, no less, whose complete collection "shall reproduce the spectrum of light, the rainbow, the Ark of the Covenant." As if this were not silly enough the great thinkers at *L'Express*, whose intelligence cannot function unless it quotes other great thinkers, mention "that fine observation by Louis Armand: *Culture is the Actualization of Heritage.*"

There is something awesome and admirable about this stream of stupidity which weaves important words into meaningless sentences. It has an effect on the mind which is not unlike a powerful anesthetic. I am almost tempted to go back to that France now governed by the Angel of the Bizarre, that France where I am ready to bet there is really a Monsieur Louis Armand. I imagine he sits in a well-appointed office with fancy carpets, surrounded by a team of bright young economists.

Whenever we see the appearance of a repetition in history, that is a sure sign that the true significant events have passed unnoticed. When a nation puts at the helm in 1958 the same man, no matter how charismatic or brilliant, who ran it twelve years before, as the French have done with De Gaulle, that is a sure sign of decay.

A mass of polar air has descended over my stately, sturdy Chicago. In the icy wind workers continue to assemble the new observatory at the edge of the lake. As it rises it does not look like a pioneer fort any more, rather like a great warship. Many ideas are brewing within our group. Bill Powers is writing to Costa and has started some experiments based on his work, but he has not been able to exhibit a non-classical effect on gravity.

Chicago. 26 September 1965.

Yesterday morning I took the examination again, staggering around with a high fever. I think my answers were adequate. My head is a bit more clear now. I am studying the Air Force files for 1952, and I am surprised to discover how little the American amateurs have understood about the nature of the phenomenon in spite of all the documents they had available from the beginning. Keyhoe's NICAP is particularly naive when it distinguishes six "phases" in the history of the sightings since 1947, with the landings a characteristic of the last phase only: "In 1954, writes one of their reports, there were a few alleged landings but no proof!" APRO fares little better: They now see the sightings as a systematic exploration of American strategic sites. Those alleged "phases" in the phenomenon only reflect the biased framework of the beholder.

By far the two aspects that fascinate me most are the landings and the ancient sightings, especially when I find these two aspects converging towards the issue of "contact," as they do in the apparition-projection cases that involve suggestion. In this sense the addition of Don Hanlon to our group is timely: observations like those of the Virgin at Knock, to which he called my attention, are indeed amazing: they throw into question any research on the phenomenon conducted purely on the classical scientific level.

I am considering more seriously Aime Michel's notion that some higher power is manipulating us. Contact with that power or its messengers (in Greek the word for "Messengers" is "Angeloï") may have given mankind its first concepts of the universe. What is upsetting is the modern recurrence of these contacts and the absurdity of the "messages." what is the role played here by the coarseness of the receiver, or by the noise in the system? Is there a deeper, real meaning in the message? What <sup>is</sup> the possible role of directed hypnosis, of spontaneous suggestion? We know so little about the nature of our own consciousness that we cannot answer these questions.

Aime Michel believes "they" have brains superior to the human brain. I still question this belief today as I did when I first wrote to him in 1958, seven years ago. What right do we have to talk about the human brain, when we know it so poorly? Perhaps we function like a powerful computer which is barely aware of its own abilities while somebody else,

outside, is programming it to use its full power towards an unthinkable goal, or simply pushing the buttons of our unconscious. "Pray more," the apparitions keep saying. Is there a race of beings who suck up the psychic energy of such prayers? Perhaps our major value in "their" eyes is our very stupidity. Everybody likes a nice clean fat pig. But a pig who asks questions about the square of the hypotenuse is an embarrassment to his own species, and an annoyance to his masters.

Perhaps our visitors are not divine or diabolical but just stupid. Bill Powers joked the other day: "Maybe they are some sort of missionaries, who think they do all this for our own good ..." In any case we are manipulated. The thought is not pleasant.

### **Chicago. 2 October 1965.**

Hynek is back from yet another trip, in time for my thesis committee to meet. Now I am seriously starting work on my dissertation. The coming year will be filled with classes, problems, committees, reviews, seminars and lectures. I have selected a very challenging topic in artificial intelligence that interests me and justifies this heavy time investment, namely the unsolved problem of real-time interrogation of computer data-bases in natural English.

During his trip to England Hynek met the major British ufologists, men like Gordon Creighton, Charles Bowen and Le Poer Trench. Most of them, he says, are literary minds who "seem ready to believe anything." All this remains at the level of a general discussion around the table. There was no genuine scientist in the whole bunch, he told me. This makes our own role and responsibility here all the more significant.

There were two important facts this year: on the one hand, the loss of public confidence in the Air Force, which was precipitated by the publication of *Anatomy*, because my book openly showed how much could have been done. And on the other hand, a few clear examples of cover-up on the part of government agencies: witness the Washington case last January.

### **Chicago. 3 October 1965.**

We have just watched the documentary released by NBC. How fragile and irrational we look, Allen and I! In a way the most solid character, stable as the rock of his own strong inner conviction, turned out to be Joe

Simonton, the old Wisconsin country boy who saw a flying saucer land in his backyard. Three occupants came out of it and he is absolutely sure they gave him some pancakes. Besides, he still has the pancakes to prove it! Quintanilla, too, looked very solid as he stood at attention in front of the camera and recited the Officers Handbook, open at the page: "Undesirable Phenomena." But this doesn't fool anybody. Naturally NBC put the emphasis on silly local sightings that proved nothing. They did not use the Toulouse case. They also cut out Vins, which I had spent so much time documenting for them in an effort to show the scientific relevance of the observation. There is an important lesson to be drawn from all this: These people are just entertainers. I was a fool to believe they would seriously try to show how this problem transcended our knowledge. That is not what the public wants to hear. After watching this documentary I measure better the extent of our loneliness, of our vulnerability.

### **Chicago. 7 October 1965.**

The Little Society met last night, minus the Rymers and Carl, who is now married and ready to go teach mathematics at DePaul University. We were joined by a University of Chicago psychology graduate named Michael Czentmihalyi. (We quickly decided to just call him "Mike.") He teaches sociology in Lake Forest. His dissertation topic was creativity in art, which clearly qualifies him for our group. Don was with us too. We reviewed the files of the July-August period. Hynek told us the Air Force had now asked its scientific advisory board to review Project Blue Book. Therefore we may soon have to select our best cases in order to present them to the committee.

I am in a strange mood again. I have some free time to study, but I lack the passion that would make me dive ahead. How could I say that I don't have every happiness? Indeed there is nothing I would change in my life. Deep inside me is the joy and love of Janine, the fullness of our existence together. Yet something else looks for expression. An orchestra inside me is playing Wagner and it won't stop. All doors closed, no sound is filtering outside to scare the neighbors while the Valkyries sweep down from the sky to take my soul away. I try to remain calm and cool while they drag me by the ear into their fog-filled caverns.

**Chicago. 23 October 1965.**

In a single hour yesterday afternoon, Don and I have located three more excellent observations of the 1897 airship. We are systematically going through the collection of the old *Chicago Herald* 2.x. the Midwest Inter-Library Exchange Center, a collection so ancient the yellowed pages crumble between our fingers.

In the evening Bill joined us at the Hilton for a meeting with Jim Moseley, a UFO researcher who edits a magazine called *Saucer News*, almost entirely devoted to amusing personal quarrels and petty gossip. He was waiting for us with three of his friends. They tried to capture our conversation on a hidden tape recorder, but we managed to elude this obvious trap. We failed completely in our attempts to convince Moseley that his magazine could fulfill a sorely needed role if he could only give it a more serious orientation, but I liked his sense of humor.

**Chicago. 30 October 1965.**

Bill Powers and I are becoming close friends. I have begun to study the original psychological theories which he is developing, based on cybernetics. He used to work in the psychology department at the University of Chicago, but he was getting no recognition for lack of a doctorate. His sense of humor, too, was a liability among professors who took themselves very seriously. He is remembered there as the inventor of the Diesel-powered Pogo stick, among other zany contributions.

Hynek called me today to tell me that the UFO question was coming before the Air Force's scientific advisory board on November 5th. If they decide to launch a special investigation, as he is hoping they will, Hynek expects to be called to Washington. Things could move very fast, he said with excitement in his voice.

Last Wednesday we had a full group meeting at Bryn Mawr. We had invited over a man who had written to the Air Force, insisting that he had witnessed UFOs over Chicago and knew the whole secret behind flying saucers. He brought with him amazing rolls of paper covered with diagrams, and he proceeded to give us a grotesque demonstration of his obsession. It was painful to watch him. He believes there is a machine in the sky, hovering over Lake Shore Drive, and it controls him. It is as large as a city and shaped like a baker's rolling pin. It is filled with wash-

## BLUE BOOK

ing machines and staffed with dead people.

He also believes that each of us is connected to the machine in the sky by a sort of psychic umbilical cord which should never be tangled or knotted, and this fact naturally places our orbiting astronauts in extreme danger. Indeed, we observed that he was very careful not to tangle up his own cord when he moved around the apartment on his way out. He retraced all his steps very precisely when we managed to convince him it was time to leave. He went away happy in the feeling he had enlightened us, and he drove home while we wondered what would happen if the big machine in the sky came to get him while he was driving in traffic at 70 mph.

"I'll think about this guy, every time I take the Lake Shore Drive," said Bill.

That was not a very pleasant thought.

Chicago. 14 November 1965.

I am doing a survey of all the UFO groups I know around the world, and I am using the weekend to bring my correspondence up-to-date with France and England. I corrected the galleys of the French text and I found time to prepare a seminar in mathematical logic based on Novikov's book.

Last night I walked through the "Old City." Chicago is a prodigy, built by humorless giants who have now faded into history, leaving behind deserted, dangerous streets. The only thing alive is the great North wind, howling in despair, looking in vain for the passions and the excesses of yesteryear.

This is not a sad city, but it is buried so deep in its own wealth, or its private misery, that it has no use for such luxuries as public squares with benches, corner coffee shops, or a common language. It feels nothing and cares about nothing. It lies squarely in the hands of corrupt cops. An outside pretense of Morality rules even what is left of the old Mafia. Chicago is a middle-aged whore struck by a mystical crisis, who thinks of entering a convent. It is a suburban matron counting her money, and a little black boy who cries for the toys he will never have, unless he steals them. The great common denominator is the wind.

**Chicago. 15 November 1965.**

With the imminent publication of *Challenge* in French and of *Anatomy* in paperback, my year of residence at Northwestern almost completed, I should have the right to catch up on my sleep. However, I do work hard and with much pleasure on my thesis research; the program modules are falling into place one by one, and I read everything I can find about artificial intelligence. I look forward to earning some money again when all this is over. I have considered jobs at the University of Chicago or at the Illinois Institute of Technology but that would mean leaving Hynek. He wants me to stay at Northwestern.

**Chicago. 26 November 1965.**

Life has become a bit slower and easier, although we are utterly broke again. Last week a French television reporter came to see us on the recommendation of Aime Michel. He has been assigned the general topic of scientific trends in the U.S. I tried to give him some orientation. He was also bringing us some concerns about Aime, who suffers from sudden fever attacks and once again fears he may have a brain tumor.

Janine, Olivier and I spent Thanksgiving with the Hyneks, as we have done in previous years. At their house we met Chinese astronomer Su Shu Huang, who is joining the astronomy department faculty. He is a sincere and pleasant man. He looks like a perfect example of the kind, brilliant cartoon theoretician, like Tintin's brilliant and zany *Professor Calculus*. As we discussed the question of possible visits by interstellar travellers, I showed him the probability function I had drawn in the appendix to *Anatomy*. As we were about to sit down at the table where Mimi was serving the turkey he tapped the book, smiled and said, with a heavy Chinese accent: "If I had been asked to draw this curve, I would have guessed it was exactly the way you did it here!"

**Chicago. 28 November 1965.**

The wind was icy and pure when we visited the construction site for the new observatory today. Everything had turned to pastel colors: the beach, the grass, the sand, the sky.

It has been three weeks since the Air Force scientific advisory board met in Dallas. Hynek has not been asked to join them. Repeated phone

calls from him and from Quintanilla have not provided any information about what was discussed, or any subsequent decisions that may have taken place.

Finally Hynek did hear something last week: Colonel Spaulding had met one of the Board members in the men's room, and he was told that "the Board thinks it has found a way to approach the problem."

So this is how science works! Our future research depends on a few words some military fellow overhears while pissing in the latrines of the Pentagon. That is a fine image of this society, and of the way it makes decisions.

While all this intense intellectual work was going on in Washington we did not remain idle. Instead we are stubbornly plowing ahead with the study of both old and recent sightings. On Thursday Hynek and I called the witnesses of the Cuernavaca case, where a massive power failure followed the sighting of an aerial object. We have to do this privately because the Air Force does not want any more "good" cases submitted to them, *for fear their statistics would deteriorate*. It would be a disaster for Blue Book to have to report to Congress any increase in the percentage of unknowns.

I am still patiently sifting through Aime Michel's original files. I have found a new package of data about the landings of 1954. What an amazing period\_\_\_And what a shock it would be if these facts were ever presented publicly in the light they truly deserve! Among our group only Don Hanlon has understood the full scope of that wave.

In Aimé's files there are many letters from witnesses and from his readers. I am surprised and impressed at how serious and articulate they are. They show a sober quality, a high level of intellect, far above that of the journalists and the scientists who comment on the cases without bothering to study them.

### **Chicago. 3 December 1965.**

Over lunch Bill Powers and I have been talking about the operators of the craft.

"In some cases," I said, "it almost seems that they are not real beings, but artificial humanoids."

"Yes," he replied, warming up to the subject, "they could be noticing machines, with fast pattern-recognition abilities! In a few minutes on the ground they could gather reams of data about us, couldn't they?"



Any rational definition of thought implies the theoretical possibility of building an automaton that will exhibit that activity. I am studying Markov's theory of algorithms, and of course Alan Turing, looking for clues. The problem of UFOs boils down to two major issues: first, the theory of time and gravitation, then the question of the nature of human intelligence.

Today we are happily celebrating Olivier's second birthday.

**Chicago. 5 January 1966.**

Classes started again this morning under a magnificent, pure sky. Perhaps it is a symbol indicating that the end of this agony is in sight for me. I have passed the qualifying examination this time. I have registered again for my last mathematics courses, although this means no gainful employment for another quarter, and penny-pinching until next summer. This month again Janine had to go tell our Greek landlady we would be a little late with the rent money. But the computer part of my research is progressing well into uncharted territory: The problem I am attacking is the automatic translation of English-like, natural language questions into computer programs that consult a data-base of ten thousand stars.

**Chicago. 10 January 1966.**

Some enormous machinations are under way in Washington. Were they caused by the series of new sightings that took place last summer, and are still unexplained? More likely it was a combination of several factors, including these new sightings, NICAP's public statements and the repercussions of my book, if I judge from the letters and the phone calls I am receiving. In any case, someone has become concerned and the huge wheels of the Pentagon have started turning. Hynek has been pushed aside and Project Blue Book was ignored as a minor pawn in the new game.

It is practically in secrecy that the Advisory Board met in Dallas. After a long silence we have learned that a physicist, Brian O'Brien, had been placed at the head of a special scientific committee charged with making new recommendations. He had *carte blanche* to staff his group. He began by selecting Carl Sagan<sup>20</sup> who is an avowed adversary of the reality of the phenomenon. From the choice of members it is easy to deduce

what the final decision is likely to be. After all, said Hynek, "Carl won't become a full professor at Harvard by taking flying saucers seriously. And he knows that!"

Hynek had already been told "pursue this UFO question no further!" And hints had been given that he would lose his Air Force contract if he displayed any real independence. Now it is clear that the O'Brien committee wants to see the files; ***but it will ignore the advice and the contributions of those who are in a position to know about those things which are not in the files.***

What foolish illusions we had! And what a lesson in science! Scientific honesty and objectivity appear as a travesty. It was absurd of us to think we could make a difference. Research under such conditions is a meaningless ritual, since truly important issues can so easily be treated behind closed doors by groups already committed to burying the truth.

Chicago. 17 January 1966.

Hynek calls me from New Orleans: Winston Markey, the chief scientist of the Air Force, has just told him of a UFO observation made by Roger Woodbury, associate director of the MIT instrumentation lab. The story is simple: His son rushed into the house screaming there was a flying saucer in the sky; the professor came out and saw it. Now Markey may be forced to take the subject seriously.

Chicago. 22 January 1966.

The Little Society just had a meeting attended by Hynek, Bill, Sam, Don, Janine and me. Hynek wanted to talk about everything that has taken place since the publication of *Anatomy*, including the Woodbury case and the sightings at Exeter that writer John Fuller has recently reported in the *Saturday Review*.

I have received an invitation to participate in a television debate in New York with Hynek and Menzel, to be chaired by President Johnson's cultural adviser. I declined, because I just have too much work ahead of me. Besides, trying to change Menzel's viewpoint is a big waste of time, in my opinion.

For several months I have been trying to convince Hynek that we should prepare a formal research proposal to centralize observation files at Northwestern, with the establishment of a technically competent office

to analyze the significant reports. He has agreed that the time has indeed come to develop such a proposal.

Janine and I are slowly emerging from another period of deep exhaustion. We have been working like dogs all week. Wednesday night, after the meeting with Hynek, we wrote computer programs until two in the morning. In the last two weeks I have also drafted two scientific papers, one to be co-authored with my adviser Gil Krulee and the other with Hynek.<sup>21</sup>

### **Chicago. Wednesday 26 January 1966.**

For the last few evenings we have been having intimate dinners in the kitchen, just the two of us, by candlelight. Olivier likes to tell long, complicated stories to the butterflies, the cats and the birds I have painted on his bedroom walls. Janine radiates sheer, sweet beauty. Our nights are splendid. The snow that falls outside seems to insulate our love from the whole world. When she smiles at me all difficulties vanish. But if she is serious or concerned, I suddenly fail to see anything ahead. If she frowns, if she doesn't come over to kiss me often enough I lose my place on the charts, I drift aimlessly for hours like a lost raft.

### **Chicago. Thursday 27 January 1966.**

My two thesis advisers had lunch together today to discuss my research findings and the theory behind the new system I call *Altair* ("Automated Language Translation and Information Retrieval").

To their surprise it is now running on the Northwestern computer, a Control Data 6400. It is hard for anyone to question the theory once they have eaten the pudding. *Altair* takes English-like questions like "What is the proportion of spectroscopic binaries among stars bluer than K2?" and it converts them into internal formulas that enable it to search through the entire catalogue of bright stars to give a numerical answer in a few seconds.

By contrast, the same problem previously required writing a special program and running it each time against the catalogue tape, with all the attending chances of programmer or operator errors, and a twenty-four-hour delay to get the results.

The significance of this is that a similar program based on the *Altair* framework could follow stock market quotes at the Exchange and answer

questions such as: "Which specific companies have shown a growth in earnings greater than ten percent in one year among Utility firms with price-earning ratios lower than twelve?" At Children's Memorial Hospital, a doctor who knows nothing about programming could walk over to a terminal and type: "What is the percentage of infants who develop complications that include a high fever within ten days after an operation for cardiac malformations?"

Following publication of *Phénomènes Insolites* I am getting reactions from my frustrated friends in France. Guerin wants to be vindicated. Aimes reaction is different. In the United States he would have achieved the recognition he deserves a long time ago, but France has nothing but contempt for people who are ahead of their time. Now he is bitter and dreams of revenge. Both Aime and Guerin tell me, "You're not going to convince anybody. Your book says clearly that the calculations do not lead to firm results. People will take that as an excuse to reject the whole problem."

Fine. That is their right. I cannot prove to them that this phenomenon exists. Indeed, our theories about UFOs are still obscure and contradictory. But one could say the same thing about many fields of science, like elementary particles or cancer research.

### **Chicago. Friday 28 January 1966.**

A haughty academic, Professor David Park, of the Thompson Physical Laboratory at Williams College, has written an irate letter to my publisher to complain about *Anatomy*. He does not like my quoting the astronomer who said, a few months before *Sputnik*, that "Space Travel is Utter Bilge."

To choose a distinguished scientist who is now dead and unlikely to make a fuss, and quote his words in a context which makes it appear that he was an idiot, deserves an appreciative chuckle from any huckster. I will not detain you with an explanation of what Spencer Jones was talking about; probably whoever found the quotation can enlighten you.

I answered immediately:

Before accusing me of quoting such a distinguished scientist out of context, I believe you should check your own sources a little more

carefully. If Spencer Jones said anything about space travel being utter bilge, I am not aware of it. I was making allusion to a statement by Sir Richard Van Der Riet Woolley, Astronomer Royal and former director of the Commonwealth Observatory at Mount Stromlo, a statement which he *repeated* in a speech to the Press Association annual meeting on June 15, 1960: "I said space travel was utter bilge when I arrived here four years ago. It remains utter bilge."

I thought it would be useful to add:

To the best of my knowledge Sir Richard Woolley is still living, and probably still of the opinion that space travel is utter bilge (...) This is certainly an illustration of the dangers of the Principle of Authority which has so much contributed to the decline of European science. I believe I detect in your letter much contempt for those who dare question the words of the so-called "great" scientists. Let me remind you that the tasks of the Astronomer Royal were defined by King Charles II of England as follows: "The Astronomer Royal shall apply his care and activity to the rectification of the tables of celestial motions and the positions of fixed stars, in order to be able to determine the much-needed longitude of terrestrial locations and thereby increase the perfection of the Art of Navigation." The King said nothing about space travel. Sincerely yours, etc.

I do not expect to hear from Professor Park again.

### **Chicago. Sunday 30 January 1966.**

Costa de Beauregard and his assistant have arrived in the U.S. at our invitation. They landed at O'Hare this afternoon. We greeted them and took them to Bryn Mawr without delay. The temperature was 25 below zero, but Janine warmed us up with a wonderful French dinner. Costas lecture on Tuesday will be entitled *Modern Ideas in Field Theory*. This will be followed by a slide presentation of their experiment. Dr. Chapman, a physicist from MIT, will be in attendance.

**Chicago. Wednesday 2 February 1966.**

The lectures are over and we have had ample time to discuss our favorite subjects in private. At the latest meeting of the Little Society, attended by Costa as guest of honor, Hynek conducted a telephone interview of a New Jersey policeman. He had seen an object similar to that observed by Woodbury. However no one, not even Costa, has any new theory to offer to explain the physical effects of UFOs, and I find this very disappointing.

Costa did tell us a few anecdotes of his work with Louis de Broglie. They do not agree on the subject of relativity any more, and this has created a rift in their long-standing friendship.

"It's really a pity to get to that point," comments Costa seriously. "There are so many other things in life we could discuss, and agree on. Quantum mechanics, for example!"

**Chicago. Friday 4 February 1966.**

It turns out the Brian O'Brien panel has recommended that the Air Force spend about \$250,000 a year to scientifically document a number of selected sightings. At least that is what Hynek has heard, and he is going to propose that the work be conducted at Northwestern.

Aime Michel is feeling better. He is encouraged, he says, by the good reactions our book is enjoying in France.

This afternoon I briefed Costa on the features of the sightings with special physical interest. He listened carefully, and encouraged me to compile a natural history of the phenomenon. He compared my research to what physicists were doing in the early days of electrostatics, which appeared just as absurd as UFOs do today. He believes the objects could be looked at from the point of view of magnetism (are they magnetic monopoles?) and of artificial gravity.

There was a strange incident when Costa and his assistant were at Bryn Mawr. The bell rang and I went downstairs to open the door. A jovial man I had never seen before shook my hand in a funny way. He proceeded to explain with much effusion that he was from the local Masonic Lodge and wanted to give his brotherly greetings to Costa's companion.

I invited him in. The man openly explained his rank and his role in

the organization, much to our guests embarrassment since Masonry, in France, attempts to preserve its status as a Secret Order. In America, of course, it has become little more than a social club.

Later Costa told us: "That's funny, I have worked with this fellow for many years, but I had to come to Chicago to discover that my assistant was a Mason!"

**Chicago. Sunday 6 February 1966.**

Jim Lorenzen called me this morning to say that Dr. Olavo Fontes, the best expert on Brazilian cases, would be changing planes in Chicago this afternoon. Since we were driving our two French physicists to O'Hare anyway, Don and I waited around for Fontes, who was travelling with his wife and three children.

Dr. Fontes is a small-built, energetic man with intense gray eyes, dynamic and serious, sincerely worried about UFOs. He finds certain correlations in the data very disquieting. For instance, the apparent relationship between the objects and power failures in large urban centers during the most recent wave bothers him: he wonders if control of energy is not the ultimate phase of "their" plan.

I asked him to clarify a statement he had made, that the phenomenon might be hostile.

"They are not necessarily hostile by nature," he said, "only they don't allow us to interfere with their plans; they use violence if necessary, whenever a witness stands in their way."

**Chicago. Saturday 12 February 1966.**

To a great extent Americans are right when they claim their country enjoys the most freedom in the whole world. But they would be horrified if they could measure how quickly the very expansion of their society is eroding that freedom. I love the United States, the flowering of ideas, the genius in organization and in management that exists here. Having seen this, one can only feel contempt for those nations that only have small problems and still succeed in drowning in them. But I **don't** like what the U.S. is becoming.

**Chicago. Sunday 20 February 1966.**

Pursuing the detailed study of the landings of 1954, which represents a massive amount of work, I discover an inverse correlation between the number of reports and population density. As I plow ahead through piles of documents more and more details are accumulating: dates, times, descriptions of the craft and their pilots. But the most important key is still eluding us.

**Chicago. Tuesday 22 February 1966.**

A strange surprise: I just found my picture in a Japanese magazine, in the middle of an article I naturally cannot understand. I can only guess it's a review of *Anatomy*. The book was also cited in the *Congressional Record* by Representative Pucinski, a Democrat from Illinois.

My article with John Welch about respiratory mechanics following major surgery is being published.<sup>22</sup> Thus everything is moving forward, and we are beginning to think about our next trip to Europe. The American Mathematical Society, of which I am a member, will participate in a major international Congress coming up in Moscow this Fall. For the first time information science will be admitted as one of the topics, and the Soviets have promised to unveil some of their computers. I have decided to submit an application to go there as part of the American delegation, in the hope this will give us an opportunity to try and find out discreetly what is happening to UFO research in the Soviet Union. Officially, of course, there is no such research.

**Technological Institute. Thursday 24 February 1966.**

It is 9:30 a.m. and I feel distressed. I reached my office, as I do every morning, after driving my little boy to the sitter's house and leaving Janine at the subway station. I parked the car in the large open area at the edge of the lake. It is icy cold here, the shore is frozen, the waves are carrying big white plates all the way to the horizon.

I sat down at my desk and started dreaming. I feel exhausted. Yet I am not sick, I have no valid reason to interrupt my work, to take a day off to rest. The truth is that for the last three nights I have slept very poorly. I keep tossing and turning. In the purple darkness of our room I feel unable to rest. My mind is confused and unsatisfied. Every day I get



more letters from readers. I no longer answer them, as I used to do. I classify them and I forget them.

Bless you, Janine, you are all I have.

Chicago. **Sunday 6 March 1966.**

On Thursday a letter arrived from Hynek, who wasn't supposed to come back until next week. As he was on the campus of the New Mexico School of Mines in Socorro he suffered a sudden loss of consciousness and fell, breaking his jaw in two places. Now he can barely speak. The doctor says he must slow down. But I saw him the next day, that devil of a man, and I heard him complain between his teeth against this "stupid accident" which prevented him from jumping into yet another airplane to fly to Nebraska and interview the witnesses of a recent case. Perhaps this time he will be forced to stay in town for a little while: I took the opportunity to remind him we should be polishing up our research proposal. He gave me the green light. Don spent Saturday with Hynek and me, as we selected the strongest cases that we intend to append to the document.

It was a slow and amusing process: Hynek sat in his armchair in the corner of the living room, near the large hanging light, and I took the sofa, with my back to the lawn and to Ridge Avenue. We spent time watching television: Saturday morning cartoons are a major distraction for him, a sacred ritual that can only be interrupted under the most extreme circumstances. Then we listened to music. Hynek's preferences run more to Offenbach than to Mahler. In the middle of our discussions he occasionally picked up a pencil to direct an invisible orchestra through some fast movement.

Chicago. **Monday 14 March 1966.**

In spite of our progress with the day-to-day research I have reached a pessimistic conclusion: nothing significant is going to happen here. The Air Force has other fish to fry. Quintanilla even says that the recommendations made by the O'Brien panel will probably be rejected by the brass. The whole problem may then return to that obscurity from which mysterious people in Washington hope it will never emerge.

Part Three

**PENTACLE**

# 11

**Chicago. Wednesday 23 March 1966.**

After breakfast on Monday morning I sat at my desk at home to resume work on a computer program, and I turned the radio *on*. What I heard told me that nothing would ever be the same again. An information bulletin was flashing on every network, and it had to do with UFOs. Four objects were said *to* have flown over a farm eighteen miles northwest of Ann Arbor, Michigan. One of them landed near a swamp in Dexter. There were supposedly no less than sixty witnesses, including *six policemen, and all the students in a dormitory at a college in Hillsdale.*

At 9 a.m. I called Hynek to tell him about it. I also alerted Don Hanlon. Within the hour Hynek was calling me back: He had spoken to Quintanilla, who said he had *no* interest in the *case*, *since* it had not been reported to the Air Force!

"That's not very scientific," Hynek said scornfully.

"I don't give a damn," replied the Major.

Upset, Hynek just hung up on him. Half an hour later it was Quintanilla who called back:

"Can you jump into the first available plane and head out to Michigan?"

"I thought you didn't give a damn. I thought it hadn't been reported."

"Well, since I told you that, it has been reported."

"By whom?"

"By the Pentagon, doggone it! All three networks are talking about nothing else. The brass is having a fit!"

Hynek had classes he could not cancel, so the afternoon had arrived by the time we could hold a meeting in his office. He made plans to leave the next morning, on Tuesday, and said he would call Bill and me if he needed help in the investigation. However we quickly realized that we could not fly with him for lack of funds. His meager Air Force budget gives him no latitude.

So he left at 9 a.m. on Tuesday, in spite of his broken jaw. I had suggested to Bill and to Harry Rymer that we take Harry's plane to fly over the landing site, but nothing was decided. We went on talking and speculating instead of doing something; I got increasingly frustrated.

More sightings took place during the night. Again Bill spoke of going to Michigan. The radio did mention that Hynek was thinking of bringing other members of his "UFO Committee" to assist him, but he kept procrastinating. On Tuesday night I reached him in Ann Arbor:

"How do you want to proceed?"

"The most important thing you can do is to complete our proposal as soon as possible," he told me.

So I pushed aside my dissertation and the piles of computer listings to extract the draft of the proposal from my drawer.

Now a sudden blizzard has buried the entire Midwest under a foot of snow. The roads have become impassable. The temperature is dropping. Yet the flap continues to develop. Several people are said to have seen an oval-shaped craft in Normal, Illinois, on Tuesday night.

The papers, the radio, all the media are feverish. Hynek is obviously enjoying the VIP treatment he is receiving in Michigan. He has two Air Force sergeants at his disposal, a chauffeur and a Jeep. The military are worried. This is the first large series of sightings since Fuller's widely read article in *Look*. A radical change has taken place in public opinion, the situation is mature. Hynek should speak out.

### **Chicago. Saturday 26 March 1966.**

Two thunderbolts yesterday. First the morning papers came out with what they claimed were "actual photographs" of the Michigan objects. I rushed out to buy some copies . . . and I dropped the paper in disgust. Anyone with elementary training in astronomy could see that the pictures simply showed Venus and the Moon. While the scientists snickered around us, the confusion was increasing among the public. There are new sightings all the way into Illinois and Wisconsin. All four states around Lake Michigan are now involved.

Second thunderbolt at 4 p.m. when Hynek gave his eagerly awaited press conference in Detroit. Every major newspaper, every television network in the United States was represented. He offered his final verdict about the two cases in Ann Arbor: *Marsh Gas!* He reserved judgment on

the other cases. But the reporters were already out the door, rushing for the telephones. Marsh gas indeed!

Now the main witness is being ridiculed and harassed in his own community. Overnight his neighbors vandalized his house, broke his windows, threw stones on his car and phoned at all hours to call him "Head Martian!"

### **Later the same day.**

Hynek has just called me: He felt the need to explain to me his Detroit statement, knowing that I was seriously disappointed, like everybody else.

"There was so much confusion, Jacques, you can't imagine what a pandemonium we had. This just wasn't a strong enough case to base a real offensive on it. I would have found myself in a very fragile position\_\_\_"

"What about the Hillsdale sightings?" I asked. "Sixty girls said they clearly saw the lights from the College."

"Well, I spoke to them too, but I couldn't get a straight story from them. Again, it was all muddled, and the lights they saw were very faint." He chuckled: "You might say that neither the lights nor the girls were very bright\_\_\_"

"At least you could have reserved judgment, you could have said, *This demonstrates the need for research once again, why shouldn't science undertake a serious study?* You could have shown there were clear patterns, even if no single sighting is going to prove anything\_"

He sighed. "Well, maybe I spoke too fast, but you understand everyone wanted a statement from me, some explanation right away. Too bad you weren't here with me. At least I have strengthened my reputation as a hard-nosed scientist."

In the meantime all the cartoonists are having a field day with the astronomer who thinks flying saucers are nothing but marsh gas.

### **Chicago. Sunday 27 March 1966.**

A radical shift in public opinion is continuing right under our eyes. It is so obvious that the objects are real, and so many good cases have accumulated, that people have accepted the existence of the phenomenon. Hynek's statement in Detroit was such a caricature that it is violently rejected by the public—the same public that would have ridiculed and

crucified him only a month ago if he had argued that UFOs deserved scientific study, and a significant expense of taxpayers' money.

It is hard to keep a cool head in this crisis. NICAP plans to give a press conference tomorrow in Washington; Congress may well start new hearings this time, a possibility that scares the Air Force out of its wits.

Hynek has just told us that if he were called before Congress he would hide nothing. He would expose the sloppy investigative methods Blue Book has been using all along. He even asked me to leak a copy of the full text of his Detroit statement to NICAP; it contained a recommendation for scientific study which the media have ignored:

The Air Force has asked me to make a statement on my findings to date. This I am happy to do, provided it is clearly understood that my statement will refer to the two principal events as reported to me: the event near Dexter, Michigan, on March 20, and a similar one at Hillsdale, Michigan, on March 21. It does not cover the hundreds of unexplained reports. . . . I have recommended in my capacity as scientific consultant that competent scientists quietly study such cases. . . . There may be much of potential value to science in such events.

All the journalists, naturally, ignored this cautious preamble, and focused entirely on the misleading "Marsh Gas" explanation. But he deserved it; why did he have to say, "A dismal swamp is a most unlikely place for a visit from outer space!"

**Chicago. Saturday 2 April 1966.**

This week I finished writing our proposal. A media storm is now raging against the Air Force. Congressman Gerald Ford, the Republican from Michigan, has expressed outrage at the suggestion that his constituents couldn't tell marsh gas from spaceships, and he has written a formal letter to the Armed Services Committee of the House to demand immediate hearings. But who will get the job of studying the problem? Can the Air Force be trusted to be unbiased? What about NASA? The Space Committee of the Senate, which controls all the big NASA decisions, has already rejected any notion of considering the UFO question. Congressman Rumsfeld, whom Hynek knows fairly well, has explained to him that NASA didn't want to tarnish its image. So they threw the

hot potato back to the Armed Services Committee. Perhaps they know that some dark and horrible secret may lurk in the basement, and they want no part of uncovering it.

On Friday morning Hynek left for Dayton, expecting to see Quintanilla in Wright Field, but the Major had already left for Washington, where he will stay for six days at the disposal of the Committee, ready to testify. Hynek just called me to say that he had to be in Washington himself next Tuesday. He wants to see me over the weekend to help him prepare his statement, which will be part of the record.

"You ought to see the Air Force people," he said with glee, "they're running around like chickens with their heads cut off!"

### **Chicago. Thursday 7 April 1966.**

The hearings started with the committee in executive session, but the doors were soon opened to the press and to the public. Hynek made his statement without clearing it first or even showing it to Quintanilla. Things were so rushed that he didn't even have time to retype it, so what he pulled before the Congressmen were my four sheets of yellow paper covered with the wobbly type of my tired machine.

He was somber when I called him yesterday morning.

"We've gone too fast," he said. "Maybe we've blown our big chance."

The Secretary of the Air Force, Harold Brown, said in his testimony that he was in favor of having a new scientific committee study the 648 reports that are still carried as "Unidentified" in the files. Now Hynek is afraid the Air Force will think he is too independent and will push Kim aside.

Things were different at noon today. Bill and I had to wait half an hour for Hynek, who was in a phone conversation with an editor of *Science* magazine who had just read *Anatomy*. "Yes," said Hynek, "Vallee is a solid guy."

Does his book describe the phenomenon accurately and fairly?"

"I think so."

Then why did you wait so long, Doctor, to reveal to the scientific world that the problem was a serious one?"

The obvious answer is that the scientific world is as close-minded as an old pig and that his colleagues would have treated Hynek as a dangerous visionary if he had spoken out. He would have been replaced by

some unscrupulous bureaucrat who would simply have swept the data under the rug. But can he say that to *Science* magazine?

**Chicago. Friday 15 April 1966.**

My thesis committee has met and I am ready to start work on the written part of my doctoral examination. Unfortunately I have another bad cold. The *Flying Saucer Review* has just published my "Ten-Point Research Proposal," but I have a strong feeling I am wasting my time with the believers. They don't need to do any research. They already know what the flying saucers are.

Every day there are more cartoons about Hynek, ridiculing his "Marsh Gas" explanation. Oddly enough this has the effect of placing him increasingly in demand as a lecturer. People want to see and touch him, and he enjoys every minute of this newfound celebrity.

**Chicago. Sunday 17 April 1966.**

It is raining on this city, which has turned to a silvery gray. Standing at the window I daydream, watching the shiny roofs of the cars and the puddles in the school yard beyond. The scene reminds me of rainy afternoons in Paris and in Pontoise: memories so powerful, so haunting, of passersby dashing out of bakeries with their bread under their arm; umbrellas opening everywhere like flowers.

I wonder what life would be like if we went back to France. I see a little house, and all my American books on a shelf; I see rain falling on a little garden. What is the feeling that comes with the rain, bypassing the conscious mind, when we have made love too long and too hard, when the brain has been dulled by great stunning waves of life? "I come from the great universe," says the raindrop perched for a moment at the edge of the roof. "I shall make a torrent that will sweep your silliness away into the gutter. I am here because men ignore their own greatness."

**Chicago. Tuesday 19 April 1966.**

Before starting work on my examination I took time to read a chapter from a book by Scholem recommended by Don, *Major Trends in Jewish Mysticism*. It describes the doctrine of the Merkhaba, the throne of God as mentioned in Genesis and Ezekiel. There used to be a wise rule that forbade the study of these subjects until the student was over thirty,



says Scholem. Merkhaba mysticism is the oldest subject in the Jewish tradition. The dangers of letting consciousness "rise through the palaces of the Throne" are very great, say the cabalists, because Angels and Arch-angels attack the traveller to chase him away, and a great fire comes from inside his own body to devour him.

I think I know what that great internal fire is.

The relevance of these traditions to the study of the UFO phenomenon is obvious: people have always seen similar apparitions in the sky and have always given them religious interpretations. Thus in the Apocalypse of Abraham, the Patriarch is said to have heard "a voice speaking inside the celestial fire with the sound of many waters, the sound of the storming sea." The same loud humming or whirring noise is described elsewhere as "the hymn that the Throne of Glory sings to his King." And Enoch, like Ezekiel, speaks of being taken away from the earth "on the stormy wings of the Shekhinah." This secret is supposed to become universal knowledge "in the next Age."

Later the same day.

For the last week I have been dragging several ills: a persistent cold, the anguish of the examination and a sort of dizziness that may be due to a slight ear infection. I am also working too hard on my second book, trying to do too much. I am upset by our lack of progress with the Air Force. I had proposed that Quintanilla pay for the photocopies Don needs to make in order to preserve the airship clippings of 1897, a mere \$200 or so, but he turned us down. And Hynek has received a letter from Senator Dirksen: "I support your idea of a scientific committee to study UFOs, but don't count on the Defense Department to fund it!" So far the Congressional Hearings in which the believers were putting so much hope have changed nothing: the Committee has simply given the Air Force the go-ahead to implement the recommendations of the O'Brien Panel. After all, it does appear logical to go on with an academic study.

Chicago. Sunday 24 April 1966.

Janine is now reviewing the first two chapters of *Challenge to Science* and I make changes based on her reactions. I am sending a package of UFO magazines to science writer Alexander Kazantsev in Moscow, in exchange for his articles about the Dogons of Japan and the archaeolog-

ical mysteries of Baalbeck. He argues that the Dogon statues wear strange masks that closely resemble astronaut helmets and that the Baalbeck terrace is not only hard to explain in terms of weight, transportation, assembly and precision of the stones, but that it would form an ideal base for the launching or the arrival of spaceships.

The dizziness is gone now, and I feel much better although in moments of weakness I still dream of spending the rest of my life in a little house in France with an old wooden door and shutters on the windows and a good science-fiction library around me. Yet I used to have all that, and I left it behind for very good reasons.

### **Chicago. Monday 25 April 1966.**

The presence of Death is what I suddenly feel this morning, very close to me. The steering wheel vibrates angrily under my burning hands. Something has come howling through my life, out of nowhere. It is like a high-speed train tearing through a herd of cattle lounging on the tracks, scattering bodies and blood. Whatever it was, it has left me inexplicably exhausted. It is barely 9 a.m. but I have had to turn around and come back home. I could not drive to the University in this state of emotional exhaustion. I felt like an old, drunken man.

### **Chicago. Tuesday 26 April 1966.**

I was simply exhausted, physically and mentally. After a good rest things are going better. Tomorrow I will start on the operations research part of my long examination. Hynek has received a letter from Sagan, who is still working with O'Brien. Other investigators are trying to get official support and money for research.

The governor of Florida, together with four newsmen, has seen a UFO from his plane. It seemed to be under "intelligent control," **but** what does that expression really mean?

### **Chicago. Friday 6 May 1966.**

What is happening is even more complex than anything we had imagined. We have received new, totally unexplained reports of sightings by policemen in Ohio and by an aircraft electronics instructor in Oklahoma. The latter involves an elongated object landing on a road and a normal human being next to it, an astonishing report. The cigar-shaped

fuselage took off. It was without wings and without any visible propulsion system. Could that be a secret prototype?

Bill Powers and I have discussed the issue of Hynek's continuing work with Blue Book: should we advise him to resign? I don't have much time to dwell on the recent sightings. My own daily life is consumed by the theory of algorithms, Markov chains and dynamic programming.

Chicago. Saturday 7 May 1966.

The whole day was spent at the Technological Institute typing my answers. I am straining under this pressure. Hynek is different, too. He seems very tired and preoccupied, unable to relax as he did before whenever we talked. I should again point out to him that he is giving too many lectures, taking too many useless trips. Next week, for instance, he is going to cover most of the country again, giving three lectures, including one to the Northwestern Alumni.

"I know that you regard these lectures as a responsibility to inform the public," I already told him last week. "But the events that are taking place demand that you stay here, on the bridge, to steer this ship."

He knows all that, and he acknowledges the advice. Bill Powers is telling him the same thing. But he likes to fly around. He just loves to be on stage, in the spotlight, even if it is only in the lecture hall of some third-rate little college in Nebraska, or in the back room of some Holiday Inn.

At noon I ate a sandwich in an Evanston coffee shop, and I watched the nice students around me. They seemed harmless enough, clean, with no experience of life and passion. I admire many things about the United States, I have acquired huge respect for this country, but there is something deeply hypocritical in American life, and it is most obvious in this quiet, silly academic suburb. I can't blame Hynek for wanting to get away at every chance he gets. Evanston is puritanical and artificial. It clearly separates three communities, keeping the affluent students who drive the Jaguars their wealthy fathers have given them for their 18th birthday apart from the professors, a bunch of dreamers who go to the Campus on their bicycle or on foot, and the elder matrons who drive Lincoln Continentals, own entire neighborhoods and call their attorneys in a huff whenever anyone steps on the gardenias.

Hynek told me about the time when he had to pick up a distinguished lecturer at the airport.

"Would you like to borrow my car?" offered one of his wealthy students, trying to be helpful.

"Why would I do that?" asked Hynek, astonished.

"Well, for your image. You see, your car . . . you know, it looks a little beaten up."

"This man is coming to see me," said Hynek, "he isn't coming to see my car!"

By contrast with these rich, idle zombies, Don Hanlon is quite refreshing. He has a deep, solid intelligence. Yet sometimes I think he has already been hardened too much by life. He is a little too tough, too proud. He used to be a gang leader in the streets of Chicago, before he discovered Jung, the Kabala and mysticism. Is there enough love left in him, enough tolerance towards ordinary folks?

The other day he confided to me that he was somewhat disappointed the first time he met us:

"After reading *Anatomy* I thought such excellent scholarship could only come from an older man," he told me with his good sarcastic smile. "So I was sure the charming young woman with the sexy voice and the French accent who answered your telephone was your daughter. I came to your house all ready to seduce Janine, before I realized you were only a few years older than me, and she was your wife, you lucky guy!"

Chicago. Thursday 12 May 1966.

My examination has been going on for a whole month. I am trying to finish the section on compiler and automata theory. I am immersing myself in the literature of the topic, a mass of articles which contribute to the information explosion while claiming to manage it. Very few of these articles offer any real solutions to practical problems.

Two nights ago we watched a one-hour CBS documentary on UFOs sponsored by IBM, of all people! It is the documentary in which I had refused to participate, preferring to stay behind the scenes. I congratulated myself on this decision. Menzel could be seen pouring benzene over a bath of acetone to explain how mirages caused UFOs. Disgusted, I turned to Janine and said, "Don't forget to let me know next time it rains benzene, so we can go out and watch the saucers!"

Under the pretense of giving equal time to all sides CBS had prominently featured the ridiculous claims of old George Van Tassel and his

Venusian friends. This was followed by Sagan, who made speculative statements about the extremely improbable nature of extraterrestrial visits: he might be willing to accept one trip every ten centuries or so, but not more!

They also had the typical handsome officer with steel-blue eyes who came from NORAD to recite his well-rehearsed lesson: There are no unidentified echoes on the radar screens that protect America! I thought of Hynek's joke about the Air Force being secretly run by a little old lady in Nebraska who owned General Motors and was scared of alien invasions. After the handsome officer came a wise, reliable-looking astronomer from the Smithsonian who said there were no unidentified objects on astronomical photographs, either.

The last two interviews were carefully edited to completely isolate Hynek. Yet he was the only sincere man in the whole bunch, the only scientist genuinely searching for the truth. He could have proven that both the NORAD and the Smithsonian statements were outright lies: we have the pictures and the files to prove it. But the editing of the documentary made dialogue and rebuttal impossible, as usual. His most important statements have been cut out.

The Project Blue Book secretary in Dayton told him in confidence that Quintanilla had spent three days in Washington actually editing the documentary along with the CBS team. That is a very serious revelation. Yet the American public is naive enough to trust such documentaries, especially when they close on a picture of everyone's favorite uncle, Walter Cronkite himself, standing with his back to a blow-up of the magnificent Andromeda nebula.

All this does not make me as angry as it perhaps should, but it saddens me. After all, many of the facts are here, in my filing cabinets, so I don't need CBS or Uncle Walter to tell me who is lying and who is telling the truth. **But the American public is being taken for a ride.** I would like to pass on the real facts to the people who can use them, and I do not know how to do it, other than working as hard as I can on *Challenge to Science*. I hope that at least a few people will know how to read between the lines, and will do their own research once they know where to look.

It is still windy here. The cold and the rain feel good as I walk along the rocks of the shore where waves crash like cataracts.

**Chicago. Sunday 15 May 1966.**

Hynek called me this morning, with much bitterness in his voice. He was the guest speaker at the Midwest astronomers banquet in Madison last night, on the theme: "Flying Saucers I Have Known." He quoted the cases most susceptible to intrigue an audience of scientists. Yet Northwestern students who were in attendance later told him they had heard many negative comments among their colleagues. People were waiting for funny stories of naive witnesses and stupid farmers who confused Capella with a spacecraft and marsh gas with an extraterrestrial mother ship. Instead he described the Mount Stromlo case, in which all the witnesses were astronomers, and the MIT case, and the sighting at Monticello by the two anthropologists we had interviewed in Madison, too close for comfort. And the explanations for these sightings? There were none, he said courageously. The audience didn't like it.

Personally I am skeptical about the possibility of any genuine research in this country even if the Air Force does create a special commission. It seems obvious that such a group will be dominated by skeptical people. I can imagine it conducting very costly statistics like Battelle Memorial Institute did in the days of *Report 14*. They can easily bury the problem in technical jargon while pretending to study it.

**Chicago. Monday 16 May 1966.**

A Northwestern graduate student has overheard a conversation about Hynek among two astronomers in a Tucson restaurant: "With all the money he makes as Air Force consultant, it wouldn't be surprising if he turned out to be the one who has been starting all those silly rumors about flying saucers!"

**Chicago. Tuesday 24 May 1966.**

My written examination is finally over; I have submitted a book of answers 150 pages thick and my Committee has accepted it, clearing me for the oral part and my dissertation itself. Relieved, I wrote to Alexander Kazantsev to confirm I would travel to the Mathematics Congress and to give him the dates of our trip to Moscow, then I drove to the beach to look at Lake Michigan. I sat at the edge of the water and started writing a new article about the patterns behind the landings for the *Fly-*

ing *Saucer Review*. I have read the Valensole file again. I imagined what it would be like to stand with Maurice Masse in his field in Provence. Perhaps I will do it some day. The clouds rushing around the skyscrapers of Chicago and the howling airliners which seemed to hover on their way to O'Hare brought me back to reality. Lake Michigan was very quiet today. Miles away to the North I could see the two white domes of the new observatory.

**Chicago. Wednesday 25 May 1966.**

Harvey Plotnick has just returned from New York. He tells me that the recent success of *Anatomy* has opened up a new genre and triggered a chain reaction among the big publishers. The people who had written off the whole subject as utterly dead have revised their opinion now that they have seen the reviews. Every major New York house has a UFO book on its list of forthcoming titles, Harvey says, to capitalize on the market we have revealed. For example, G.P. Putnam is bringing out a book entitled *Incident at Exeter*, by John Fuller, which is the best of the bunch.

**Chicago. Tuesday 31 May 1966.**

The Memorial Day weekend was spent making more revisions to *Challenge*. I see things so much more clearly, now that I can refer to the French text which is already in print! I have paid a heavy price in time and hard work, for a few clear ideas. Now I start preparing for the oral part of my doctoral examination while listening to some appropriate music, Victorias *Officium Defunctorum*, an extraordinary piece from the Spanish Renacimiento sung *a cappella* by the choral of Pamplona. Just what I need to take my mind far, far above the feuds and the recriminations of conflicting UFO theorists.

**Chicago. Tuesday 7 June 1966.**

Françoise Gauquelin, who is in town for a few days, came over for dinner tonight. She still works tirelessly on her statistics with Michel, and they make steady progress in their study of man's unexplained relationship to the cosmos. Are they rediscovering a forgotten science, which might have given rise to the astrological tradition among primitive peoples who kept the vision but lost the method?

Françoise told us that Aime Michel had gone off permanently to his beloved eagle's nest, a farmhouse in his native village high in the Alps of Savoy. I don't blame him, but his Parisian friends are sad: he will lose contact with all the important trends, they predict. His enemies say: "We told you so, he's out of touch, he just wants to talk to himself." Few people understand his need for communion at a higher level, beyond the petty gossip of Paris, beyond the desire to explain his beautiful vision to the constipated rationalists who are the new arbiters of French thought.

### **Chicago. Wednesday 8 June 1966.**

A major event has happened in the last few days. A friend of Brian O'Brien has launched a bold new campaign that is taking everybody by surprise. His name is James McDonald, forty-five years old, professor of atmospheric physics at the University of Arizona. Having suddenly become interested in the subject, he read many books, including *Anatomy*, and decided to do his own research. Through O'Brien he asked to be authorized to spend two days at Wright Field. He began by requesting to be shown all the cases of "globular lightning." He was amazed and horrified at what he saw: case after case that obviously had nothing at all to do with electrical discharges in the air. So he asked to see more and started reading the general files, getting increasingly upset as he kept on reading.

McDonald moved very fast once he realized, as he told us bluntly, "that the explanations were pure bullshit." So he bypassed the Major and went straight to the General who heads up the Base, to tell him exactly what he thought of Blue Book. After forty-five minutes, which is much longer than Hynek ever spent with the General, they were talking about the humanoid occupants! Then he flew back to Arizona and started contacting all the amateur investigators, one by one, from APRO to NICAP. He made an appointment to see Hynek.

We have just had lunch with McDonald today, and it is clear that an entire era has come to a crashing end. This man has many contacts, many ideas, and he is afraid of nothing.

He reached the campus about 11:30 and Hynek took him on a tour of the observatory. At noon I went to pick them up, and I drove them back to Hynek's office, where we all sat down. McDonald signed the Guest book, and I presented him with a copy of *Phénomènes Insolites*.



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After that the serious business began, with a **forceful** attack against Hynek:

"How could you remain silent so long?"

I jumped in before a fight could erupt.

"If Allen had taken a strong position last year the Air Force would have dropped him as consultant and we wouldn't be here talking about the phenomenon."

McDonald brushed aside *my comment*.

"I'm not talking about last year. It's in 1953 that Allen should have spoken out! Public opinion was ready for a serious scientific study."

"In 1953 I was nothing, a negligible quantity for the Air Force," replied Hynek. "Ruppelt regarded me with considerable misgivings, as a first-class bother. He didn't like to have a scientist looking over his shoulder."

"Yet he says some nice things about you in his book."

"That didn't stop him from playing very close to the vest whenever I was around. He didn't let me see his cards."

The debate remained on that level, with McDonald insisting that Hynek had a duty to say something while Hynek would only concede that he had been "a little timid." Bill and I kept trying to explain to McDonald that any *forceful* statement by Hynek would have thrown him *out of* the inner circle. It could even have precipitated a decision by some General to put the files into the garbage.

Eventually we set aside our differences and the four of us went to lunch. At the restaurant the discussion became more constructive. Hynek retraced in detail the real history of Project Blue Book, truly an incredible tale. Thus he explained how, following Ruppelt's departure, he had seen a succession of unqualified, uninterested officers at the head of Blue Book. He was almost never invited to give an opinion. Hardin neglected his duties completely, he said. He spent all his time following the stock market while waiting for retirement—indeed, today he runs a brokerage office. McDonald was astonished, although he ought to have some experience of how the military runs. I can see how difficult it will be for the public to understand the situation, when the history of this incredible period finally gets written down.

**Chicago. Thursday 9 June 1966.**

Two o'clock, and my oral examination is over. Things had started very badly this morning. I woke up too early and I was seized with nausea. I managed to drive all the way to Evanston. When I called my adviser to tell him I might be late, he said, "If that can make you feel better, you should know your written examination was outstanding, we're all impressed\_\_\_\_" Another professor told me my responses were among the best he had seen at Northwestern, and the oral part went well.

Afterwards Hynek bought me lunch. He assured me the road was clear for my thesis work. Naturally we compared notes about McDonald, and we discovered we had the same impression: extremely positive and enthusiastic at first, then a certain feeling of mistrust towards the man, an uneasy reaction that was hard to define.

**Chicago. Sunday 12 June 1966.**

Tomorrow Hynek goes to Wright Field to meet with the Base Commander, General Cruikshank. He wants to find out just how impressed he was with McDonald's arguments. He has also written to Harold Brown, telling him frankly how he felt about the whole issue. In his answer the Secretary of the Air Force says he has "carefully studied" his ideas: indeed the Air Force will go ahead with university-based investigations, which McDonald wanted to scratch as academic, worthless and irrelevant. The whole project is now in the hands of General James Ferguson in the Pentagon. Hynek and the Dean of the Northwestern Faculty of Sciences have an appointment with him. Other universities are being approached but it seems that Northwestern is the only one with enough guts to look seriously at the problem.

John Fuller is trying to set up a meeting with U Thant, the UN Secretary-General.

**Chicago. Thursday 16 June 1966.**

While Janine and I are beginning to bounce back from the recent turmoil, Hynek seems more preoccupied and tense than ever. The source of his worries is McDonald's abrasive, insulting ways, so diametrically opposed to his own gende and witty personality. I pointed out to him that McDonald's radicalism would in fact make the way smoother for him. He

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is preparing a lecture before the American Optical Society in which he will argue that a serious, sober study is needed. In contrast, McDonald now advocates throwing everything overboard.

Our old black Roadmaster has finally died. The engine was still running but all the other systems were failing one by one, so we had to replace the monster. It is in a brand new Buick, a little blue convertible, that I drive Hynek to the O'Hare bus stop at the Orrington Hotel, and we have coffee together under white and pink parasols, discussing various hopeful scenarios. He and the Dean are scheduled to meet with Pentagon officials to propose the creation of a specialized research center at Northwestern. There is bright, hopeful anticipation in the air.

The Air Force has decided to get rid of the UFO problem once and for all by throwing it *to the scientists*, so the whole issue has become a political one. The Johnson Administration doesn't want any move that could embarrass its Secretary of Defense, Robert McNamara. The Air Force wants to be able to short-circuit all inquiries by referring any sharp question to some sedate university.

### **Chicago. Friday 17 June 1966.**

I am trying to visualize what our research center could do, how it would be organized physically. But I am not sure I want to go on living in Chicago and raising our son here. We do have many acquaintances on Campus, and we are close to a few people with whom we have dinner occasionally, or go to a party or a movie. But apart from these few friends we see nothing but an emotional desert all around us.

### **Chicago. Sunday 19 June 1966.**

Hynek has suddenly become fascinated with psychic surgery. A wealthy industrialist he knows has come back from the Philippines with a color film of "Dr. Tony," who is said to remove tumors and cure cancer by putting his bare hands into the bodies of his patients, only occasionally using a crude instrument like a pen knife or a spoon to scrape off some infected tissue.

Recently Allen announced that he was going to borrow the film to show it to his colleagues at the observatory. So I invited over two top surgeons with whom I am working downtown, namely Dr. Lewis (who invented the technique for blood refrigeration in open-heart operations)

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and one of his senior assistants.

After the movie, which was suitably gory, the astronomers were turning green but the two surgeons seemed delighted.

"Well, what did you see?" I asked them in Hynek's presence.

"We saw two kinds of things," said Dr. Lewis. "First of all we saw some absolutely fascinating primitive surgery, like the time when Tony removes a tumor by scraping it off the back of the eyeball, or when he breaks open a boil on the woman's skin. We are taught in Medical School that's where our own science comes from, but we rarely have a chance to see it done as it was done in the Middle Ages. By the way, we take the same steps Dr. Tony takes, only we perform the operation under anesthetics, with cleaner instruments and sharper knives. But make no mistakes about it, we do basically the same thing!"

"What about the psychic surgery itself?"

"When he was supposed to open the abdomen I think it was sheer sleight of hand. We couldn't recognize any internal organs."

I was impressed with that reaction. He never said, like so many rationalists, "I don't believe it because it's impossible." He simply said he did not recognize the internal organs. Here is the sign of a true scientist, a pragmatic man who deals with facts.

### **Chicago. Tuesday 21 June 1966.**

Hynek returned to Evanston today with some important news. He had gone off to New York to see Fuller and U Thant, who was anxious to know what the United States is going to do about the UFO question, because some of the member nations had expressed *concern*. Hynek assured him that the Air Force had firmly decided to create an independent scientific commission, the only remaining question being to know where it would be located. This could be a step towards the UN setting up their own study, under the Space Committee.

"You know that I am a Buddhist," said U Thant. "We believe there is life throughout the universe."

"Most astronomers would agree with you," replied Hynek. "The question is to know how 'they' would ever come here, given the enormous distances involved."

"Perhaps their lifespan is measured in centuries rather than years. Coming here could be as simple for them as going around the block is

simple for us!"

They went on to discuss the possibility of alien bases on Mars. U Thant also wanted to know about observations made by pilots. Hynek quoted *Anatomy*, adding in his usual cautious manner that such observations "had indeed been reported."

Finally they discussed possible action points. U Thant explained that the initiative could only come from member nations. It is their government which must bring the subject up before the General Assembly.

Hynek has requested my help in this. I will ask Aime Michel if he can set up a meeting with a French government representative, and I will write to Fontes to get him to initiate similar action with Brazilian authorities.

While we were thus talking in Hynek's office a long letter arrived from the Secretary of the Air Force, and he read it to us. Brown said that Hynek's recommendation to turn the problem over to scientists was now getting the highest priority.

We also learned that Lyle Boyd, Menzel's co-author, would be happy to move to Evanston if we were to get the research contract. Finally, a group of unnamed scientists said to be from Wright Field has asked Hynek to supply his "twenty best cases." Who are these people? I advised him not to send them anything until we know for certain where these friendly strangers come from and what kind of hidden agenda they may have.

Chicago. Wednesday 22 **June 1966.**

What I heard today has left me very puzzled. Hynek and the Dean just came back from Washington, where they had conferred with General Giller, who serves on Ferguson's staff. Ferguson himself belongs to Air Force Research and Development. And Giller told them categorically that *under no circumstances would Northwestern get the contract*. The Air force, he patiently explained, is looking for a university that has not had any previous involvement with the problem. Hynek says it is like opening a restaurant and looking for a chef who has not had any previous involvement with cooking!

The real reason is perfectly clear. They are only looking for a rubber stamp, and the last thing they want is the intellectual independence of Hynek's team. There may be a small silver lining here: perhaps the Air

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Force will give us the task of organizing the historical data, while the major contract aims at future cases only. Another relatively positive aspect is that Hynek will now be free to speak out without an "Air Force" label. He will begin by writing a carefully worded preface for *Challenge*. I also recommended that he push harder towards the creation of a UN commission, where he would be the logical leader. And I added:

"Let's see how those who get the award will explain away the UFO Phenomenon. They don't know what they are getting into."

### **Chicago. Thursday 23 June 1966.**

Hynek can't sleep any more, caught as he is between McDonald's vitriolic attacks and the Air Force's desertion. He turned a lot of ideas around in his head last night. Finally he picked up the phone and woke up the Lorenzens to share his distress with them. They told him that Jim McDonald had had a strong interest in UFOs for the last four years. So, why is he pretending to have suddenly "discovered" a scandal? Why has he picked Hynek as his primary target?

Yesterday, at the Pentagon, Hynek told the Air Force that he felt like Moses, who led his people to the Promised Land but was not permitted to enter it. One of Giller's assistants took him aside and told him that the contract would be renewed yearly, so that it might well come back to Northwestern. Hynek would like to believe it, but Bill and I told him he was naive. It is probable that those words were said simply to cushion the blow.

We are going to make a real effort to get the contract for the encoding and classifying of the files. If Northwestern is selected I will abandon my plans to leave the United States and return to France next year.

Under great pretense of secrecy, Winston Markey has "revealed" to Hynek that three men would supervise everything about the scientific program to be run at the university that will eventually be selected. And these three men are Hynek, Menzel and McDonald. We take that with a huge grain of salt, too. This man may be the Air Force chief scientist, but he seems to send Hynek into one blind alley after another.

"Why do you think the Air Force is so anxious to wash their hands of the whole problem?" I asked Allen.

"The Air Force is not a monolith," he answered. "I saw it clearly at the meeting with Giller. There are several groups, each with its own axe to

grind. One of these groups is clearly thinking that UFOs are none of the Air Force's business, and that the problem belongs in an academic research setting. They are the minority. Most of those guys think the phenomenon is just a lot of hogwash, and they are looking for a convenient university to write a negative report and kill the whole business."

"Aime Michel has written to me that he was still in contact with someone from the British Intelligence Service, who told him something similar: the Pentagon wants to wash its hands once and for all of the UFO question, so they will dump it on some university, preferably one that isn't going to cause any trouble."

"There's another scenario," said Don, who had been patiently listening to us. "Maybe all this is just a cover. Maybe Blue Book was a sham from the beginning, and Allen has been used. It is unthinkable that the four of us here should be the only group in the world seriously studying this whole question."

He had just voiced a thought that was in all our minds.

Hynek drew a few puffs of smoke from his pipe and said:

"I have often thought of that."

And yet, knowing how Government bureaucracies do things, it is quite possible that their scientists would be as puzzled as we are, even if the Intelligence services of the major powers (the US, the USSR and the British) are sharing their UFO data, as another contact of Aime Michel keeps telling him.

**Chicago. Sunday 26 June 1966.**

Jim McDonald called me yesterday from Tucson to get more data about power failure cases. We ended up spending an hour on the phone talking about the general situation of the field. He confessed to me that his radical campaign bore little fruit so far. It seems all he has accomplished is to antagonize the Wright Field people. He acknowledged he had not succeeded in convincing Kuiper either. Even his friend Brian O'Brien, with whom he had another meeting last Friday, remains skeptical. One would think he would learn something from this. Yet he continues to claim that the lack of interest in the subject among scientists is all Hynek's fault. He has clearly been indoctrinated by the folks at NICAP, especially Keyhoe and Hall. In a conversation with McDonald, Hall has even insinuated that Hynek didn't really know much about the UFO problem,

and that he had only done research on "five or six cases," which is patently false. Hynek's only interest in the whole thing, Hall told McDonald, is the money he gets from the Air Force! It is not surprising that the field makes no progress, mired as it is in this kind of unfounded rumors, bitter infighting and deliberate calumnies.

Jim tried to recruit me for his camp.

"If it wasn't for your influence, and all the research you brought over from France, Hynek would still be arguing that ninety-nine percent of those reports are due to Venus or to marsh gas!" he said. "It's time for you to move on."

Yet I don't see what good McDonald's approach will do, if he keeps behaving like a bull in a china shop.

### **Chicago. Monday 27 June 1966.**

For the last two weeks the weather has been very hot and muggy, and we feel exhausted, with no energy to work. Chicago bores me. I read a lot (right now, *The Revolt of the Angels*, by Anatole France). I just mailed to Allen the end of the *Challenge* manuscript and a copy of the dust jacket.

### **Chicago. Tuesday 28 June 1966.**

While I was typing the appendices to *Challenge* the phone rang and I heard Allen's joyous voice. He was calling me from Colorado to report on a direct request by Quintanilla: he now wants to have our proposal for data reduction of all the Blue Book files as soon as possible. This would be a parallel effort to the main contract the Air Force has not yet awarded. The Major stated that Northwestern had the highest chances of getting the job, since both Allen and I were there. This would not be a sole source contract, however. At least one other university would be consulted to provide healthy competition.

Allen was elated at this news, and he seemed ready to jump back on the barricade and fight. He was reading the end of *Challenge*, he told me, and he congratulated me: "Reading it, I realize better than ever that the phenomenon embodies an authentic mystery. It shows up in your graphs with the hourly distribution of sightings," he said. We made grandiose plans for the Fall.



**Chicago. Sunday 3 July 1966.**

I have just finished typing the Air Force database proposal. Since Janine had to work this afternoon I went to her office with her to use the Xerox machine there. Everything else is closed because of the Fourth of July. We dropped off the package at the main Post Office, to ensure Allen would get it in spite of the holiday. I hope he will be pleased with my fast reaction. Clearly this project has his undivided attention now. We have had several years to refine our techniques and our arguments, so I am confident he will find few problems with the document I have drafted.

**Chicago. Friday 8 July 1966.**

On Wednesday morning I went to O'Hare with Janine and with the observatory administrator to greet Hynek as he flew in from Denver. We held a meeting in the TWA Ambassadors' Club to review the proposal in detail, far from the ringing telephones of the office or from his busy house in Evanston.

If the history of all this gets written some day, I take pity on those who will try to extract the deeper meaning and the actual motivations of the protagonists. The Air Force wants to get rid of the responsibility for a mystery it obviously can't cope with. If they lean too heavily towards the skeptics' side, turning all UFOs into marsh gas and globular lightning, the witnesses will have good reason to feel insulted, and they may complain to their Congressmen. But if they were to confess the truth, namely that *many* sightings go unexplained, the American public may simply get scared and feel unprotected. This is one battle the Air Force cannot win, especially when the scientific community looks down its august nose at the whole thing and refuses to get its hands dirty. The new concepts that started to evolve in our own minds five or six years ago have not spread among our colleagues yet, not even among the best informed of the ufologists. But sooner or later these new concepts will become the dominant framework. Historians will take them for granted. It will be very hard for those who will look back on this period to understand our hesitations, our painstaking maneuvers.

Hynek received the proposal on Monday. He had only a few changes to suggest, mainly in the budget section. I proposed that we add Lyle Boyd to the team. After all, Boyd wrote Menzel's book and would be an

excellent source of knowledge to balance the staff of our project. Driving back to Evanston we rushed to a typewriter and we had the document polished up in time for Hynek to leave for Dayton. He promised to call as soon as he had anything new.

I was at home and asleep when he called last night about 11 p.m. The Air Force had already started to process our document. There was general confusion in Dayton, because they didn't know yet which university would get the main research contract: "The right hand of the Air Force doesn't know what the left hand is doing," he said, laughing. Our only competitor for the data analysis contract is Battelle Memorial Institute in Columbus, not far from Dayton. Many years ago Battelle compiled *Special Report #14*. It is said that they were paid the sum of \$600,000 for it, a very large amount in 1953, showing that the Air Force was seriously looking for answers at that time.

Hynek did present our database proposal at a 10 a.m. meeting attended by Quintanilla, four Colonels and the Wright Field chief scientist, Dr. Cacciopa. The briefing was well-received, he said: "If it was up to the Dayton people we would already have the contract."

The Air Force is eager to get on with the job. They are getting a little anxious because another one of the universities they contacted has turned them down, and two leading scientists have told them they wouldn't touch the UFO research business with a ten-foot pole, no matter how much they were paid. Hynek keeps over-reacting to the criticisms voiced by McDonald. I have had to advise him against writing a strong UFO manifesto and sending it to *Science* magazine: "Let's at least wait until we have a signed contract," I recommended.

Jim Lorenzen called Hynek during his stay in Boulder. He told him that he and Coral were thinking of dissolving APRO. They are looking for someone to buy the files for fifteen thousand dollars.

### **Chicago. Saturday 9 July 1966.**

I went to the edge of the lake alone, to think. What are we trying to accomplish? I came to the sad conclusion that our efforts are childish and probably hopeless, because this is a very shallow, desperate world that has no vision and takes no chances. *Anatomy* will soon be out in paperback. It will be in every airport and every drugstore in America, from Maine to Oregon, sandwiched between the latest report on teen-age

sexuality and some Western novel of cowboys and Indians. What good will that do? People are closed to new possibilities, to their own mental potential. Even Bill Powers laughs in my face when I try to tell him about the achievements of authentic psychics like Croiset in Holland. The UFO groups are surpassing one another in the silliness of their statements, in the pettiness of their behavior, in an attempt to destroy or to undermine their competitors. This is a joyless, humorless world.

**Chicago. Sunday 10 July 1966.**

Aircraft mechanics have gone on strike, so Hynek is driving back to Chicago from Dayton in a rental car. Don, Bill and I are scheduled to meet with him at the observatory. When I think about the coming year it seems probable that the UFO scene will now be narrowed to two main groups, with our little independent team caught in the middle. On one side will be the university group that will be funded by the Air Force, and on the other side NICAP, which will find a strong supporter in McDonald. I think he has enough ambition to see the UFO problem as a springboard that can send him to the foremost echelon in American science.

The other day he told me on the phone that "Hynek's hesitations demonstrated he wasn't the man of the situation." The implication was that he, Jim McDonald, was the one who should lead ufology to its ultimate victory and that I should rally under his banner. He certainly is a true man of action, capable of organizing a vast campaign, leaving no detail uncovered. He does not have Hynek's subservient attitude towards power, his obsequiousness towards the military. For example, McDonald has clearly seen through Hynek's harmless pleasure at having a Jeep and a driver at his disposal in Michigan. What he fails to recognize in Hynek are the other important and subtle traits in his character: his profound sense of scientific adventure, his kindness, his ability to grasp the hidden side of situations. It is to those abilities that he owes his knowledge. He has understood this research domain deeply, with his heart, while everyone else was using only their brains, and laughing at the witnesses. Where was McDonald all those years:

**Chicago. Saturday 23 July 1966.**

Yesterday I brought back the galley proofs of *Challenge* to Betty McCurnin at Regnery, with some minor corrections. She has done a remarkable job on the book, with care, precision and intelligence. I also gave her Hynek's new foreword. He had written a first draft in which he was awkwardly apologetic, trying again to defend himself against McDonald. There is a French saying, "He who excuses himself accuses himself." I wrote to him in Canada, where he spends a couple of weeks every summer to get away from everything, advising that he take up the problem again from a more authoritative standpoint. Why should he be defensive before McDonald? It is to the public that he owes an explanation. McDonald, who is fast becoming the darling of the ufologists, is only another demagogue.

The observatory has received a letter from an aerospace engineer, the same man who was so secretive when he visited us last year. He is asking us to send him our "twenty best cases," a request already made recently by a mysterious group of scientists from Wright Field. Our files seem to be in high demand, but who are these people? Is this man working for some Intelligence agency? Or simply for the secret study McDonnell-Douglas is rumored to be starting? Does he think we're stupid? Bill has replied on our behalf in an evasive way.

In one week we will be back in France, for a short stay on the way to Russia. It will be good to see Aime Michel again. Should I tell him everything? He advises me to contact Professor Rocard, a physics expert from Ecole Normale Supérieure who has access to De Gaulle. Should I speak to Rocard with complete frankness? Or, on the contrary, should I stay in the shadows, waiting for them to demonstrate that they truly understand the scope of the problem?

**Chicago. Sunday 24 July 1966.**

Bill Powers called me yesterday, relaying a discouraging message from Hynek. The Dean now refuses to sign our research proposal on behalf of Northwestern! After all this work, he is going to turn down the opportunity and kill the project. The Dean shakes in his boots, it seems, because a reporter for the *Saturday Review*, a fellow named Lear, has contacted him with the intention of writing an article about Hynek. He thinks North-

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western will then be exposed as a hotbed of false science. In fact, it is much more likely that the reporter is inspired by McDonald and will criticize Hynek for not doing enough to solve the problem.

The situation is made worse by the fact that no university has agreed to take the main Air Force project, which is still going begging, as McDonald told me on Friday. The academic community refuses to touch the problem, and Northwestern is now aligning itself with other campuses out of fear. Hynek is furious. So is Bill. Hynek talks about suing the *Saturday Review* if Lear's article costs us the contract. He also thinks of taking a one-year leave of absence, founding an independent research company, or even resigning altogether to take this work outside. Bill said courageously that he would follow.

Academic science is a circus. This is not what I want in life. I want to be part of an intelligent, organized effort with people I respect. I don't care to play politics, to convince the masses. I simply want to do things that have not been done before.

Later the same day.

I am finishing the study of the Air Force files for 1951. I find landings and large cigar-shaped objects, just as in Aime Michel's classic work. I am beginning to think like McDonald: how could Hynek have missed this? The wave of late 1951 is astounding. Scientists will be amazed when they finally decide to look at all this.

Chicago. Tuesday 26 **July 1966.**

Hynek is still fuming. He talks of going to another university, to any college that would agree to let him work on this contract. His letter to Northwestern's Dean of Sciences, which was sent yesterday, is well worth preserving:

I fail to understand the University's attitude on the UFO proposal. What wrong could there possibly be in reducing the Air Force data to machine-readable format? (...) They have finally taken my advice and have earmarked better than a quarter million for this—and now my University balks!

I think the University is afraid of its shadow... You will recall that I brought up the whole UFO problem before the trustees at the

Casino Club and asked permission to bring UFO work to Northwestern. Permission was tacitly given. Is Northwestern afraid to do a scientific job that needs doing because of a little possible criticism? Are we a University or an ostrich farm?

Feeling very hopeless in the middle of this debacle I have cleared my desk in anticipation of our trip to Europe. I have put the final touches on my cardiac simulation program. But the respect I once had for this University is gone.

Hynek just called from his cabin on the shore of a little lake near Blind River, Canada. He wanted to wish me a good trip to France and Russia and to finalize our plans. He isn't sure that his letter to the Dean will reverse Northwestern's decision. He is still worried about McDonald, who gives orders at Wright Field as if he owned the Base. Hynek has written an article for the British magazine *Discovery*. He asked me to take several copies of it to Europe, along with his foreword to *Challenge*, to make his position clear to all our friends abroad.

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### **Paris. Friday 29 July 1966.**

The joys of travelling with a group: even after the traditional pictures on the tarmac, where we huddled with other scientists, they still wouldn't allow us to go into the chartered plane. We were exhausted when we finally took our seats. Olivier slept in my arms while I watched the horizon, the arctic light over towering clouds, a fantastic picture of other worlds, like dawn rising on Saturn.

Annick, Janine's sister, was waiting for us at Orly. She took us to Maman's clear, airy new apartment on rue de la Mesange, behind those *Arenes* where I spent some of the best time of my student years. I had been awake for some twenty-six hours when I finally collapsed into bed. Aime Michel came over and woke me up two hours later. We started talking, laughing and arguing. We had dinner at a restaurant near Place Monge. I told him about Hynek's meeting with U Thant, his recom-

mentation for an international effort and our hope that French officials might put the UFO question before the General Assembly. He will try to reach Yves Rocard tomorrow. He is the only French scientist we know who has access to the proper government levels.

**Paris. Saturday 30 July 1966.**

Lunch with Aime, on rue de la Huchette. I told him about our plans to visit the USSR as Hynek's unofficial ambassadors and to find out what was going on there, officially or privately. I have this urge to walk around Paris, to watch a thousand details and register them in my mind. Thus I strolled for three hours, loitering under the sunshine and the rain. And I came to realize what was wrong with Chicago, why I felt so dry and oppressed there, why the human dimension was missing from American streets.

**Paris. Monday 1 August 1966.**

Aime has now read both of our research proposals and the foreword to *Challenge*. He is happily surprised to see how far Hynek's position has changed. A phone call came. Rocard was inviting us for lunch.

We found the professor in his office at Ecole Normale Supérieure, and all of a sudden I was thrown back ten years, even a hundred years. Dusty bookshelves, glass walls along endless dark corridors, creaking wooden floors—Also Rocard himself, perhaps the most powerful physicist in France, cultivating the look of a schoolmaster in a nineteenth-century provincial college. He looks short and gray, with a sad little moustache. But he is a brilliant man under this unassuming appearance; his eyes burn with wit and with the sly caution of bureaucrats:

"In my position, there isn't much I can do, you realize that, don't you? Of course, I do have a few contacts—"

I had to keep reminding myself that I was speaking to someone who invented nuclear bombs for a living.

Craftily, he warned us that he suffered from very bad hearing. Aime Michel took me aside and told me not to believe any of it: French journalists have often mocked "Professor Rocard's carefully simulated deafness," a feature which is convenient in politically difficult circumstances—

He called his chauffeur and asked to be driven to the Marly restaurant, where we made very ambitious plans over lunch: Could Aime be given a

semi-official role, so that he could be our counterpart if our research went ahead in the U.S.? Could we set up a meeting between Hynek and Peyrefitte, who is De Gaulle's current research minister? Nothing was resolved, but I decided to give Rocard the benefit of the doubt, to play with all my cards on the table. I gave him our proposals and our twenty best cases, none of which, of course, contain classified material. Hynek had advised complete openness. Now the ball is in the French camp. We will find out if they have the guts to do something with it.

**Marigny. Tuesday 2 August 1966.**

We arrived in Caen last night, and we drove on to Marigny. Normandy is cold, it rained the whole way. Fortunately the house is full of toys and laughter. In the attic we found a bull's head made of wicker; my nephew Eric wore it while I teased him with Janine's red raincoat for a wild and joyous corrida around the backyard, then I went back to work. I have to mail the galleys of *Challenge* back to Betty in Chicago this week.

**Marigny. Saturday 6 August 1966.**

A curious remark by Rocard comes back to my mind. He told us there were rumors in the administration that the French secret service was going to be reorganized by De Gaulle. He was hoping that would mean that our kind of research, all this forbidden science, "would not be discouraged as strongly in the future." What could he mean by that?

Rocard is in a good position to have experienced both sides of the equation. He directs the physics research for the French nuclear program, but he has also been viciously attacked as a charlatan by his rationalist colleagues because he dared to publish his private experiments on dowsing. This will surely cost him his election to the French Academy of Sciences, according to Aime.

The professor has explained his experiments in a book entitled *Le Signal du Sourcier* (*The Dowser's Signal*) published two years ago by scientific publisher Dunod. It reviews his personal tests of the relationship between dowsers and the earth's magnetism. It also describes some formal physical tests he conducted by getting his students and colleagues to walk through a frame where he could unobtrusively create a current, hence a magnetic field. From these tests he concluded that the human organism could actually detect weak magnetic gradients, and he hypoth-



esized that the detection mechanism had to do with the spin of the proton. Naturally the very fact that he would dare to set up such tests in his laboratory of Ecole Normale Superieure, and that he would publish the results, is anathema for the narrow-minded French rationalists.

This morning another discouraging letter arrived from Hynek, who is still on vacation in Canada. The University's final answer to our database proposal is a definite NO. The Dean is afraid. We have against us many scared, gutless men who have painfully reached positions of apparent power as professors, as directors of various institutes. They don't know what's around them in this dark and scary universe, and they don't want to find out.

I keep taking notes in these diary pages, very religiously, because I know that only those who live through such crises of the mind can record them. In ten or twenty years, who will believe that Hynek's research, the work of his whole life, was blocked by the Dean of his own university?

**Marigny. Sunday 7 August 1966.**

Under the shadow of yesterday's bad news, I am afraid of what we will find when we go back to the U.S., and I dread the stale atmosphere around Evanston.

Is it the rain over Marigny, the dampness that permeates everything? Janine was exhausted when she woke up this morning. She works too hard. She places her hopes in me for our future together. And what can I do? Finish my dissertation, for one thing; drop out of any publicly visible effort on the UFO question; keep it as my private study. Later, next year if we can, let us come back to Europe.

Up in the attic is a grand old nineteenth-century clock that belonged to her grandparents. We will take it back with us to count the hours we still have to spend over there. Although we are in August the weather in Normandy is truly horrible, with rain pouring by the bucketful. This afternoon I will stay home and take apart the old clock. Tomorrow we plan to visit Janine's relatives in the little village of Yvetot-Bocage. And in less than one week we will be landing in Moscow.

**Paris. Thursday 11 August 1966.**

We are back in Paris, which looks sour and nauseous. The French landscape is changing with the new generation. Strange young men have

appeared along the sidewalks of rue Saint Jacques. A kid with unkempt clothes and uncombed hair is typical of the new breed. Stoned to his ears with the fashionable new drugs, a jacket draped over his shoulders, he is sitting in the gutter. He pretends to be reading Kafka.

If I came back to France, I would do it my way, I thought as I ate a sandwich on Place Saint-Michel. I would follow my own course.

Another letter from Hynek, who is still fighting valiantly: our proposal has been submitted to the Air Force after all, but with a "restrictive notice" attached to it by Northwestern. I have never heard of a "restrictive notice." Everything will be decided in September.

### **Moscow. Saturday 13 August 1966.**

It is five o'clock and we have just returned from the Kremlin. Janine has fallen asleep in the next room, exhausted. She is afraid that the bad cold she caught in the rains of Normandy will come back.

For the next three weeks we will live in two tiny rooms on the sixth floor of this huge university building that reminds us of various edifices we have known: the sitting rooms could well come out of an old Texas hotel, the cavernous empty hallways could be borrowed from some dusty museum. But the disgusting meals served on plates of dubious cleanliness have no equivalent anywhere in the world. We each have been allocated an old wooden desk, a narrow iron bed whose mattress has not been exposed to fresh air for years, a table with a top that falls off when you touch it. We are not even allowed the luxury of sleeping together in this place!

We do have a window, however. It looks over the gardens and the avenue which goes down Lenin Hill towards the Moskova River, while the building rises far above us. But the meals are revolting. There is no excuse for such food.

We left Paris yesterday from Le Bourget. As we flew above Brussels the fat Tupolev 114 entered thick clouds and stayed inside the grayness, so that we never had a chance to see much of the countryside. We went through customs as a group, a "Delegation." This gives us some power we would never enjoy as mere individuals. Everything here has been designed with groups in mind. Thus we went from the airport to the university as a unit, a formal *Delegation of Mathematicians*. Janine and I became friendly with a Seattle professor.

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It was unfortunate that we arrived after dinner time. Very calmly, our Intourist group leader told us that everything was closed and that it would in fact be very healthy for us to stay a whole night without food.

This morning we rushed down to breakfast. It was a disappointing experience. After a cup of bad tea and a soggy toast we took the bus for Red Square, the Kremlin and the wonders of Saint Basil cathedral. We have seen nothing of Russian life so far, except for some very decrepit sections of the city. The buildings do not age gracefully as they do in Paris, they just crumble into ugly ruins, as in America. Except that the Americans usually blow up their own buildings with dynamite as soon as they have passed their prime, to make room for bigger ones. There is no such concept here.

The crowd is animated and carefree. One does get a European feeling about the people. They look like a bunch of kind bureaucrats. You almost expect them to be carrying stacks of forms under their arms.

### **Moscow. Sunday 14 August 1966.**

This morning I registered for the Mathematics Congress, which is organized in a very professional manner. The main dining rooms are now open, which helps a little. We are close to Lomonosoff Prospect, where Kazantsev lives in an apartment building we can see from here, just beyond the computation center.

We have just walked around the grounds with our new guide, a delicate and sensitive Russian girl named Galia. She is a physics student who wants to become a teacher, and serves as a hostess at such international meetings during the holidays. Galia speaks English fluently. Unfortunately she does not know this university very well. She finds it ugly, too gigantic. She works in Moscow proper, and speaks eloquently of the beauty, the poetry, the proportions of the old city. I found my way to the observatory, the Chternberg Institute where Shklovskii works, but this is Sunday and we cannot visit the place.

### **Moscow. Monday 15 August 1966.**

Noon. We spent the morning at the Chternberg Institute, where a friendly astronomer answered our questions through an interpreter. He showed us the time service, the solar department and the computing

section where we saw our first Soviet electronic machines. They look absolutely identical to IBM hardware of ten years ago. We visited the small library and signed the guest book, where the name of Bertola and a few other Americans had already been recorded. Finally we saw the domes, which are inoperative, as in too many observatories I have known since Meudon. The building reminded me of some French provincial place, sleepy, solemn and dusty.

### **Later the same day.**

We took the subway to Kropotkinskaya and walked around the old streets of Moscow with Galia. We visited the former aristocratic section which contains the houses of Pushkin and Tolstoy. We came back by subway again. Janine still feels very tired. Everywhere we find the same impression of decay and odd emptiness, but I may have understood the reason for it: this city is built in anticipation of the winter. These big green and yellow walls which seem so incongruous and inhospitable in the August sun must be admirable under the snow.

### **Moscow. Tuesday 16 August 1966.**

The official opening of our Congress takes place this afternoon. Several groups of mathematicians have arrived at the last minute. Notable among them are the highly visible French. They came here in their noisy Citroens, as if pushed by an evil Western wind. They brought wives and families, who yell at the crying kids, leave greasy papers on the marble stairs erected by the Proletariat, beer bottles at the feet of Karl Marx and candy wrappers around Lomonosoff. And just to make certain everybody will know which country is responsible for these improvements to the landscape of their hosts, they proudly display big badges that spell FRANCE on their jackets.

We arrived at the Palace of Congresses in time for the inauguration, and watched with amusement the curious spectacle of contemporary mathematics. The Russians fell into each other's arms with obvious glee. The French paraded around, posturing with strange rituals and mannerisms. The British seemed distant and disdainful. The Japanese were dignified and anonymous. In the grand amphitheater we heard a speech by the Soviet Minister of Automation, followed by the Fields medal ceremony, the mathematical equivalent of the Nobel Prize.

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During the long intermission we walked inside the Kremlin. We were astonished at the elegance of the domes which seemed so heavy in the photographs we had seen. Up close they give a remarkable impression of clever use of space and volume. We went back to the conference center in time for the unavoidable *Swan Lake* ballet. When we came out again we saw the stars: The red stars atop the bulbous towers and also the little stars sparkling far away in the sky, the stars that are still beyond politics, beyond the reach of man.

### **Moscow. Wednesday 17 August 1966.**

This morning I attended Ativah's lecture on elliptical differential operators, which went over my head, and Meltzer's lecture on automated inference, which I found of special interest since it relates to my current work. Meltzer is from Edinburgh, where a major center of artificial intelligence research is starting to flourish.

In the afternoon I skipped the meetings to walk along the Moskova with Janine, right under the Kremlin walls. We sat in the park and we reviewed our problems: Northwestern, the Dean, my dissertation, Janine's career in business data processing, the future of my books. I found this assessment rather discouraging. It is true that the sight of Moscow does not inspire one to daring solutions. The heavy architecture, with the awkward columns endlessly stuck over dark greenish masses of masonry, seemed designed to enhance our melancholy.

It is hard to characterize the crowd. The way young people dress, they could blend into any group in Paris or Chicago. But the older generation look as if they had been stuck in a closet for twenty years: the men wear wrinkled suits, the women shawls and shapeless dresses. In the few contacts we have had people seemed to show a depressing lack of personal initiative.

The Marxists are always talking about the unresolved "contradictions" of the Western countries; but one of the many unresolved contradictions of socialist States has to do with their whole approach to human problems. Why do they feel they must make final decisions today on moral, ethical or esthetic issues that rightly belong to future generations? As soon as the "progressivist" is free to act on his ideas it seems that he must turn into a dictator and issue absolute edicts, unaware that in doing so he denies the progress of the very society he is trying to build,

since he closes off its future choices. In the Soviet Union these progressivist bureaucrats have arbitrarily decreed what constitutes beauty, morality and intellectual worth. They have crippled the very soul of their own people.

**Moscow. Thursday 18 August 1966.**

At nine this morning there was a very interesting lecture by Lotfi Zadeh, a young mathematician from Berkeley. He was introduced by Kolmogoroff, who pointed out that this was the first instance of a lecture on control theory at an international mathematics congress, and warmly recommended this new domain to the attention of the more "traditional" specialists.

Zadeh presented himself humbly as a mere electrical engineer. He criticized current optimization theory as too narrow and simplistic; it fails to address the real problems, he said. For example, criteria functions are generally expressed as single values rather than vectors (multiple values). Too often, when we are asked to study a complete system we can only build a theory of a small sub-system of it.

He gave economic plans as an example. Anybody can define the ultimate objectives of a five-year plan, he said. The true challenge is to define realistic local goals on the way to the big result. To be more specific: it is easy to decide that tractor production shall double in five years. The real question is, how many tractor drivers should be trained each quarter? How many tires will be in use by the time 50% of the plan has been implemented? How much should be invested every month? This is far from being a classical problem.

Another real-world concept discussed by Zadeh is that of "fuzzy sets." In mathematics it has always been understood that sets had a rigorous definition, but in practice that is rarely the case. A physicist will often talk of quantities whose value is "close to ten" or "much larger than one." Such fuzzy definitions will be a key to the understanding of artificial intelligence in years to come.

In the afternoon we strolled along Lomonosoff Prospect and we found Kazantsev's apartment building. With Galia, who is witty and charming, we enjoyed a long visit to the old Moscow University. Later, leaving the campus, we followed an avenue that led to the Novodevitchi convent, a beautiful structure with elegant bulbs and lofty spires rising serene-

ly above the trees. Fresh flower gardens led to a dark crypt where the floor was paved with sculpted tombs. In the large chapel tourists walked around while old women prayed in the flickering candle light. A wrinkled crone travelled along the walls like a fast-moving spider, crossing herself quickly and kissing each icon. Galia explained to us that a funeral had just been celebrated by the priest. This woman was praying for the soul of the departed. As a young Soviet girl she expressed her revulsion before such superstitious behavior.

A group of crying women suddenly rushed into the chapel, filling it with their lamentations. We retreated to a larger room where something like a lying statue was resting. It took us a while to realize this figure was in fact the dead man himself, his corpse embalmed, its skin shiny as wax. Galia's face turned purple as she saw it. She rushed out of the church as fast as she could. When we caught up with her she was still very shaken and we had to calm her down. Her encounter with the old religion had caught her unprepared.

### **Moscow. Friday 19 August 1966.**

I attended a very exciting lecture this morning by Richard Bellman of the Rand Corporation. The room was filled to capacity, because his work is well-known here. He gave an overview of operations research, emphasizing dynamic programming techniques. He went on to talk about information processing, stressing the problems of delay and adaptation.

### **Later the same day.**

As we were watching the night that fell over the meandering river a young Russian man approached us with an offer to buy foreign currency at twice the official rate. One of the ways for us to land quickly in a Soviet jail is to accept such offers, so we told him that we had nothing but rubles.

Another young man who had witnessed the scene then spoke to us. He said he was a graduate student in applied mathematics, who had few chances of practicing his English. (In fact, he spoke quite well. The level of language education here is remarkable. Galia speaks with very little accent, in a way which is full of precious little phrases. For example she will start every other sentence with "As a rule ...") When I asked him if he used computers in his work he opened his briefcase and showed me a

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program he was writing. It was assembly code. He knew of high-level languages like Fortran but they were not *yet* in general use, he said.

The sun was setting, the air was turning colder; he was preparing to leave. Instead we invited him to have dinner with us, much to his surprise. During the meal we learned some new things about current conditions of scientific research in the Soviet Union. As in the West, he told us with some bitterness, fundamental researchers are paid far less than their industrial counterparts. It is only when they do something sensational that they are given a little money or an increased budget. This may explain some of Shklovskii's *surprising statements* about his beliefs that intelligence can already be detected in the universe. Generally speaking (or, as Galia would say, "As a rule"), the same bureaucratic rigidity is found here as in France. There is a young mathematician named Arnold who is so legendary among his peers that he has been proposed for membership in the Academy, but he was rejected because of his young age and independent spirit.

Our guest seems to be a good specialist who ignores completely what he calls "non-scientific questions." Thus he regards science-fiction literature as useless dreaming, and he says he leaves politics to the politicians. Also he doesn't see how there could ever be controversies in science since all the methods of science are exact by definition. He must have missed Lotfi Zadeh's lecture about fuzzy sets! I think such a dogmatic attitude reflects immaturity in Soviet science education.

We are struck by the lack of enthusiasm among the young. When we talk about the space program their apathy is obvious; it borders on contempt. Shklovskii *seems* to be practically unknown among the people we meet, while he is a big man in the Western media. There is a sharp division between the "literary" and the "scientific" minds, which I find disappointing.

### **Moscow. Saturday 20 August 1966.**

We still have ten days to spend here, and time is becoming heavy. Today we continued to play tourists, visiting the Lenin mausoleum, the tombs of revolutionary figures, the throne of Peter the Great, and Catherine's opulent carriages. Once again we returned exhausted. I went to bed and read Hoffman's *Fantastic Tales*.

This was a book that strongly impressed my father, perhaps because he



and my mother had enjoyed the opera drawn from it. He had owned a copy that burned with his whole library in 1940. When I was a teen-ager I saved some money in secret and bought another copy for him on his birthday. I had some difficulty with the bookstore owner, who told me "It isn't a book for young people." I never read the book, and I imagined it contained all sorts of delightful horrors. Now that I have a chance to read it, I find nothing but a series of good old nineteenth-century stories, a lot of dark hints about "magnetism" and some distant fumes of sorcery, diluted in well-behaved rationalism and wilted poetic notions. I fail to find anything to justify my father's excitement or the bookstore owner's trepidation. After all, when I bought it as a teen-ager I had already read Poe and more sulphurous writers, not to mention the truly grotesque and gory depravities described in Sophocles and Homer, which we were translating daily in Greek class, word for horrible word, dripping with blood.

**Moscow. Sunday 21 August 1966.**

A leisurely excursion took us to the Moskova-Volga canal today. The boat stopped at the edge of a forest. We ate bread and cheese, and we walked happily through the woods. This was a welcome change of pace. All day yesterday I had been in a dark and silent mood as I finished reading Ruppelt's book, *Report on Unidentified Flying Objects*.

When I compare Ruppelt's situation with ours I can imagine how broken he must have been when he left Project Blue Book, and I despair of ever seeing the problem posed in honest, realistic terms. Even after an expert committee recommended that the research budget be quadrupled, the Air Force did not follow through. On the contrary, they killed Ruppelt's excellent proposal to install cameras on radar antennas to establish once and for all if anomalous echoes corresponded to some real objects. He also wanted to study the correlation between saucer sightings and sudden jumps in atmospheric radiation levels, and he was never allowed to do this.

I had not suspected that so much had been done in the U.S. during the fifties. Ruppelt's ultimate failure is discouraging. Hynek is the only one who has not failed miserably, perhaps because he has remained in the shadows, "biding his time," as he often says, and observing events quietly as a spy inside Blue Book. But what will happen if he tries to go public

with our ideas? His colleagues are already placing banana peels under his every step.

Here in Moscow the charm of a few initial discoveries, the friendliness of some contacts, has faded quickly. Given the global nature of the UFO phenomenon one would have to be completely stupid not to try to find out how it is manifested and perceived in the USSR, which is one of the largest countries of the earth. But our first feeling of discovery and adventure on this trip has been replaced by an impression of bitterness, of stifling oppressiveness. In spite of what little Galia claims, the city seems very dull to me: nothing happens. We sleep badly. Something is very wrong here, there is a sickness of life, an immense sorrow in the air.

Moscow. Wednesday 24 August 1966.

Another agitated night. I was worried with incoherent dreams on this camp bed where I can barely turn around. We have accomplished everything we could accomplish here, so we are now trying to arrange to leave the Soviet Union a week earlier than scheduled. The bureaucrats of Aeroflot are not helping us. The walls seem to be closing in on us. We feel trapped.

We succeeded in visiting the main computation center of Moscow University with a small group of interested mathematicians today. It wasn't easy. We experienced an initial rebuff when a stern-looking official dressed in gray declined our request with simple words which required no translation: "Machini? Niet!" Thanks to some energetic Russian students our group was able to get around the bureaucracy. We saw two machines, the M20 and its transistorized version the BSM4. Soviet engineers were willing to answer all our questions but as we began to realize how far behind they were the dialogue became awkward, even embarrassing.

The real surprise came for me when I pulled out a small deck of cards I had brought in my suitcase, a program for stellar motion calculation written in Algol. The Soviet specialists passed the cards among themselves in astonishment. I explained to them what the program did. They shook their heads, confused.

"Don't you have a compiler for the Algol language?" I asked.

"Yes, yes, it is a standard language, of course."

"Then we should be able to run my program on your machine."

"No, that is not possible."

"Why not?"

The man shrugged, a helpless look on his face.

"Because we do not punch the information in our cards the way you do."

"But your cards are exactly the same size and the same thickness as ours!"

"That does not matter, we do not write them in the same way."

They proceeded to explain how they had "improved" on the simple IBM concept of keypunching. They pack the information into the cards column by column, instead of dedicating one column to a single character. Thus they can put three times more data per card. Unfortunately this makes it impossible to read foreign cards. It also prevents them from printing out the contents of their own cards along the top edge as we do, so they never know what is on each card unless they make a special note of it by hand, which of course is very time-consuming! They were astonished by the pin printer pattern used by IBM. They have never built such a machine. So much for the international exchange of "standard" scientific programs!

The State has a monopoly on ideas here, and it channels carefully selected thoughts, one at a time, into popular consciousness. Open discussion of the UFO subject is clearly discouraged. Alexander Kazantsev, who is a recognized writer and engineer, is the exception that confirms the rule, because he has been smart enough to treat the question in a science-fiction vein, or in the context of archaeology. Shklovskii is an exception too, because he plays a role similar to Sagan, who keeps proposing gratuitous speculation without looking too closely at the data. If the saucers turn out to be significant, Sagan will take credit for having daringly theorized about cosmic visitors. If they are discredited he will claim he always saw clearly through their mythical character. Shklovskii has made sensational statements about the periodic signals received from source CTA-102 which he says may indicate an intelligent extraterrestrial source.

We need to grasp once and for all how alone we are in our research. This trip to the end of Europe has convinced me of it. Reading Rupert confirms my feeling that a deep and weird drama is unfolding around us. But what can I do with this knowledge? I am not a missionary at heart like the brash Jim McDonald. I do not intend to mount any crusade.

Later **the** same **day**.

Galia has insisted on showing us the Moskova by boat. She was right about the beauty of the trip. It is from the river that one must see the Kremlin, as it rises above the grayish haze. It is a great sight indeed, with a single red flag at the very top. This earth is a poor little planet, subjected to the whims of power-hungry men.

Moscow. **Thursday 25 August 1966.**

It is raining today and our charming guide has a cold. In spite of it Galia came over and insisted on taking us to the bookstores on Gorki street. I found two short stories by Kazantsev in an anthology of his works entitled *A Space Visitor*. One of these recounts the 1908 Siberian catastrophe when an unknown cosmic object impacted the taiga. I looked in vain for Menzel's book, which we were told was the only UFO book published in Russian. This led me to explain to Galia how we had become interested in this topic. In spite of the official censorship she was perfectly well aware of its reality. They even have something similar to the American *contactee movement*. Some members of the Academy have received letters from individuals claiming to be in touch with an outer space source, and even receiving psychic orders from it. They assume that they are mentally disturbed. However, according to Galia, a Soviet criminologist has analyzed these letters and came to the conclusion that a genuine new sociological pattern was emerging, based on some authentic, unexplained observations.

While the phenomenon is often tainted with religiosity in the U.S., here it takes a more extreme turn. Thus a crazy woman has killed a great Jewish Soviet plasma physicist, by throwing him under a subway car. She had heard a voice from space tell her that the Jews must die because their existence on earth is very miserable, and they will be much happier in the next world.

"And the Academician who has centralized these letters, was he acting officially?"

"No, people just wrote to him."

"Is there a government body that conducts investigations on these cases?"

She shook her head energetically: "Not to my knowledge."

"If something strange passed in the sky what would you do, Galia?"

"I would watch it pass in the sky."

"Would you tell the authorities?"

"Whom could I tell? As a rule there is no place to report something like this."

What she is really saying is that anyone reporting this would be suspect as a source of potentially harmful rumors. Spreading unfounded rumors is a crime in the Soviet Union, which explains why we have been very selective in our conversations on the subject, some of which I do not want to record here even in these private notes. There is a thin line between exchanging scientific information with people and "spreading rumors," and we are not always sure how the line will be interpreted.

This afternoon we went back to the Intourist office with Galia, who used her official clout and barked orders at them as if they were a bunch of ignorant, despicable moujiks. They snapped out of their lethargy and immediately found us two seats in the first Tupolev that leaves for Paris in the morning.

Sadly, we had tea with Galia for the last time. We told her we hoped that some day she would be able to come and visit us in the West.

### **Paris. Sunday 28 August 1966.**

We thought our early return would be a private surprise for Maman, but instead we found a whole family reunion in her apartment. My brother Gabriel was there with his wife and kids. It was Eric's sixteenth birthday. A distant American cousin on her way back from Lebanon was also there with her two children.

I have been walking around Paris with almost mystical pleasure. I bought more old books, from Ovid's *Art of Love* to a history of religions dating from 1666. We are struck by one detail: nobody has asked us any questions about our trip to Moscow. The French people we know are intellectuals who already have all the answers about everything. They read newspapers like *Le Monde* that provide them with complete and definitive knowledge. What would they do with the testimony of first-hand observers like us? The actual reality of things is of no consequence to them. What counts is to joke sarcastically about current events. Elegant cynicism is the fashionable attitude, not informed opinion. The French wit attacks and contradicts, using brilliant paradox to demonstrate that the

world's evils are grotesque and that mankind's sorrows are irrelevant, as long as the little cafes along Boulevard Saint-Germain don't run out of croissants.

I have made some progress in my research on the humanoids. A book of grimoires by Paracelsus (born 1491, fifty years after the invention of the printing press) contains precious definitions of what he calls Elemental Beings, who seem strangely related to our modern UFO operators. I want to dig deeper into this, and that means I need to find more texts by the Hermeticists, as these texts existed before they were adulterated by occult amateurs. Once I find these documents I will have to investigate the parallels with recent phenomena, a long-term effort.

Paracelsus writes that there are four orders of spiritual entities, namely Nymphs, Pygmies, Sylphs and Salamanders. He notes, however, that these names were attributed to them "by people who had not spoken to them." To be completely accurate one would have to include giants and other orders of beings, such as mermaids and sylphs. These beings are seldom seen, but their existence, he says elegantly, is beyond doubt:

Myself, I have only seen them in a sort of dream (...) These beings appear, not to remain among us or to ally themselves with us, but in order for us to understand them. These apparitions are rare. But why shouldn't they be? Is it not enough for one of us to see one angel, in order for all of us to believe in angels?

Modern science, of course, would answer an emphatic "no" to this last question. Elsewhere he writes about the Gnomes, whom he also calls "Homunculi":

They can appear at will small or tall, handsome or ugly. They know all the arts and use the light of nature. Some who saw them thought they were the souls of men who died shamefully or by suicide. Others thought they were empty spectres, some took them as manifestations of sorcery. **Think twice before becoming allied with them. As soon as you are linked to them you have to do their bidding. When they are angry they inflict heavy penalties. Sometimes they kill. There are proofs of it** (my emphasis).

Unfortunately he did not give the details of these cases. It is known, however, that he did extensive research among the miners of Germany,

who often reported seeing dwarfish humanoids in the underground tunnels they dug in search of coal.

**Paris. Monday 29 August 1966.**

Two depressing letters have arrived, one from Hynek's secretary, the other from Hynek himself, mailed from Atlanta airport on his way to Dallas. The Pentagon has rejected our proposal once and for all. This solves our problem. Clearly the Air Force does *not* want to see any serious study of its files carried out. What are they afraid we would find?

Hynek's letter to *Science* magazine has also been rejected. In the words of Joanne Belk, editorial assistant:

Dr. Abelson has given *me your* letter of 1 August about UFO's (sic). He found it very readable and would normally have considered it for publication had not the *Saturday Review* devoted a large part of its 6 August issue to the same subject—You will agree with him, I know, that we could not follow the *Saturday Review* articles so closely as to run the risk of echoing their feature.

That is the most ridiculous turndown letter I have ever seen. Since when does a scientific magazine have to consider what others have published in the popular press before it decides what its readers should see? Furious, Hynek wrote directly to Abelson: "There has been a spate of books and articles, and there will be more. Vallee's *Challenge to Science* will soon be out, Fuller of the *Saturday Review* is shortly to have another . . . the existence of other articles might be construed as good reason to publish my letter now, rather than to wait," he pointed out, adding with considerable restraint:

I had hoped, and still hope, that the pages of *Science* could carry a sober status report on the UFO phenomenon amidst the barrage of articles written by people who are journalists and who simply do not have access to the data, or if they do, could not possibly have had the time to do justice to it.

For good measure he has leaked his *Science* letter to a number of prominent people, including Dick Lewis, who is publishing extracts of it in the *Sun-Times* as "The letter *Science* magazine was afraid to print . . ."

**Paris. Tuesday 30 August 1966.**

We walk along the old streets, breathing the dusty haze that rises from ancient books. I immerse myself in humanity, in the million fragments of moving life, colors and melodies of Paris. When we travelled to the Soviet Union our first impression was one of strange peace, the delight of being liberated from the ever-present bombardment by advertising on radio and in the streets, from the intrusive billboards inciting you to buy various products. Yet when we came back the sensory stimulation of advertising was a welcome joy. I was surprised to find that after the first period of blissful peace I had actually missed it.

Janine has left for Normandy. I am without my tender, lovely brunette, and I cannot stand this separation even for a short time. I miss her conversation. All day I find myself talking to her about what I see, what I think. I miss her pretty face, her body. What would my life have been without her? She has brought me happiness, untold wealth.

**Paris. Wednesday 31 August 1966.**

This afternoon I paid a quiet fraternal visit to Serge Hutin in his mysterious house of Fontenay-aux-Roses. Even though I have long dropped out of the formal Rosicrucian Order (I feel that far better sources are available to me now) we are still very close in spirit. I had not had time to write, and he has no telephone, but I trusted that "the Cosmic" would take charge of our meeting, as he would say himself. Indeed, as soon as I rang at the iron gate his mother appeared in the alley and she asked me to come in. I saw the abandoned garden, the dark old workshop at the end of the yard, and the numerous cats they have collected.

The great scholar was there, to answer my questions about Hermeticists and Homunculi, Hildegard of Bingen and her visions, and about current trends in the occult underworld. He works in two small rooms, one of which serves as his Sanctum, where he is surrounded with cats, art objects, occult artifacts, engravings and paintings. In this environment I regress easily to former times, to my childhood itself, with the wind blowing in tree leaves, the ancient touch of the stones of ruined castles. It is the old house that speaks to me of forgotten eras, it is a dark passage-way beckoning me to look back through the dust of memories.

I spent four hours in Hutin's Sanctum. Afterwards he walked with



me to the bus stop and waved at me when I climbed aboard the 194, as if I had been a cosmic traveller leaving for another galaxy. He is a man with a great heart and profound knowledge. I could almost hear the old house groaning, lamenting my ungrateful departure.

It is midnight. There is an empty place next to me in the bed. Janine is not here and I miss her sweetness. I think of a thousand things I would like to tell her.

**Paris. Thursday 1 September 1966.**

My mother just informed me of U Thant's resignation from the United Nations. De Gaulle is in Cambodia, openly criticizing American policy in Vietnam.

This afternoon I indulged in another long walk through Paris, buying astronomy and folklore books, and Enel's treatise on the origins of Genesis and temple teachings in Egypt. I also read an article by Seguin stating that among 500 popular tracts printed between 1529 and 1631, no less than 180 related "marvellous phenomena" of which ninety-five were celestial events. All that begs to be dug up and studied again.

I am angry at Aime Michel, who has just published a review of our book in *Planète* under his pen name Stephane Arnaud. He used this opportunity to get back at Muller and at Kourganoff. Why is he hiding behind me to attack them? I have no quarrels with Kourganoff, a man I admire very much. And I have forgiven Muller, whom I do not expect to ever see again on this earth.

Janine comes back tomorrow with my little boy. On Saturday we will visit an art show at *Saint-Germain-des-Prés* where my mother displays four paintings. On Monday we fly back to Chicago.

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**Chicago. Wednesday 28 September 1966.**

Autumn has returned, and I work full-time again at Dearborn Observatory. Nearly a month has passed. With the rejection of our UFO proposal I found a depressing situation when I returned. I haven't felt like

writing. My current project is the reduction of photometric observations of stars recorded at Kitt Peak. Fortunately the computation center has a new director named Ben Mittman who has an open mind on frontier subjects. I have introduced him to Hynek.

There have been two other notable changes. Our legendary Sergeant Moody is no longer at Blue Book. A new man, Lieutenant Marley, has taken his place. He has just spent three days in Evanston. Hynek's letter to *Science* will finally be published. Small consolation indeed.

It is hard for me to shake the melancholy that took me when we came back here. Don Hanlon, whose humor and intelligence were so refreshing, and who would have made an excellent research assistant, enlisted in the Navy when it became clear that our proposal was dead. It will be hard to replace his wit, his knowledge and his friendship. And McDonald is still arousing the media, turning the same humorless crank, endlessly accusing Hynek of every crime. I am glad I have scrupulously kept this diary. It may preserve a record of the truth as we lived it.

A French newspaper someone had left in the plane that brought us home contained a very curious report: two young electronic technicians have died on a hilltop near Rio de Janeiro while waiting for a flying saucer they claimed to have contacted. The case is becoming known as "The Lead Mask Mystery" because the corpses had crude masks over their eyes. Inspector Jose Venancio Bittencourt, a veteran detective of the Rio police, is quoted as saying it is the strangest mystery in his career.

### **Chicago. Friday 30 September 1966.**

My thesis committee met today to hear my defense of my dissertation project. They gave me the green light. Mittman said I had attacked a very tough problem but he felt my approach was correct.

When I drove downtown to bring a collection of UFO magazines to the binder, I found the city as I had left it: with a big hollow feeling.

Hynek and I have entered into a secret pact after I pointed out to him that we were doing more and more superficial things and less and less actual research. All our time is spent with journalists, curiosity seekers, tourists, believers armed with a thousand pet theories we have all heard before.

"Allen," I finally said, "this kind of activity will never lead us to a scientific solution, and you know it."

"All right, Jacques. Let's say that by Christmas, either we will have obtained a radical change, or we will go back underground. Let other people worry about the problem."

"Yes! Let's see what McDonald and NICAP will do to solve this mystery, since they are so damn smart!"

**Chicago. Sunday 2 October 1966.**

Now Aime is urging me to return to France: "Get your doctorate, come back here to work in the computer business."

It turns out De Gaulle has finally discovered the importance of computers and has launched a great new project he calls the *Plan Calcul*. He is bitter at Johnson's refusal to sell him a CDC 6600 with which to compute his H bomb. He now wants to regroup the French computer industry around national objectives.<sup>1</sup> Economically that is not a bad idea. The problem is that the plan will surely be managed, once again, by mediocre and greedy men. Chances are it will fall under the weight of their petty machinations for control. Such grandiose schemes always do. The money will probably be frittered away in the form of subsidies to the big, inefficient electronics industry.

If I do go back to France I will simply be another pawn in this game. In my discussions with Rocard I understood this: he reached his position through a combination of scientific excellence and skills at dealing with bureaucrats and ministers. Yet that does not give him any real power. When France explodes an atom bomb in the South Pacific he watches the proceedings from the bridge of the flagship cruiser. But he is a great man in a little world, a prisoner of a mediocre system, like Costa de Beauregard who studies the physics of consciousness, and Remi Chauvin, a zoologist and parapsychologist who teaches insect sociology at the Sorbonne. None of them is allowed to undertake the research they would really like to conduct.

**Chicago. Thursday 6 October 1966.**

Ben Mittman has offered me a full-time job in the systems group, and I have decided to accept it. I am not doing very much UFO research at the moment, other than discussing the subject with Bill Powers. We have lunch together almost every day. Actually we spend more time talking about psychology than about flying saucers. I am fascinated with Bill's

novel ideas on control theory and on behavior as the control of perception, which he wants to expand into a book.<sup>2</sup> He reads me entire chapters of his manuscript at the cafeteria, and I give him my reactions and critiques.

Hynek has just told me that tomorrow the Air Force is supposed to announce the name of the university it has selected for the UFO contract. He expects it will be the University of Colorado in Boulder. Everybody else has turned down the job with contempt, including Harvard, Columbia, MIT, the University of North Carolina and the University of California.

**Chicago. Friday 7 October 1966.**

Allen came *to* open the door himself, in *his* robe and slippers. He was surprised to see me. I handed out to him the first copy of *Challenge to Science*.

"I brought you something to read," I said. "Have a nice weekend!"

"Come in for a minute," he proposed.

But dinner was already on the table, Janine and Olivier were waiting for me, so I *left* quickly.

Several memorable things happened today. Our joint article for the Astronomical Society of the Pacific has just appeared, linking Allen's name and mine in mainstream astronomy as they have been linked in UFO research. We had lunch at the Red Knight Inn, a funny old place which has become a sort of headquarters for us. There was a new member in our little group, a man named Fred Beckman who is an electroencephalography specialist at the University of Chicago Argonne Cancer Research Laboratory. He is also a great connoisseur of food and wine.

"I *don't* suppose you come here for the cuisine," he said as he looked disdainfully around the restaurant, observing the mediocre fare on neighboring tables and the stern matron who was our waitress.

"They do have some unusual dishes," I said. "Look at the menu. It even offers *Chicken Consumé*."

"They must mean *Consommé*? Chicken soup? What on earth is *Chicken Consume*?"

"It literally means 'burned-out chicken.' Haven't you tried it?"

He laughed *good-heartedly*:

"I am afraid I have had it many times. Generally in places just like this!"

Fred Beckman is a witty man with an acerbic contempt for most of humanity, with an exception for orchestra conductors. He has a deep passion for complex, exquisite things: Oriental carpets, Chinese gongs, Mahler symphonies. He has read *Anatomy*, after finding a notice about it in *True* magazine, of all places! He told me it was the first book on the subject which he did not throw away after a few pages.

As Hynek expected, the Air Force has officially announced the award of a \$300,000 research contract to a team at the University of Colorado led by Dr. Edward U. Condon, a nuclear physicist of established reputation.<sup>3</sup> Hynek and I will be invited to Boulder next month to help define their strategy. I suggested a structured seminar format rather than an informal discussion which would be a waste of time.

In the midst of all this Jim McDonald is trying hard to recapture the attention of the media, which is slipping away from him. He is now telling the press what he secretly believed all along: Flying saucers are extraterrestrial spacecraft. Big deal. He does not have any evidence to support his statement, so he has very little impact except among the small circle of the ufologists, who were already convinced. In the scientific world he carries far less weight now than he did last Spring. Besides, the gossip in Arizona academic circles is that his wife doesn't believe in UFOs.

The whole day has been magnificent. Dead leaves falling from the trees that line the avenues landed with crackling sounds inside my little convertible. The sky was full of stars and of life.

### **Chicago. Monday 17 October 1966.**

Jim McDonald was going through O'Hare tonight. He took the time to call me, at the end of a gray cold day. He seemed much more calm than last time, perhaps because his personal position is now a matter of public record. He wanted to know what was happening behind the scene in France. I gave him a vague answer, without mentioning how high our contacts went. He told me he felt good about the Colorado team, given Condon's reputation as an intellectually independent man: he once refused to work at Oak Ridge because of the security requirements there, an action that led to his loyalty being questioned during the witch hunts. McDonald only regrets seeing no field-experienced scientist on the team. He is absolutely right on this point: all the members are laboratory men. How can they possibly relate to the human reality of the sightings?

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I pointed out to him that it was a pity he didn't join forces with Allen. He did not reject the possibility of doing so in the future, noting that Hynek's statements had recently evolved in a more positive direction. I did not tell him that Allen had deliberately used the opportunity *to* write a foreword to *Challenge* as a springboard to answer his criticisms.

### **Chicago. Sunday 23 October 1966.**

The whole weekend was spent gathering ideas and documents to complete my doctoral research.

Hynek called tonight. He had just returned from Colorado where he had had dinner with the Condon committee and with his friend Dr. Roach, the same man I heard in June 1963 discussing the scientific training of astronauts at the National Bureau of Standards.

"My first impression? They will approach the subject very serious-

"Who was at the dinner?"

"Well, most of the committee, and the wives too. The wives thought the whole project was ridiculous, of course. Especially Mrs. Condon."

"What makes you think they are serious, then?"

"Roach and Condon feel they have a real scientific responsibility here. And the psychologist in the group, too, a fellow named Wertheimer. I read the Minot case to them, and you could have heard a pin drop."

"Is our trip still on?"

"Yes, they've invited us on November 11 and 12. And it will be a seminar, as you requested. I will speak for an hour, then a break, then I'm counting on you to take over for an hour. General discussion is planned over lunch and for the rest of the day."

### **Chicago. Thursday 27 October 1966.**

My dissertation is written. It is entitled "Search Strategies and Retrieval Languages." It is really about the interrogation of databases in natural language. Janine and I spent a long, tedious evening making and binding six copies for the committee.

For the last three weeks I have felt exhausted. I saw a doctor today for an extensive series of tests.

If Europe is often sordid, the United States only offer two choices: the grandiose and the utterly slimy. For the last year I have not noticed

very much that was grandiose around here. Chicago has become *very* ugly, with its rundown buildings, its drunks along Skid Row, its corrupt political machinations bordering on the criminal.

**Chicago. Saturday 29 October 1966.**

Contrary to what I feel, the doctor says I am in excellent shape. Back to work.

Raymond Veillith, the editor of *Lumières dans la Nuit*, has sent me a collection of the old French UFO magazine *Ouranos*. I am reading it with surprise and delight. The founders, Marc Thirouin and Jimmy Guieu, had grasped the nature of this problem well before 1954. Their magazine was outstanding. It also provided a useful service in recording the exaggerated confidence prevalent among the skeptics; witness the following statement by Le Lionnais, president of the Association of French Scientific Writers, and Great Inquisitor of Rationalism:

The belief in flying saucers ... is one of the most regrettable hoaxes of our time. Naturally, no man of science places the slightest credit in it. Statements implying that the Pentagon considers it seriously are false—it would be highly desirable to eliminate this intellectual poison from our country: it can *only* weaken the mental health of the nation.

**Chicago. Sunday 30 October 1966.**

A review of the key sightings of the last five years occupied me all day. I have concentrated my efforts on analyzing three contact cases involving humanoids, namely the Betty and Barney Hill abduction, the Antonio Vilas Boas sexual episode and the Douglas case. I also compiled a summary of the Brazilian *Lead Mask* case in Niteroi near Rio, where two *men* who were known to be engaged in an effort to contact flying saucers were found dead on a hillside with no apparent sign of violence. Several witnesses had reported a strange luminous disk over the site the same evening when they died. This opens up a much wider enigma than I had suspected when I first read a notice of it in a French newspaper; it *takes* the subject beyond everything Blue Book and the Condon committee are doing.

**Chicago. Friday 4 November 1966.**

Mimi Hynek just called me to say we are scheduled to have dinner with the leading Brazilian researcher, Dr. Olavo Fontes, and his wife in a Clark Street restaurant. They have been in town for two days. From our first meeting it was clear that Fontes was very much up-to-date on the situation in the U.S., thanks to his APRO contacts. Also in town is Dr. Duggin, an Australian scientist who is doing research privately. We all attended an interview Hynek was doing for WGN and the three of us ended up on camera, lending an international flavor to the show.

In the evening everybody met at our Bryn Mawr Avenue flat. Bill and his wife brought an editor from the *Saturday Evening Post*. The Mittmans also attended.

Late that night I took Duggin and Fontes to their hotels and I drove back home along the lake at 2 o'clock in the morning, turning some of their statements over and over in my head. Fontes is convinced we are living the last few years before the revelation that a space invasion is in progress. I do not share his point of view, but I cannot prove that he is wrong.

**Chicago. Saturday 5 November 1966.**

Last night over dinner Hynek made an interesting remark: If the Condon committee were to reach the conclusion that UFOs did exist, responsibility for their study would immediately be taken away from him. A similar situation existed when the Atomic Energy Commission was created. Overnight, Fermi and Oppenheimer ceased to be free academic physicists. They became the employees of an enormous political machine.

"I have always been struck by that sudden abdication on the part of the atomic scientists," said Hynek. "After all, they were the ones with the knowledge and the power. The politicians could not do anything without them. But they were unable to defend this treasure, the impending energy revolution, against the greed and the thirst for power of the bureaucrats and the military."

The very nature of our problem has selected those individuals who choose to work on it, men with high ethical standards and personal integrity. Unfortunately these men are isolated, thousands of miles away from each other, in a divided world with poor communications. Their



own work is not devoid of ego problems. On Monday Hynek received a confidential letter at the observatory. It came from Pierre Guerin, who was warning him against accepting a space science prize for which Aime Michel had proposed his name. Naturally, Hynek asked me to review this "confidential" letter for him, after which he was disgusted with everything French.

I am not far from having the same feelings. Professor Rocard has not given any sign of life since I visited him. Yet I had taken the unusual step of bringing him our very best data, our most detailed cases. His interest in them is reflected in the fact that he never returned these documents, which are probably filed somewhere in the dark recesses of some French office. But he never bothered to give us an answer regarding France's possible interest in a debate at the United Nations. I feel I have been used, a feeling one often gets in working with the French higher circles. The next time an outstanding case comes my way, it is not to them that I will bring it.

### **Chicago. Sunday 13 November 1966.**

Last night I came back from Colorado with Hynek, following two days in Boulder with the Condon team. All our hopes were distilled into those two days.

First it was Denver rising from the night horizon beyond the wing of the plane, Dvo rak music playing in our earphones. At Stapleton Airport we rented a little white sports car. Hynek took the wheel.

"Well Jacques," he began as we drove on the freeway that connects the city to its academic suburb, "this is a truly historic occasion. To tell you the truth, I have been waiting for this all my life. Did I ever tell you how I became interested in science?"

"Wasn't your mother a schoolteacher? You told me she once gave you a book about astronomy that fascinated you."

"That's not what made me decide to take up science as a profession. So many people get into science looking for power, or for a chance to make some big discovery that will put their name into history books\_\_\_\_ For me the challenge was to find out the very limitations of science, the places where it broke down, the phenomena it didn't explain."

"Had you studied the paranormal before you decided to become an astronomer?"

## FORBIDDEN SCIENCE

"I had spent a great deal of time reading about esoteric subjects. Of course I wouldn't say any of this to my colleagues, they would think I'm crazy. But as a student I read everything I could find about the Rosicrucians and the hermetic philosophers."

It was my turn to take a deep breath.

"I might as well confess to you that I have spent several years in the same studies," I finally told Hynek. "Until recently I even followed the Rosicrucian Order."

"Which one?" asked Hynek.

"AMORC, which is headquartered in San Jose."

"You know there are several movements that call themselves Rosicrucian, don't you? Among the hermetic writers I was very impressed with Max Heindel when I was younger, until I started reading the books by Manly Hall. Eventually that led me to Rudolf Steiner, who I believe is the deepest of the group." <sup>4</sup>

"I always admired the old traditions which state that there is no such thing as a physical Rosicrucian organization. The only valid Rosicrucian Order, they claim, is not on this level of existence. And they insist that the true initiation, the only illumination of the spirit that counts, cannot come from any human master, but only from nature herself. When I read this I dropped my membership to the San Jose group. I continue to wonder if there may be a genuine 'Rose+Croix' that remains invisible."

There was a silence as we both savored the realization that we had followed such a similar course.

"I have never stopped thinking about what must lie beyond all this, he said with a gesture that encompassed the dark shapes of the Rocky Mountains to the West and the vast plains to the East. "I never cease to be fascinated by the limitations of our science, as great and amazing and powerful as it has become. Now we are about to see how it handles this phenomenon of UFOs that has become so familiar to you and to me. Yes, we shall see..."

The next morning we were standing with our backs to the blackboard in a lecture room on the University of Colorado campus. In front of us, seated at a horseshoe table, fifteen members of the committee. Professor Edward U. Condon, internationally known atomic scientist and author of America's first book on quantum mechanics, was at the head of the table. To his left, project administrator Robert Low and

## PENTACLE

physicist Dr. Franklin Roach. To his right, three psychologists including Dr. Wertheimer, the son of a Gestalt theory pioneer, and a cloud of assistants and graduate students. A secretary ran the tape recorder.

At several points during his presentation Hynek requested that the tape be halted so he could tell the whole story of the weaknesses of Blue Book, giving a blunt appraisal of the various officers who led the project. Ruppelt and Friend were the only ones for whom he felt some *respect*. The *committee* was fascinated.

In an effort to give an understated presentation I had entitled my own section of the briefing "Analysis techniques applicable to the UFO problem," keeping it suitably academic. Unfortunately the group did not include any information scientist, so some of my more technical recommendations missed their target. In spite of this the committee praised my summary, decided to follow the guidelines I had suggested for their own computer compilations, and adjourned for lunch.

Hynek gave a splendid press conference where he appeared as a man who was finally vindicated after years of unrecognized labor. He fielded some vicious questions quite well. After the reporters had gone we went back to work with the committee. I stressed the global nature of the phenomenon, with a history of the sightings as seen from Europe, an important element, I told them, for anyone trying to understand the whole problem. As I looked in Condon's direction during this presentation I was dismayed to observe that he had fallen asleep. Bob Low caught my eye and motioned for me to go on as if nothing was happening. I later learned it was quite common for him to snooze after lunch.

At six o'clock Hynek and I left the campus and we drove to the house of George Gamow, where we celebrated what we felt was a real victory with vodka and good cheer. Gamow's big guard dog was playing in the snow around the jumble of rocks we saw through the wide living room window. Hynek reclined happily in a big armchair and popped almonds into his mouth while Gamow told us stories *of the world of physics* before World War II. Here, I thought, was the man who first formulated the "Big Bang" theory, assuming that the universe cooled down as it expanded, a man who had worked in the old days of atomic theory with Madame Curie and with de Broglie. He reminisced happily about his visits to Paris.

"I was just a graduate student when I had a chance to go see de Broglie. I rang the bell at his mansion and a butler introduced me. Right there I realized we had a big problem, because I spoke no French, and de Broglie spoke no English, so that we had to conduct our exchange of ideas through equations hastily written on pieces of paper."

"A week later I attended a physics meeting in London where de Broglie was the keynote speaker . . ." Gamow chuckled: "The same man I had just visited in Paris went to the lectern with calm, measured steps and delivered a brilliant presentation, without any notes, and *in perfect English*. I was thunderstruck. Had he learned the language in one week? I enquired of an older American physicist who was acquainted with the European scene. "You fool, he told me, when you go see the Prince de Broglie in Paris, of course you're expected to speak French!"

I told Gamow how much pleasure I had had, as a young French student, devouring his "Mr. Tompkins" series, which was full of wit and gave me my first glimpse of relativity and quantum theory. He was so pleased to hear my confession that he rummaged through his library and came back with one of his books, which he inscribed to me.

We left Gamow and went up into the mountains for dinner with Bob Low and his wife. Low asked me bluntly how old I was. That question, it seems, had been the subject of an animated debate among the committee after my briefing. Having settled that point I brought up the international aspect again, a topic I had privately discussed with Condon at lunch: Did he know Professor Rocard? I had asked him. The French would welcome a discreet relationship with the committee, if only as a matter of mutual information. I also told Low the names of some French scientists of international standing who could help manage the long-term implications of this research if the University of Colorado ever recognized it as a significant problem for science.

"I had thought of Jean-Claude Pecker," said Low, lifting his cocktail glass. He seemed to reflect on that and changed his mind:

"Come to think of it, perhaps it would be better to keep the French out of this altogether. What we have here is a group of American scientists who are on a good track, they are really self-sufficient." Bob Low went on: "The French couldn't be expected to make any real contribution anyway. They would just muddy up the water, as they always do, with their personal quarrels and their big egos."

We went to the **Lows'** house afterwards to watch our press conference on the nightly news. The reporter commented that it was just as hard to pin down Dr. Hynek as it was to catch a flying saucer.

On Saturday morning we had a smaller meeting under the informal leadership of Franklin Roach. "What are the practical objectives we should set?" he asked. "And what tactics can we use to satisfy both the Air Force and the politicians up in Washington while following an impeccable methodology from the viewpoint of the National Academy of Sciences?"

"Clearly the report must answer the question, *is there or is there not a UFO problem?*" proposed Hynek.

"What about the computer statistics?" asked Roach.

"For heaven's sake," I insisted, "whatever you do, take the time to do it thoroughly. I know you only have a couple of years, but a rushed computer job on the basic data will bias your whole effort, you'll only get garbage."

We were free at noon. I bought the Denver papers to find out if our press conference had had any impact. Nothing. Clearly the media did not share our sense of a historic occasion. I did find a small article on an inside page, entitled "Two Giraffes are expected at the Denver Zoo." I showed it to Hynek:

"That could apply to me," I mused. "People are always commenting that I am too tall."

"You seem to forget that my name is High-Neck!" he replied as we both burst out laughing.

We flew back to Chicago with a feeling of elation and enthusiasm, certain that we had influenced the committee and the research which would follow.

"Do you realize that all those people at NICAP would have given anything to attend that meeting?" Hynek said as the plane took off. We went over the data we had presented, and he concluded: "I think we've done all we could."

### **Chicago. Monday 14 November 1966.**

Yesterday's *Denver Post* did mention our visit to Boulder, under the headline "Respectability Given Saucers With CU Job." It quoted Allen:

Hynek said it's highly likely there's life on other planets somewhere in the universe, but not very likely that any spacecraft have

visited earth. Flying saucers so far "exist only as reports," he said. Some of these reports have substance, "but there is no hardware," Hynek said—no tangible evidence of saucer visits.

**Chicago. Wednesday 23 November 1966.**

In the brand-new computation center at Northwestern I have a fine office looking out to Lake Michigan. I cleared another big hurdle today. My thesis committee reviewed my progress on the Altair system. After I fielded their questions, with Albert Grau, one of the architects of Algol, giving me a hard time on language formalism, they asked me to leave the room so they could deliberate on my fate. Finally Gilbert Krulee opened the door, stepped into the hall:

"Congratulations," he said, shaking my hand warmly.

"Make no mistake, Jacques, it's over," Allen said later as we sat in front of a couple of beers in Skokie. "You have finished, except for a little polishing up on the text of your dissertation. That was the last examination you'll ever take. You are a doctor, even if the formal graduation remains a few months away."

Is it really over? I am still trying to catch my breath.

**Chicago. Saturday 26 November 1966.**

Now I feel like a convalescent recovering from a dreadful disease. I am relaxing in bed, with the Air Force files spread out around me all over the blankets. The phone just rang: I did not bother to pick it up. This morning some television reporter called from New York. He wanted to do a show on UFOs. I turned down his offer to fly me to Manhattan. "That doesn't matter," he said, "we have a team right in Chicago to tape you there." Regnery usually filters these requests.

I want to be left alone to resume my research. I have nothing to say to the world, no message. I am leading no crusade. I have published my data and anyone who knows how to read can find it in my books. Now it is within myself I want to look for truth. I don't care if others approve, or even notice.

There is no true science without visionary thought.

**Chicago. Sunday 27 November 1966.**

I have just finished reviewing the Air Force files for the first eight months of 1965. In numerous cases I find myself in sharp disagreement with Hynek's conclusions. He rarely challenges the Air Force's rejection of the reports. When they write "Aircraft" he tempers it with "Possible Aircraft." Yet some of these so-called *possible aircraft* perform maneuvers that are well beyond the laws of aerodynamics. I realize he is still in a delicate position. He has been fighting like this for nineteen years. But he does present a broad target to critics like Jim McDonald. So far I am the only scientist outside the Air Force who has studied the cases one by one with enough attention to detail to see what was happening. A case in point is the Rapid City sighting of 2 August 1965, which the Air Force explained as "stars" with Hynek's blessing. Yet witnesses located in geographically separate sites saw the lights move to the East, a radar also tracked the objects and they were seen from an Air Force control tower and from several missile bases: How could they be stars? "You are much too timid," I told Allen again the other evening.

**Chicago. Saturday 3 December 1966.**

Long letters to be typed. They go to Marc Thirouin, to Rene Fouere, to Raymond Veillith and Guy Tarade in France; to Galia in Moscow, with a copy of *Challenge* I am asking her to deliver to the Lenin Library; to Alexander Kazantsev; to Charles Bowen at the *Flying Saucer Review* in London.

Now it is two o'clock in the afternoon, everything is already dark, the electric lights are turned on in our apartment. Olivier is playing with the large parking garage I built out of plywood for his third birthday. Outside the snow is hard, dirty, crackling underfoot.

What a marvellous tribute, the description of Paracelsus by Strunz:

He had a mind with powerful abilities whose rare maturity translated scientific problems into warm human terms\_\_\_\_His restless life never removed from him the enchantment that flooded the immortal impulses of his soul like a golden sun, the vision that belongs to a great poet of nature. Yet few men of his time recognized, as he did, the fantastic results to be gained through the

empirical-deductive **method**—Paracelsus felt **like** an artist and thought like a mathematician, exactly as he combined the laws of nature and those of the microcosm, that is, the laws of man and those of his consciousness, his feelings and his desires.

**Chicago. Sunday 11 December 1966.**

In a few minutes Janine will call me for dinner. I have spent two days of happiness, although an outside observer would say that all I have done is to retype the final text of my dissertation, and to redraw all the figures. My happiness comes from having Janine and Olivier here with me, making a paradise as I write. I cannot imagine a deeper sense of life than working hard, with both of them so close to me.

Last week I interviewed with a man from IBM to discuss work opportunities outside the United States. He said they would greet me with open arms if I wanted to join them either here or in France. They do have a facility near Nice. But why should we go and bury ourselves in sleepy Provence, when we could be changing the world?

**Chicago. Wednesday 14 December 1966.**

Marc Thirouin has sent me a very friendly and warm letter. I miss France and I am not looking forward to a third winter in Chicago. The wind fails to disperse the unhealthy haze that hovers above the city. The sunset is pink with chemicals and gray with dusty rain. Clark Street is greasy and fat.

**Chicago. Saturday 17 December 1966.**

Before I become too over-inflated with my new academic distinction, I should note what Paracelsus has to say about the title of Doctor:

Doctors? What use is the name, the title and the robe, if one lacks the knowledge? To whom are due the honors, the ring and the **mantle** of the Doctor, if not to those who deserve them by their science? When the illness of our patients pushes our backs against the wall, then the splendor, the title and the ring become as useful to us as a horse's tail.

Last night we drove to Northbrook for Bill Powers' annual party. Most of the guests were local socialites full of conventional wisdom. We



passed unnoticed, as we talked quietly in a corner with Sam Randlett.

We saw Allen between two planes. He was flying back from Dayton, where Quintanilla is still mad at him for his account of the Michigan fiasco in a recent *Post* article. Every time we are together the Marsh Gas case comes up in the conversation, and he tells me again: "Jacques, things would have been so different if only I had taken you to Michigan with me!" Yet I can't understand Hynek's recent actions. He is far too apologetic about the Marsh Gas case, and puts himself into a position of weakness that is unnecessary. And he travels more than ever. Over the last two weeks he has not spent five days in Chicago.

Allen dabbles in too many things. A few months ago he was fascinated with psychic surgery. Now he is publicly quoted as supporting an even more shaky affair, the alleged "psychic photography" of Ted Serios, a beer-drinking "psychic" who stares into a Polaroid camera and produces pictures of the leaning tower of Pisa and other monuments. It can be argued that such claims deserve to be investigated with an open mind. But why does Hynek need to make public statements about such matters when he has only met the man once, at an evening party, and has conducted no serious test? I told him things would be different if a special camera had been designed with detectors inside, to establish whether or not light penetrated through the lens when Ted Serios operated.

### **Chicago. Monday 19 December 1966.**

Snow mixed with rain, darkness. Here is a cross section of newspaper headlines: More Army divisions sent to Vietnam. Who killed Kennedy? Is Queen Elizabeth really happy with her husband?

I will not join the Faculty at Northwestern. I have too little empathy for the students here: most of them are just rich brats who go through the motions of learning. And I certainly have no respect left for the administration.

This morning our Control Data mainframe refused my program: Too many transfer addresses, too many transfer addresses ..." Ah, my dear machine, if you only knew!

A griffin is roaring within me, a devil scratches at the waxing moon with his trident. I have a date on the hillside with nymphs and salamanders. Every tiny second is filled with pleasure, knowing that I am alive, and Janine and Olivier with me, and the countless galaxies above.

**Chicago. Wednesday 21 December 1966.**

I have to fight a lingering cold to drag myself to work. After lunch I get so tired that I drive home, listening to country music to try to boost my energy. (*Kansas City Star, That's what I are...*)

I have started to write a satirical novel called *Zorponna*, in which Parisian students attempt a revolution against the Establishment. The uprising fails utterly, of course, but not without some very Rabelaisian episodes. One of the characters is a giant named Major Syrtis who tells the students:

Miserable dwarves, you would start a revolution? Ah, this is laughable! Already you are afraid that the guards might come here, and you speak of seizing power? No, a revolution with such servants would be little more than a joke.

Instead it is the giant who ends up destroying the pompous city of science when he realizes the pitiful state of academic knowledge. He starts crying rivers of tears which soon engulf the whole country and reduce it to little more than a salt island battered by the ocean waves.

I doubt if this will ever move a publisher to the point of printing the book.

Someone has sent me a packet of newspaper clippings from France. They show a distressing picture of errors and misinformation about the status of UFO research in the U.S. The Air Force is constantly misquoted. French journalists are so sloppy they even confuse NICAP with Blue Book! If somebody had paid them to cloud up the issue completely they could not have done it any better.

**Chicago. Thursday 22 December 1966.**

Hynek has returned from Dayton with pessimistic news again, having spent a long time with Bob Low. NICAP has made such an impression on the committee, and they have done such a great job bad-mouthing everyone else, that Condon has decided not to establish any ties with Coral and Jim Lorenzen, the leaders of rival APRO, because "Mrs. Lorenzen's ideas are too eccentric." Yet APRO probably has the best world-wide sighting files.

The committee has started to work on something they call "the

Wertheimer hypothesis." According to this newly hatched wonder theory the phenomenon does contain some authentic elements but they are beyond the scope of science. In other words, flying saucers are as foreign to scientific thought as the possible reality of God.

"I don't call that a hypothesis," fumes Hynek. "I call it an abdication."

These people are ignoring everything we have told them in our briefing, every practical fact. The tragic thing is that the rest of the scientific world is fully expecting an answer based on serious analysis.

**Chicago. Tuesday 27 December 1966.**

Two Northwestern professors are proposing the creation of an information science center of which they offer me the directorship. In the meantime the news from Paris is not encouraging. French scientists are simply hoping to stay well enough informed so they can take some of the credit if the wind should suddenly turn in America. But they are afraid of facing their responsibilities, of handling openly the observations that come from their own people.

Allen has just made a big decision: Stating that he "must act according to his national responsibilities in this field," he is going to transfer all his personal files to the observatory. He wants Bill and me to devote a lot of energy to case analysis. He himself promises to devote all his spare time to it during the next quarter.

**Chicago. Friday 6 January 1967.**

We had another meeting at Bryn Mawr last night. Fred Beckman brought over University of Chicago mathematician John Thompson, of group theory fame, and two other senior scientists. Things did not go well, however. Instead of giving them his best arguments Hynek, who had just flown back from Los Angeles, practically fell asleep on our couch. Now he is gone again, flying off to yet another lecture.

The sad fact is that we no longer share the same viewpoint.

He loves so much to be in the spotlight that he squeezes the last drop of publicity his association with the Air Force brings him. Perhaps that is his way to seek revenge for the ridicule of the Marsh Gas season, or to compensate for the fact that the University of Colorado has been given responsibility, authority and money to conduct a study which, in all fair-

ness, should have been entrusted to him. But his commitment to spend time with us on the analysis of the data is not being kept, and our personal motivations have started to diverge. Last September he had promised me that we would go back underground if a radical change had not happened by Christmas. He has forgotten that agreement. And he pays little attention to Fred's urging that our first step should be to create a support group of really top scientific talent.

**Chicago. Saturday 7 January 1967.**

Hynek seems to be repeating NICAP's old mistake, taking his case before the media, showing his face on every channel in every town in America. Now he is all excited about writing an article for *Playboy*. I told him rather bluntly that what he was doing now was show biz, not science.

**Chicago. Sunday 29 January 1967.**

I have been reading a copy of *Le Com te de Gabalis*, lent to me by Don Hanlon, a delightful story of occultists having intimate relationships with the fair creatures of the Elements.<sup>5</sup> Alas! The precious salamanders of my own reverie, if they ever look for me here, will come too late. Nothing can keep us in Chicago beyond this year. We may be going back to France as early as March.

In my last letter to Kazantsev I suggested that we collaborate on an article for *Priroda*, the Soviet equivalent of *Science* magazine. He answered me in Russian. It took me most of Friday to translate his letter, but we are snowed in, so I had nothing else to do. It was well worth the effort. He was asking me: "Why should we waste our time with a little publication which is only read by a few academic stuffed shirts? I have just spoken on the phone to the editor of *Young Technology*, which runs nearly two million copies a month. He is waiting for our article!"

Yesterday two of our astronauts died in the fire that destroyed an Apollo capsule. That means a six-month delay in the race to the moon. There is a tragic lack of logic in our rocket program. Between the politics of Vietnam and the management of our space technology, America is making very costly mistakes.

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## **Chicago. Monday 30 January 1967.**

"My poor children, I'll get you out of this mess yet!" I vowed as I sat at the kitchen table this evening, drinking my coffee alone, surrounded with shadows and half a dozen socks Janine had put out to dry.

I had just come back on foot, carrying Olivier through streets covered with three feet of snow and ice in the colorless twilight. Stalled cars are in the way and snowplows cannot move, even through the side streets. Janine has called. Her program must run on a computer that is not free until five o'clock. She will come back whenever she can.

In the Evanston train all the faces seem old and lost. The wealthy and the poor share the same look in this cold, as if they had dropped out of the sky into a place where city blocks follow other city blocks without reason.

Nothing is happening here. The very existence of the Condon committee is freezing all independent efforts. Hynek went to **Dayton** without being able to see Quintanilla, who is still mad at him. The Air Force seems to delight in the idea of shielding itself behind the University of Colorado.

Would our return to Europe be an abdication? Perhaps there are other solutions. My friend Jean Baudot, who is administrator of the computing center at the University of Montreal,<sup>6</sup> has written to inform me that Radio-Canada was flooded with reports from the Canadian public which no one has taken the trouble to analyze seriously. Perhaps we could try to open in Canada the "Second Front" which is proving so hard to start in Europe. As long as the U.S. Air Force has a world monopoly on UFO research, nothing will get done.

## **Chicago. Sunday 5 February 1967.**

A journalist named Otto Binder has sent me a copy of John Keel's interview with Colonel Freeman, a spokesman for the Air Force in the

Pentagon. Freeman told Keel that he was currently trying to identify certain individuals who had approached and intimidated UFO witnesses. *They wore military uniforms*, he said, *but nobody knew who they were*.

When I asked Hynek about this, he told me that he had come across an instance of the same mystery. During his recent stay in Dayton he had heard Lieutenant Morley frantically calling up every Air Force facility in New York and New Jersey to find out who was responsible for certain military investigations reportedly under way at Wanaque, where a peculiar object has been *seen* over a large reservoir. It wasn't a Blue Book investigation, and no other group would admit to being involved, so the Air Force itself is forced to start looking under the bed.

Our relations with the Condon Committee are getting tense. Following a trip Bob Low and Bill Powers took together to Joplin, Missouri, the committee has formally requested that any future investigation be conducted separately rather than jointly in the future. Was Bob Low shocked to find out that Bill was a far more knowledgeable researcher than he was? Perhaps they don't like to admit that they prefer to study the witnesses rather than the phenomenon itself. Thus they have summoned Kenneth Arnold, the private pilot who coined the term "flying saucer" twenty years ago, to ask him *why he wanted to believe* in UFOs. They are preparing a psychological questionnaire aimed at witnesses. Physicist Franklin Roach has quietly left for Hawaii. This leaves the committee under the control of four psychologists around Condon, who doesn't seem to give a damn about the whole subject.

### **Chicago. Monday 6 February 1967.**

The release of my Altair system is having a strange effect on the astronomers to whom it is dedicated, and there are some interesting social lessons to be drawn from it. This program is an artificial intelligence system that takes questions in English, questions such as "How many stars having a spectrum between GO and K2 are triple?" which previously required the writing and debugging of complex special programs and a 24-hour delay in getting each answer by conventional means. Furthermore, astronomers who do this kind of work need fast answers and are not necessarily skilled in programming. For **all** those reasons Altair was a big sensation for about three weeks. Then, remarkably, people stopped using it and returned to programming things the hard way. This hasn't

affected my work, since I have now moved beyond these experiments in natural language processing to develop generalized data-base management systems. But I am a little piqued by this lack of interest in a program that, after all, has delivered all the benefits it was designed to bring.

One of the astronomers who should be a primary user of Altair finally confided to me why he had returned to the old clumsy way of doing things: he has recently mastered a computer language, and he is so thrilled at this new skill that he is disheartened by the idea that the machine can solve his problem without the need to write a program at all, since Altair generates its own internal code and runs it without the user being aware of it. He prefers to run the tape through a different computer program every time he needs a new statistical answer.

This leads to an important observation about artificial intelligence. When the genuine power of the computer is unleashed, it is a threat so direct and so close to us that the intended users tend to develop strategies to ignore it.

In my work at the computation center I have now adopted a data-base language called Infol (Information-oriented language) developed by a man named Bill Olle and his team at Control Data. Infol is the first in a new generation of software called *non-procedural languages* because they don't require the user to specify how the computer should do something. They simply indicate what they want, and the system itself generates the appropriate procedure.

The Infol system is only available for a particular machine, and of course we have a different model for which it does not run. I have started to write a new Infol compiler in Fortran, a language which is widely used. This will make it possible to move it to any other machine when the need arises, and it will solve a large class of data-handling problems for the university community, from medical data-bases to geology or the management of student records.

This morning I returned a call from Jim Wadsworth, a research associate with the Condon Committee. He is working with psychometrician David Saunders, an expert in statistics who does most of the committee's computer work. Wadsworth wants me to turn over to them my whole computer catalogue of UFO cases, in order to integrate our work into their files. In the spirit of scientific cooperation I agreed to do it, perhaps foolishly, and I did not ask for any compensation for the many

years of hard work Janine and I have put into this catalogue; not only into the data that was included, but into all the cases that we screened out.

My only fear is that they will drown the fish of our careful research into an ocean of unfiltered statistics and will come out with no pattern at all—hence no UFO phenomenon. And they will be able to claim that their data *supersedes* ours. This game has been played before, whenever statisticians wanted to kill an unpopular theory.

A terrible cold wave has come to Chicago. There is so much snow in the alley that I cannot get the little Buick out of the garage. Every morning I carry Olivier to the house of the sitters, who live seven blocks away.

A skeptical science writer named Philip Klass has added me to his list of favorite targets, and we have had one tense telephone conversation.

"Have you ever seen an airplane?" he began.

His style is that of the trial litigator who tricks you into admitting that you once made a wrong turn into a one-way street and deduces from that fact that you are capable of murdering your own grandmother.

Philip Klass explains all UFOs in terms of hoaxes, plasma phenomena and increasingly implausible atmospheric effects that leave far behind the most extreme miracles of Menzelian physics.

I declined to respond to his critiques in *Aviation Week*. Instead I sent him a short poem I wrote entitled *Klass dismissed* that rhymed in -ass from beginning to end. It was inspired by the story of Brazilian farmer Antonio Vilas Boas (another name rhyming with Klass) who once assured Dr. Fontes he had been abducted inside a flying saucer and was forced to have sex with its female occupant:

Many nights ago I have seen, alas!  
A wanton plasma, a fiery mass.  
The glow of copper, the luster of brass  
Flashed across the sky with a tail of gas.

It landed, my friend, better to harass  
My deluded mind, and a pretty lass  
Alighted, wearing a robe of canvas.  
"Come here," she said, "and show me with class  
How life goes on, and theories pass?"



It wasn't a very good poem, but it did the job. I have had no further problem with Philip Klass, who has a good sense of humor.

**Chicago. Saturday 11 February 1967.**

A Northwestern professor wants to organize a meeting of psychologists in Washington this September to debate UFOs. He is also inviting Wertheimer of the Colorado committee. What is the use of such academic games? Wertheimer is already convinced the subject has no scientific validity whatsoever. Fred Beckman tells me that the psychologists of the German school, and particularly the Gestalt theoreticians, have always been known as dogmatists and doctrinaires. The Gestalt collapsed about 1930 but some of its old branches show new signs of green life from time to time.

**Chicago. Sunday 12 February 1967.**

At the moment Fred is the most active member of our group. We are frustrated because there is a flood of new sightings coming from Southern Illinois and we cannot do anything about it. Even calling the local police using Hynek's name is hard to do: the Air Force won't pay for the calls, we don't have any money, and we can't get the University involved. Hynek himself is always away, giving lectures.

Fred keeps telling Allen he is wrong in not seeking serious contact with top-level scientists, and that he should stay away from publicity instead of courting it.

"All you will accomplish is to drag the whole subject down," he says whenever we can corner Allen for a few minutes.

"I have no illusion left about the integrity of my colleagues," Hynek replies. "I might as well put the problem before the public and hope that does some good. Just look at the episode of my letter to *Science* magazine: These people were acting in bad faith."

They both have a valid point, and they never solve their argument. Unfortunately Hynek ends up making ambiguous or contradictory statements that satisfy neither the academic world nor the public he is attempting to enlighten. As usual, none of the major U.S. newspapers has mentioned the current UFO wave. Yet the sightings have been sweeping Southern Illinois. The observations are concentrated around the point where the Ohio and Mississippi rivers meet, halfway to New Orleans

from Chicago.

In preparation for our leaving Chicago I have been making duplicates of all the key sighting reports in my files, classifying them by type. If the Air Force ever disbands Project Blue Book or destroys the data I will be able to reconstruct the important cases in the history of the field from my own notes.

**Chicago. Tuesday 14 February 1967.**

Today I sent a copy of my entire computer catalogue to Jim Wadsworth and Dave Saunders at the University of Colorado. A little voice in my mind called me a poor naive fool, and told me I was stupid to comply with their request.

**Chicago. Wednesday 22 February 1967.**

Bob Low, who is emerging as the key man on the Condon committee, has spent most of the day with us in Chicago. Hynek had lunch with him and drove him to the campus. I then gave him a tour of the computation center, and Bill took him to the new observatory.

This visit has taught us a few new facts about the committee. Contrary to what we thought, the psychologists have lost a lot of ground. Bill Scott, one of the most radical, has left the group. The Wertheimer delusory hypothesis they were considering last December has allegedly been dropped. "Now the real work begins," says Low. A couple of new physicists have joined the team. Condon, who will be sixty-five next week, feels tired and wants to prepare a revised edition of his complete works. He really has no interest in the UFO question, never did.

"After all," remarks Allen with irony mixed with some bitterness, "that's why he was selected to run the project!"

When we brought up Menzel's relationship to the committee we heard an unexpected diatribe. Condon did speak to Menzel on the phone for fifteen minutes, but when he hung up he turned to Bob Low and remarked: "You know, this man is insane."

Menzel has *demande*d to be allowed to spend four days with the committee and to speak to each member separately. We think this method *will* have the opposite effect to what he wants.

Low tried out a few interesting scenarios on Allen. What would he think of a final report recommending that Blue Book be closed down? His

contacts in Washington have told him bluntly that the Air Force wants to get rid of the whole issue once and for all. Hynek doesn't know what to answer to these trial balloons.

It also turns out that the CIA is now collaborating with the committee in the analysis of photographic data, a fact which will be kept out of the final report. Low has been given an extensive tour of the classified facilities at the Photographic Interpretation Center and was very impressed.

We spoke briefly after Low had left the campus. The most important thing, according to Hynek, is that the committee now realizes that there is a real UFO problem: "They understand that the water was not an illusion after all. It's wet, and it's cold."

**Chicago. Tuesday 28 February 1967.**

Last Friday we left Olivier with the sitters for the weekend. He was delighted to sleep over at his friends' house for a change. Janine and I flew off to Montreal, where we were greeted in a warm and simple fashion by Jean Baudot and his wife Stella. I brought Jean up to date on the history of our various proposals.

The next morning Jean gave me a tour of the campus, notably the new linear accelerator. I presented him with a tape containing the same data I have given to the Colorado project. In the afternoon we visited the city, and the next day we drove off to the snowy Laurentians to spend time with some of Jean's friends who have a chalet there. It was a charming, nostalgic weekend. The little villages in the snow with their quaint churches and their enchanting names reminded us of the French countryside.

On Monday we discussed computer science again. Jean introduced me to the director of the center, who surprised me by offering me a position on the faculty. I promised to consider it very seriously. It is indeed a tempting offer because it would enable me to continue to collaborate with Hynek while gaining some healthy distance away from the U.S. and its complicated scientific politics.

**Chicago. Saturday 4 March 1967.**

The bookbinders of Chicago are staying in business thanks to me, it seems. Every week I bring them another batch of case data or another part of my archives to be bound. I am buying many books on all sorts of

subjects, most recently the six volumes of Casanova's *Memoirs*, which I read with pleasure. It is a profound and honest work in spite of an unsavory reputation which is largely overstated. Certainly Casanova was a rake and an adventurer, but he was also one of the keenest observers of social and political life Europe has ever had. I also read *Flammariion's Pluralite des Mondes Habites*, Howard Hinton's *Fourth Dimension*. I have finished reading *Le Comte de Gabalis*, which gives me renewed interest in the old folklore and the knowledge that may be hidden in the grimoires of the Hermeticists, even if the evocation of amorous Salamanders seems a bit outlandish.

**Chicago. Sunday 5 March 1967.**

A current review of the history of the field cites *Anatomy* as the starting point of the reappraisal of the UFO problem. But what good is it, if no solution is in sight? At least we can better distinguish between what is significant and what is irrelevant.

Condon and the Air Force may well realize in two or three years that Hynek and I were right in 1965 to say that Aime Michel and Ruppelt were right in 1956. While they are mulling over the problem in Boulder I can use those years more effectively, to do innovative work. I must go ahead into unexplored areas. I owe it to the poor idealistic student I was a few years ago, who was so thirsty for new challenges and uncharted journeys. I cannot betray him now by resting on a few laurels.

**Chicago. Thursday 9 March 1967.**

There is now an avalanche of garbage about UFOs in all the media, from television to the tabloids, the porno magazines, the occult press, and the respected national newspapers. This includes the most grotesque hoaxes and mediocre pieces about space visitors. I am utterly disgusted by this tendency of the professional press to take a serious subject and to turn it to ridicule, yet we should have predicted this would happen. By putting the UFO question before the public Allen has unwittingly created a market for every sensational hack. Since the tabloids have no standards of accuracy or reliability they can satisfy the public's demand for ultimate "answers" much better than any scientist.

We are also witnessing the beginning of a genuine wave of sightings. Many of the reports that reach us describe objects flying over the roads and

projecting beams towards the ground.

My friends will be surprised when I turn down the Montreal offer and when I give up the leverage that my current expertise could give me at Northwestern. But I think it is precisely when we have mastered a subject that we should reject it. I have been observing my son, who will be four years old in December. I am amazed at the speed with which he learns new concepts. His secret is that he rushes without hesitation into anything that grabs his interest, and he works on it until the mystery is gone. Then he turns away from it and never looks at it again. If adults had the same intellectual stamina we could accomplish miracles every day.

Man never "conquers," "controls" or "discovers" anything about nature. He only conquers, controls, and perhaps, with luck, discovers himself.

**Chicago. Thursday 16 March 1967.**

Allen has been on a long trip to the Southwest and the West Coast.

"It would be unthinkable for Condon to reject the reality of the phenomenon," he told me when he returned. He visited Holloman Air Force Base with Bob Low and his secretary, Mary-Lou Armstrong. They had dinner together and they discussed Menzel's recent trip to Boulder. Mary-Lou laughed so hard as she recalled Menzel's speeches that she fell from her chair and landed flat on her back on the restaurant floor. Menzel's explanations for the cases were so ridiculous that only propriety and respect for a senior colleague prevented the members of the team, including Condon, from laughing openly in his face.

Bob Low also told Allen that if any additional computer work became necessary the committee wanted the contract to go to me.

It is likely that Condon will request a contract extension to produce his final report. One scenario would have him recommend the setting up of a permanent research group.

Costa de Beauregard would like to see me go back to France. He writes that the Blaise Pascal computing center is being reorganized. Why not join the National Center for Scientific Research (CNRS)? Another friend of Costa has spontaneously forwarded my resume to Meudon Observatory. I burst out laughing when I read that. A very ironic turn of events indeed, if I were to go back to Meudon\_I wonder if the rats are still scurrying under the wooden floorboards at midnight, and if the Observatory still employs same nasty accountant.

The French Society for Operations Research has accepted an article I sent them on **self-encoding** systems.<sup>7</sup>

**Chicago. Sunday 26 March 1967.**

Last night Janine and I invited Allen, Fred and Ben Mittman to join us for dinner at a fancy suburban restaurant where Janine's hard-working brother Alain is now part owner, in Aurora. I wanted to get everybody together in a somewhat formal atmosphere to announce to them I was seriously planning to leave, and to tell them frankly and openly what I thought of our current research. I had conspired with Fred, who kept deploring the lack of an opportunity to talk seriously to Hynek. He had prepared his own statement stressing two major points: First, the UFO Phenomenon is available for scientific study; although it is not reproducible at will, it does reappear frequently. Second, the application of today's technology to the phenomenon could yield important new knowledge and should enable us to establish its nature, if not its origin.

Unfortunately, if Hynek listens politely to all this, he does so in distracted silence. He follows the discussion for a few minutes, then he seems to *drift* away, occasionally jumping back in with a few anecdotes that are superficially amusing but leave us frustrated.

In private Fred tells me that he suspects the Pentagon is eager to preserve the *status quo* at all costs. It should be relatively easy to get good measurements and good photographs of the phenomenon, the kind of information he calls second-generation data. Once enough of these cases are on record we have no doubt that the entire bulldozer of American science could start rolling over it, not just a few isolated scientists. Hynek is the one who should initiate this effort.

The tragedy is, I am losing my confidence in Allen. We have outlined a systematic scientific program but we are doing nothing to implement it.

Fortunately there are many other joys in my life. This evening the stereo was playing Katchaturian's *Saber Dance*. We thought Olivier had gone to sleep, but suddenly we saw him stand up on his bed, a blissful smile on his face, one finger raised up in the air. We played the record all over again for him.

**Chicago. Wednesday 29 March 1967.**

Lunch with Allen. He read to us the draft of a chapter he had written or a proposed collective textbook on UFOs. Although clear and lucid, it is purely a rehashing of things he has said before. Bill Powers, in the meantime, has asked me to comment on the new draft of his own book on behavior, which I read with attention a few pages at a time while my programs are running.<sup>8</sup>

This evening McDonald called me to say he was coming to Chicago in three weeks. He "wanted to meet with me to discuss the results of my last trip to France. He is still trying to bring me into his team. I told him we should include Hynek, and he reluctantly agreed.

The man is as sure of himself as ever, he keeps telling everybody what to do. He has lost nothing of his arrogance.

**Chicago. Saturday 1 April 1967.**

When I mentioned to Hynek my likely departure from the United States his first reaction was to oppose it.

"The next two years are going to be vital, Jacques, they will decide everything," he told me.

"Come on, nothing will happen until Condon writes his final report anyway," I reminded him. He had to recognize that my arguments were solid.

"If you do go back to Europe, that will give me an excellent excuse to go and visit you there."

**Chicago. Sunday 2 April 1967.**

Janine makes me happier than I have ever been. Her beauty, her gentleness goes so deep inside me, it leaves me breathless.

**Chicago. Thursday 6 April 1967.**

The current time is precious, we should be dedicating it to serious research. But Hynek is drifting away from such research more and **more**. when I reminded him that Jean Baudot and the head of computation at Montreal were scheduled to meet us at O'Hare, he seemed to have forgotten all about it:

"I can't be there," he told me abruptly, "I have to speak to the students

at a college in the suburbs."

"But they come here to facilitate the exchange of UFO data with Canada," I objected, "this is a high-level contact for us \_\_\_\_"

I have put a lot of effort into preparing this meeting. The Canadians have many UFO cases that are just begging to be analyzed, and which could have an important impact on U.S. scientists. But Hynek doesn't seem to be interested in these contacts. Nor does he remember what we had decided at our dinner in Aurora. In spite of all this he remains charming, captivating, charismatic, but the fact is that he does nothing of long-term viability. He has filed away and forgotten Fred's solemn lecture and our precise proposals for the application of science to the UFO problem. I do not think that he has committed in the past the grave mistakes of which McDonald accuses him, but there is no question in my mind he is committing them now. That is another reason for me to continue to think of returning to Europe.

Aime Michel has exhausted his own contacts. He feels stumped in his efforts to get some serious attention in France:

I don't see how I could go much higher than Rocard, other than seeing the Old Man himself (De Gaulle) but he doesn't give a damn. What I see is failure all along the line. For the first time I have no alternate plan. There is no hope for your idea of a *second front* of independent research here. Everybody is looking to America\_\_\_\_The new interest in the media is limited to reprinting extracts from Frank Edwards and John Fuller's books. The newspaper bosses tell me: "Maybe you are more knowledgeable than those guys, but our readers don't think so."

In response to my last letter, where I stressed that it was unthinkable that no one would be studying the problem seriously in France, he writes:

There are some individuals who are doing their own study on a private basis, just as we do. Maybe there are some people who collect French UFO data as agents working on behalf of British, American or Soviet Intelligence. But for France itself, don't make me laugh! High Gendarmerie officials have told me what happens to the cases: the reports go up through the hierarchy depending on the level of notoriety of witnesses and the personal rank of the



officer in charge who wrote the report. When they get to the appropriate level they are kept for ten years, after which time they are burned! Therefore all the reports of 1954 have now been destroyed. It is enough to make one want to bite off one's testicles in frustration, isn't it?

And he offers his personal conclusions:

It is urgent to do nothing, at least openly. Once Condon publishes his report, which I already know by heart, we will just have to gauge the reactions.

**Chicago. Saturday 8 April 1967.**

Next week I am scheduled to attend a computer conference in Pittsburgh. Janine will take a few days off with Olivier, staying at Alain's place in Aurora.

Fred made a special trip to Evanston the other day to have the long-postponed serious discussion of the UFO problem he had been requesting for months. He found Hynek fumbling with his stereo, connecting this and that, adjusting bass and treble without once listening seriously to Fred's arguments.

"And all that to play some wretched *French can-can!*" remarked Fred with disgust.

The only thing that relieves the drabness of life in Chicago is the opportunity to have almost daily discussions with Beckman, whose expertise embraces areas as diverse as photography, classical music, electronic microscopy or neurology, and also with Bill Powers, whose book on behavior is making steady progress. Bill is another rare mind, who has analyzed the process of thought in a new way, looking at the brain as a system that handles cognitive information rather than as "an organ that secretes behavior," as he puts it.

Bill thinks that the brain must be the minimal structure that will enable man to compete and to survive, with all the flaws that implies. He has also speculated that the next step in the evolution of thought might be engineered rather than evolved.

"When I was born the triode had no practical application yet," he points out. "And look how far we have gone in the development of log-

ic structures, first with transistors and then with integrated circuits. Do you know what that means? It's much more likely that we can *build* a better thinking structure before nature has a chance to *evolve* it."

Indeed, today's integrated circuits are on the way to a reduction in size that will approximate synapses, and it is possible, at least on paper, to imagine logical devices approaching the complexity of the brain, although it will be decades before we can build them.

Bill is thinking about automata that would have no preset task; he doesn't believe in the learning machines of Newell and Simon,<sup>9</sup> which start from a set of rules such as the rules of chess and gradually learn to play better and better, but do continue to play chess and nothing else. His idea, which is heretical but simple, is that intelligence should be regarded as an attribute of the environment. And it is foolish to try to simulate it. One should build an artificial thinking being, turn it loose, and let it learn.

"If you ever build it, I'll volunteer to program it," I tell Bill.

"Then we can get Fred to critique what we've done," he jokes.

The last news we have received from the Colorado committee comes from Bob Low's secretary and administrative assistant, Mary-Lou Armstrong. A week ago she sent along a stack of recent sightings.

"We haven't heard anything particularly interesting from our other sources: radio, TV, Associated Press, NICAP etc.," she says, adding:

None of us has had the time to try and figure out possible explanations for these sightings—as we had, at one time, intended to do, so we'll certainly appreciate any comments you might have on them. The procedure we are following on the processing of sighting reports is still, at best, vague. It changes all the time. However, we are sending out questionnaires on the interesting ones, and all of them are getting punched for the computer. One of our biggest problems is that now there is only one person working on the actual coding and reading of the sightings as they come in. We had two people doing it, but one of them, Dan Culberson, was in a very bad motorcycle accident two weeks ago and is still in the hospital. Brain damage, the works, a real tragedy.

The Condon committee fascinates me. I feel that what I am observing here is the very heart of the scientific machine, as it gets corrupted by

politics. Under the thin veneer of academic respectability, it is plain human baseness we see, with no sacred fire, no warmth, no lasting value.

**Chicago. Sunday 9 April 1967.**

Aime Michel writes that he is afraid he has a tumor in the abdomen that may be malignant. I feel disheartened when I realize that a mind of his formidable caliber exists in France, and that he is left alone in a little village in the Alps.

Alexander Kazantsev writes to me that

Interest in the UFO question keeps spreading among our scientists. I am currently a member of a group of physicists that I hope to put into direct contact with you. Like the Americans, they are primarily interested in discovering the secret of UFO propulsion. They are intrigued by the report of a measurement of radioactivity at Montville, Ohio, after a landing. They would like to know the soil composition there, to compare it with soil samples from the Tunguska explosion of 1908, in Siberia.<sup>10</sup>

I don't have any data or any physical samples from the Montville case, and I am a bit curious about this "group of physicists." If they are so interested, why don't they write to us directly?

**Chicago. Tuesday 11 April 1967.**

A fellow graduate student, an athletic beach bum from San Diego, is trying to convince me to move to California. Whether I go East or West, I must make a move, because I am increasingly discouraged by Hynek's distracted approach to our work. This morning an episode took place which was particularly depressing, although its comical aspects come out in the retelling. We had a business meeting scheduled with Harvey Plotnick and his editor Betty to discuss a contract for a proposed textbook. They came to the observatory punctually at ten o'clock, having driven all the way from the Loop. No sign of Allen. His secretary told us that he was at the new observatory having his picture taken, and that in half an hour he was expected in the Dean's office. Naturally he had completely forgotten our meeting. The three of us started talking to Bill about one thing or another.

Hynek finally arrived, returned a few urgent phone calls, uttered a

few disconnected sentences, talked vaguely about the Dean and suddenly, having noticed a pile of old Ranger photographs *on top of a* filing cabinet, he grabbed them in a moment of desperate inspiration and dumped them in Harvey's lap:

"Here!" he told the astonished publisher. "I'll be back in an hour. Why don't you look at these nice pictures of the moon?"

Exit Allen, leaving us all embarrassed. I miss the days when he was not such a celebrity, when every discussion of "our subject" was a conspiracy, when every chance to advance it was treasured, every research opportunity eagerly seized. The topic has become fashionable entertainment, not serious science. Media men hire Allen as they would hire a guitar player. He rushes wherever he *sets* a spotlight, and if the spotlight moves he moves with it.

#### **Pittsburgh. Thursday 13 April 1967.**

The conference on Information Processing is taking place on the campus of Carnegie-Mellon. I skipped the banquet. I came back to my hotel on foot, stopping at the local Howard Johnson's for beef burgundy and coffee. I was briefly tempted to hire a cab and visit the city, then I thought better of it. What is the use of seeing another bunch of buildings made of steel and marble? We have all that in Chicago.

Three hundred and fifty experts are gathered here. They seem incapable of communicating with human beings. The masters of computer information are only exchanging a few superficial words when they are *not* reading aloud their own jargon-filled papers on retrieval statistics and balanced trees.

#### **Chicago. Sunday 16 April 1967.**

Kazantsev's secretary has sent me a copy of the Soviet magazine *Sme~no*. with an article by Felix Zigel that quotes *Challenge* and reproduces two pictures from it.

Yesterday I went to Hynek's house. I had promised that I would help him reorganize his files, which overflow into shoeboxes and in a wicker basket in a little room on the first floor. He complains that he *doesn't* find anything any more, and I can see why. The reports of various years are mixed together. Pictures and letters get lost.

Having established a method to reclassify this mess, I began the real

work early in the **afternoon**. By evening I had returned many of the documents to the places where they belonged, in neat folders and envelopes. But I had stumbled on something I felt was important.

I found it among the relics of Project Henry. It was a simple letter dated 1954.

It came from a cloud physicist at the University of Chicago who was studying for a doctorate at the time. Together with three other physicists he had seen a bright unidentified object in the sky over Arizona. The letter gave precise details and calculations.

It was signed James McDonald.

When I showed him the letter Hynek was dumbfounded. He examined the text, as well as the response from the Air Force, which was attached to it.

"I don't remember ever seeing this," he said.

"Evidently it went to Captain Hardin," I pointed out. "He's the one who replied to it."

"If I had remembered this I might well have contacted McDonald. We would have met much earlier."

"You could have had an influence on the policy of the Air Force, if the two of you had joined forces at the time. We could rewrite history here: when the 1954 wave swept Europe you could have contacted Aime Michel\_\_\_\_"

"The Condon Committee would never have happened," Hynek mused. But he shot the whole idea down: "I probably would have been labeled as a crackpot. Whipple wouldn't have asked me to work with him at the Smithsonian in 1957. Northwestern would never have hired me as the director of Dearborn\_You and I wouldn't be here talking."

This brought the discussion to the subject of McDonald's upcoming visit. I told Hynek he could not refuse to meet with him. He tried to get out of it:

"You could take care of it with Bill and Fred ... find out how he feels." He wanted to avoid another confrontation at all cost.

I tried to make him feel guilty: "You already forgot the meeting with the Canadians, and with our publisher. This time you've got to be there."

He finally agreed that we had to find out once and for all if he could work with McDonald. Mimi Hynek has a dinner party planned for Saturday, but it can be rescheduled.

We hear that McDonald is not having much success so far. An article he recently published in the *National Enquirer* has eroded whatever measure of respect he was beginning to gain among his peers.

On the Air Force side, Lieutenant Morley has just briefed Colonel Sleeper, who has responsibility for the Foreign Technology Division and reportedly would love to see them dump Project Blue Book altogether. In this kind of circuitous communication the facts are irrelevant. All the conclusions are already drawn.

My California friend has a job offer for me in San Diego. My first inclination is still to return to France. But Allen has vowed to keep me in this country no matter what happens.

**Chicago. Tuesday 18 April 1967.**

At lunch today I brought Bill Powers up to date. He told me he placed no trust in McDonald.

"All his views are negatively oriented," he observed. "He doesn't propose anything concrete, otherwise he would just do it and move forward. All he does is to complain and criticize."

Allen showed me an article by Walter Sullivan, science writer for the mighty *New York Times*. He uses his position to heap ridicule on this whole domain, ignoring thousands of credible cases to linger on a miserable joke in which Boulder students released plastic bags over the Colorado University campus. He delights in recounting a mistaken report of a "monster" in Pennsylvania where the witnesses may simply have come upon a stranger relieving himself in the bushes: an analysis of the soil only revealed traces of urine. Sullivan is predicting that Condon's study will point out some remarkable facts about the weakness and the fallacy of human testimony....

"That's a trial balloon," I said. "They are paving the way for a negative report. Just as Bob Low did the other day when he asked what you would do if they recommended closing down Blue Book."

"Yes, but don't you see? It's not enough to be right, Jacques, in a situation like this. You need heavy guns if you're going to fight against such people."

This evening I wrote to a few prospective employers in France; then I walked over to the drugstore to buy some stamps. The weather is still cold. I wear my winter coat with the collar turned up. In the black sky are

Mars, Jupiter, Venus and only a thin crescent moon.

**Chicago. Saturday 22 April 1967.**

Jim speaks today before the annual meeting of American newspaper editors. He has sent me the advance text of his remarks. This time all the cards are on the table. He repeats loudly what we have been saying in private about the Air Force, which he accuses of negligence, and about Menzel whom he practically calls incompetent. He also comes close to calling Hynek a coward. Yet his statement on the basic UFO problem is almost a carbon copy of Allen's own declarations, couched in a more sensational style. He accuses Menzel of not conducting a proper quantitative study, but he is guilty of the same thing. One minute he makes a big show of blowing up the doors that we had already forced open, the next he rushes forward crazily and hits his head against solid brick walls.

The fact is that Allen is not responding creatively. He does not follow Fred Beckman's advice on scientific methodology, nor does he listen to me or to friendly biologist Frank Salisbury. Instead he reacts to McDonald's attacks with superficial comments: "Did Jim get published in *Science*, as I did? Was he invited to speak before NASA?" And he doesn't pay attention to what we tell him. For instance, when I mentioned Zigel's article, he volunteered:

"I know somebody in Washington who has made some discreet inquiries about this fellow Zigel. It turns out he is a professor of aviation in Moscow."

"That's what I told you several months ago," I had to remind him. "We spoke about it over dinner in Aurora. Zigel is a friend of Kazantsev."

"Really? No, I don't remember hearing **that**."

**Chicago. Sunday 23 April 1967.**

When Fred came over to our apartment yesterday he announced that a new sighting had taken place at South Hill, Virginia, last Friday night. Two witnesses have seen an object on four legs in the middle of a road. It took off on a bed of flames, setting the asphalt on fire.

We had lunch with Allen. We pointed out that his audience was getting increasingly confused. Sometimes he talks about UFOs as if they didn't exist, other times as if they were extraterrestrial visitors, showing slide after slide of astronomical phenomena. But when he is pushed to express

a theory he wanders off into semi-mystical statements about ghosts and astral travel. We stressed that he should adopt a firm **position** and keep it, instead of wavering constantly in his answers to reporters. Also he shouldnt continue to allow himself to be used by the Air Force. He looked at us and commented ironically:

"With two grand strategists like you I can only win!"

We had to laugh, because it is true that it's easy for us to criticize him. We don't have any more influence on the media or on the Pentagon than he does.

We drove back to his house in my car and I resumed the work on the files while he called up the witnesses in Virginia. The sighting turns out to be significant. It is confirmed by the police, and there are clear traces on the ground. I suggested that Bill Powers fly there as soon as he could.

In the evening Jim McDonald came over to Bryn Mawr. Olivier was ready for bed and Janine had the place in perfect shape. I hadn't seen McDonald in a year or so. I thought he looked much older, and he seemed to have lost some weight.

He told us frankly that his press conference, without being a complete fiasco, had not led to the major fight he had been hoping for. Menzel had skillfully avoided him. Klass had wasted his time. Quintanilla had not contradicted him. Reporters in the audience had asked questions that were *too* limited in scope.

Jim continued to view Allen as the prime culprit: Hynek should have opposed Robertson and Berkner in 1953, he claims. Hynek should have given his resignation to ATIC, slammed the door, alerted public opinion.

Allen, who was the target of all these accusations, arrived as Janine was passing the coffee cups around. McDonald, who had already vented some of his anger, was cordial with him even as he resumed his tirade.

"Jim, there was nothing I could have done at the time of the Robertson Panel," Hynek reminded him. "I was an unknown, just an assistant professor at Ohio State. I spent most of my time in the hallway, waiting to be called in by the panel when they needed some specific astronomical answer."

"What about 1957?" countered McDonald. "How could you stay idle when there were so many sightings going on?"

"I had other fish to fry in 1957. You forget *Sputnik*, the whole mad-



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ness, the setting up of observation stations around the world. Also, I had no permanent *contact* with the Air Force at the time."

Gradually, Jim appeared to see the strength of these arguments. I watched him as he seemed to grasp the true reality of Blue Book. Janine offered more coffee and our traditional cake. We were all tired. McDonald and Hynek, seated a few feet away from each other in comfortable chairs, seemed almost ready to set aside their differences. I told them that in their personal arguments they were losing sight of the key scientific issues.

After midnight the ice was completely broken between them. The conversation came to Ed Condon and his mistakes, a subject on which they could naturally agree. McDonald said he had a source in Washington (which he would not reveal) who assured him Condon would soon be forced to take more money, to shake up Bob Low's loose management style, and to expand his operations, all of which gives him much hope.

They didn't leave until two o'clock this morning.

Same day, 3 p.m.

Hynek calls. Bill is now at the site of last Friday's landing at South Hill, Virginia. He has found traces one inch deep in the road surface. The dimensions of the landing gear, as deduced from the marks in the asphalt, are very close to those of Socorro. Bill had the local hardware store re-open so he could get some tools and *plaster of Paris to make* a cast. The object itself was dark and silent. It did not emit any flames, it turns out, yet the road burned underneath it.

When Allen called Bob Low to tell him what we were doing in South Hill, he found out that the Condon committee had not even heard of the case and had no intention to investigate it. Low is much more interested in directing his energy to the subject of McDonald. He is sending his Washington friends *to* interview all the important people Jim has visited there, the classic maneuver of a skilled bureaucrat doing effective damage control.

Chicago. Tuesday 2 May 1967.

Ben Mittman has spoken to me again about the information processing center Northwestern wants me to direct. I would have *carte blanche* on research. I would have assistants and a secretary. I could build

the Infol compiler and continue my own parallel research. I am tempted to accept. On the professional level my work here is fascinating. But I am missing something more important than intellectual fulfillment here. I miss something of the heart and the soul.

Allen called me at 5 p.m. He was just back from a meeting with Condon, whom he had found so sick he became seriously worried about him. Condon has cancelled all his scheduled trips and was unable to attend Hynek's lecture. When Allen reproached the committee for not doing enough field research, Bob Low answered sharply: "We are not an operational committee." They had a fight about that, with most members taking Hynek's side.

In his private meeting with Condon, Allen stressed that the committee could not make the mistake, in their final report, of stating that all the sightings had conventional explanations. He also told Condon that Northwestern was ready to undertake an in-depth study.

Allen is quite optimistic. He still hopes that Condon might recommend that a follow-on contract be given to our group. If my information center was up and running by then, we could have an impact on the course of this research. There is only one thing that is forgotten in all these fine political maneuvers: the reality of the phenomenon itself.

Chicago. **Friday** 5 May 1967.

Every morning Janine leaves for work early and she doesn't get back until 6:30 in the evening. She's an excellent business programmer, and a perfectionist, so the computer consultants who employ her exploit every minute they can get from her.

This afternoon Allen told me Condon had suddenly become very curious to find out what the Russians were doing. He answered that he had no information whatsoever, but if Condon invited me to Boulder I could probably fill him in.

"I prefer not to be involved in that," he added, "it's your business.

What kind of comment is that? It is one thing to remain suspicious and vigilant when dealing with the Soviets, and it is another to pretend they do not exist. There is a large amount of provincialism among many Americans. But Allen, who has extensively travelled, should not suffer from this. To study UFOs is like studying meteorites, or an epidemic disease: the research has to cross national and cultural boundaries.

Aime Michel writes that I might be able to get a job with Thomson, the French electronic giant. Aware of my secret priorities, he knows how to seduce me:

Salaries are half of what they are in the States, but you would have a view of the Obelisk, the *Bibliothèque Nationale*, and the booksellers of the Latin Quarter.

We hesitate. Should we stay here after all? We keep reading the ads for houses and apartments in the North Side. We visit the high-rises, then we come back to our old street where loose papers fly around. The cold wind is swirling in the schoolyard. A kid on a bicycle goes from house to house, delivering newspapers. Little faces of children smile sadly at us. The corner hardware store is slowly being driven out of existence by the supermarket across the street. In desperation, the owner just stares out the window at passing cars.

If we stay in Chicago this is where I want to live, not in some fancy house in the suburbs with a three-car garage. The only real wealth of this country is in these modest, hard-working folks. Here, with enough energy, I could build a little empire within another empire. But it would just be another academic rut, and I would have to watch Janine coming back from her job later and later every evening, her mind filled with subroutines to be tested and flow-charts to be redrawn. This is not the life I want.

### **Chicago. Sunday 7 May 1967.**

Will Spring ever come? The sky is clear and the sun stares at us from on high, but at ground level the cold wind is still dragging everybody down. Our research proposal for the Information Science center was well received by Rowena Swanson of the Air Force *Office* of Scientific Research, an influential woman who is a potential champion for our work in Washington.

Yesterday at the Midwest astronomers meeting Eric Carlson and I presented the results of our comparison of various mathematical techniques for the calculation of the radial velocity of the stars. At the computation center I make fast progress in the development of our new data-base software, and I now see clearly the solutions to problems that stopped me six months ago. Outside my job, however, I find no frame-

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work for our existence, no art of living, in spite of our expanding network of social friends. What people call their "way of life" is really just a series of mechanical actions designed to make a little more money.

Maman writes to me from Paris:

The catastrophe that has befallen the Russian cosmonaut, after the death of the Americans, makes me very sad. I don't have any courage left in order to do my work. I was really hoping that this time "they were there." However Paris is sunny, the cafes are full of people. I went out to change my mood. At the Saint-Severin cinema they showed a movie by Seban in collaboration with Marguerite Duras. It was called *La Musica*. Once again I came away disappointed: Nothing but dark philosophy, rolling by for two hours of monotonous conversation, without anything happening. I felt like screaming.

It is the spectacle of Paris that delights her:

The real cinema for me is in the streets of Paris: La Huchette, La Harpe, Galande. They are full of characters whose origin I can't even guess, but what beautiful faces! You can't tell the girls from the boys any more. Same hair, same costumes. And those little food places that belong at the end of the world\_They let you dine for ten francs by candlelight in a room full of smoke. I don't dare go in there. I feel like a foreigner in my own city.

Charles Bowen writes from London:

This morning, just as I was leaving the house for West Byfleet station I caught sight of the mailman going up the road. I slowed down and my daughter Pauline (who brings back the car) told me: "Don't stop, you'll miss your train." To which I replied: "There may be a letter from Jacques Vallee, I'll be glad to miss my train to get it." The mailman solved the question by leaning his bicycle against a garden wall and running onto the road waving a letter for me ... your letter!

Dear old Europe. America still has a long way to go to become so touching, so human. From the other end of the world our little friend Galia writes:

During my whole stay in Armenia I have regretted very much that you were unable to see the town with me, and that you could not go to the Echnyadzin, the center of the Armenian church\_\_\_\_ I must say Armenian priests are very handsome. In general Russian priests too are beautiful, but they don't have the big sparkling black eyes and the pale faces of the Armenians. The inside of the cathedral is very beautiful. Do you remember the Novodevichi convent, and the church near which I was trembling with fear because of the corpse of the old man? There were many icons hanging on the walls. The only difference between the Russian and the Armenian churches is the lack of icons in Armenia.

Chicago. Sunday 14 May 1967.

The news media report that the Pope went to Fatima yesterday. Fifty years ago three little shepherds saw a UFO and its occupant there, with the theological implications we all know. The Pope visiting a saucer landing site...and American crowds watching the event through the wonders of the Early Bird *artificial* satellite. I find this mixture of *mysticism* and technology quite amazing.

On Friday I had lunch with Bill, Don (whose destroyer is about to leave for Vietnam) and an engineer named Bielek who works in Morton Grove. He says he is a close friend of naturalist and parapsychology researcher Ivan T. Sanderson. Bielek is a big man, about forty, wearing a brown suit and a very serious and rigid attitude. He behaves like a secret agent whose pockets are full of microfilms and who wants to make sure you know it: the exact opposite, naturally, of what a real secret agent would actually look like. Listening to him is like talking to someone who has just had breakfast with J. Edgar Hoover. He throws the most extraordinary hypotheses around, but never bothers to develop them or to resolve their contradictions.

"How closely do you think the Intelligence agencies are following the saucer question?" I asked.

"They are holding regularly scheduled meetings to which the Air Force is never invited," said Bielek provocatively.

On this point at least he may be right. They must have had a secret inquiry for years. It would not be logical for a low-level project like Blue Book to be the only game in town.

I have recently found myself mixed up in an old intrigue which has been another subject of fascination among American ufologists over the last ten years. I am referring to the story of M.K. Jessup<sup>11</sup> and Carlos Allende.

When the late Jessup published his book *The Case for the UFO* in 1954, someone anonymously sent a copy of it to the Office of Naval Research in Washington. The pages were covered with hand-written scribbles, apparently authored by three different people using color ink. The point of these notes was that the authors were extraterrestrials living on earth. At about the same time, M.K. Jessup received three letters from a man named Carlos Allende or Carl Allen, who claimed to have witnessed certain Navy experiments in 1943. Run in collaboration with Einstein, these experiments were said to have attempted to make a ship disappear.

Jessup seems to have become obsessed with these stories. He also became convinced of the existence of extraterrestrial bases in South America, where he made several trips. Did he feel he was going to go crazy? Was he afraid, as one rumor suggests, that people would think there was insanity in the family because of his strange beliefs, which disturbed the very conventional patterns of American life in the fifties? One evening he gave all his papers to Ivan Sanderson. The next day he got into his car, closed the garage door and let the engine run until he was asphyxiated and died.

"So, is Sanderson going to release Jessup's papers?" I enquired.

"He can't do that, because of National Security, of course," answered Bielek.

It turns out that last January I received a postcard from Mexico, addressed to me at the observatory. It was signed *Carl Allen* and offered me an opportunity to buy a very expensive copy of the famous annotated book, of which only 114 are said to exist. I replied politely at the time, declining the offer. But it is only later that I realized I had stumbled on the track of Carlos Allende. I have no interest in the annotated book, which I believe is sheer garbage, but I am fascinated with the way apparently rational, intelligent people fall victim to such cheap "mysteries.

Chicago. Monday 15 May 1967.

A new wave of sightings has begun. My early warning indicators have served their function: there was a Type II sighting (cloud cigar) in Akron,

Ohio, on April 8 at 22:00, and five days ago there was a Type I case (landing) near Dijon. I wonder if this combination always signals the beginning of genuine waves. I have resumed work on my master card index.

This morning I met with Hynek and I got his attention long enough to obtain his advice on the steps to take with the Russians and with the United Nations, which seem to have a continuing interest in all this. We also called up Mary-Lou Armstrong in Colorado. She told us that Bob Low had gone back to Washington, where the Air Force rapped his knuckles because he didn't dispatch enough investigators into the field. As McDonald had predicted, there is now talk of additional funds to train two teams. But Low, who is a great opportunist, is already planning to use the new money to go on a junket, taking a personal trip to Prague instead. An astronomical congress is scheduled there this summer.

Chicago. Saturday 20 May 1967.

Allen has learned that differences are deepening within the Condon Committee. Wertheimer is back in the group. Condon himself neglects his duties, either because of poor health or through lack of interest. Jim Wadsworth has just returned from the state of Washington, where he went to investigate some mysterious sounds. He has found an explanation for them—they were caused by an owl, he is telling the witnesses. Besides, a local farmer has now killed an owl and there are no more sounds.

Fred called me last night. In his opinion the "owl" explanation is a joke. A friend of his, who has done his own inquiry with the local civil defense authorities, has found out that high-quality recordings were made which show artificial signal patterns. The Air Force is not going to conduct an investigation because, as Quintanilla eloquently puts it, "sounds don't fly." Instead they are thinking of financing a group of physicists who think they can generate luminescent lens-shaped objects in a gas to test the plasma hypothesis.

Yesterday a Swedish journalist told us that last March, in the North of Sweden, two gray objects had hovered over a house, terrifying the four people who lived there. The Swedish Air Force explained the sighting by saying there had been an electrical discharge between two high-tension wires, a fact which certainly would not generate dark phenomena.

Something very grave has been going on for the last twenty years, and nobody knows what it is. I am going to go back into my quiet per-

sonal research, outside the established frameworks. I will cancel my appearance at the UFO panel of the American Psychological Association, and I will tell media people that I have nothing to add to what I have already said in my books. Talking to the press now is a waste of time. They are not looking for the truth in the phenomenon, but only for entertainment.

**Chicago. Saturday 27 May 1967.**

Our little group meets again tonight at Bryn Mawr. Allen now calls it "the Invisible College" in reference to the scientific movement that preceded the creation of the Royal Society in the early 1660s in England, at a time when it was very dangerous to be interested in natural philosophy. Hynek and Bill, Fred and physicist Jarel Haslett will be there. Yesterday Allen spoke to Mary-Lou again. Menzel and Condon were in the room with her, so she had to move to another phone to speak freely. She told him the plenary session of the committee last Thursday had been a joke:

"I have 150 cases I can't explain!" said Bob Low.

"Give them to me," answered Menzel without any hesitation, "I will explain them for you!"

In the meantime not only did the committee drop the study of the peculiar sounds in the state of Washington, but they missed an opportunity to study a recent first-class case, in Winnipeg (Canada). A prospector observed a landed metallic disk. He is now at the hospital with multiple burns.<sup>12</sup> Again, Hynek shrugs: "*Testis unus, testis nullus*." A single witness is no witness at all. I fought him on that point: the man has suffered serious enough burns to be hospitalized. "If we don't go study such cases we'll never find the other witnesses who may very well exist. And why should it be surprising for the landings to have few witnesses? If there an intelligence is involved that's exactly what we should expect.

Fred, who turns out to be a patient and realistic investigator, has contacted the APRO representative for Illinois, a man named Achzener. He has custody of two fragments of the famous "steel meteorite" which he claims to be pieces of a saucer and are frequently associated with Canadian engineer Wilbert Smith.<sup>13</sup> But there is more: Achzener himself has had numerous personal sightings, and he has built a simple magnetic detector, which has inspired Fred to build a similar one in his apartment here in Chicago.



Last night (Saturday, at 1:05 in the morning) Fred was quietly talking to a friend of his, a man who is in charge of **aircraft** maintenance on the carrier *Kitty Hawk*, when he heard the detector go off, after it had been connected for barely an hour. They rushed out into the street in time to see a small orange disk, larger than Jupiter, crossing their field of view from North to South in six seconds. After this they heard a strange sound following the same apparent path. It took fifteen minutes for the magnetic pendulum to return to normal.

**Chicago. Sunday 28 May 1967.**

Fred came to see us tonight looking very pleased with himself. He was carrying an instrument about four feet high. He set it down on the floor and pointed out a plunger hanging from a long wire into a loop connected to two batteries, with a red light and a bell to give off an alarm when a magnetic anomaly is detected.

Hynek came over wearing old yellow trousers and a khaki shirt, and he spent ten minutes crawling on the carpet to try and take a picture of Fred's device. While he was doing this he casually told us he had once seen an object similar to Fred's luminous disk, when he was working at Perkins observatory. It was reddish-orange and it flew too slowly to be a meteor. It crossed the field of view defined by the slit in the dome. When Allen rushed out he was on the wrong side of the building and could not see it again.

Fred told us some fascinating details about his meetings with Achzener, mischievously adding, as a good connoisseur, that the man lived in a suburb called Brandy Wine. Achzener, who is a salesman with Motorola, claims to have seen no less than fifty unidentified flying objects since 1957, including a large metallic disk with a dome over Louisville airport. Another time he saw four saucers hovering above his house while a terrified cat, his hair standing up, jumped on him and held on to his clothes with all its claws. The disks floated slowly out of sight. Some of his other experiences involved telepathic communication.

The most interesting stories relate to Wilbert Smith's personality. Achzener assured Fred that he had witnessed some of Smith's experiments which produced "a five percent change in the force of gravity." Another time, during a meeting at Smith's house, three short whistles were heard on the FM radio. Smith's expression changed. He got up sud-

denly, excused himself and rushed out. He came back forty-five minutes later, his shoes covered with mud. The implication is that he had a secret UFO contact that night.

Another time Achzener had the thought that he'd like to smoke a cigarette. Smith, apparently reading his mind, suddenly told him, "let's go buy some cigarettes for you." They drove to a coffee shop and sat at the rear of the room, with Smith keeping his back to the wall. At one point Smith made a peculiar signal, which Achzener says could only have been *seen* by three other people: an old man, the waitress, and a short fellow about four feet tall who got up and drove off in an old car with California license plates.

"I wanted *you* to see *this* person," Smith *said*. "*That's why we* came here. He's an extraterrestrial."

Such are the stories that circulate among American ufologists. Achzener has given Fred two fragments of the "mysterious" metal found on the shore of the Saint Lawrence River and highly prized by Smith. They turn out to be made of steel with a high manganese content. The analyses done in Canada are so clear-cut that Haslett has not bothered to reproduce them. The metal is certainly of ordinary industrial origin.

The discussion among us then took a mystical turn. Allen said he was envious of those witnesses who had psychic abilities and whose vision could reach into the astral plane. Bill interrupted him rudely: "Every time I've met a mystic I've met an uneducated slob." Don Hanlon, who could have nailed Bill down with a few words on this topic, wisely refrained from saying anything.

I like this group very much. Fred is *refined* and clever, a walking scientific encyclopedia. Jarel Haslett is a clear-thinking physicist. Hynek is both secret and transparent, subtle and irritating, an adventurer of the mind but *not* a man of action, a scientist patiently looking for the limitations of science itself.

"It's hard to give a definition of UFOs that doesn't also embrace ghosts, or the Virgin Mary," I pointed out in response *to* Bill's comments. He didn't believe me, so I pulled down from the shelf the book by Delaunay, *A Woman Clothed with the Sun*, and I read to them the record of the 1868 Knock apparition in Ireland.

"How do we know all this happened?" asked Bill. "Do the witnesses even exist?"

Many scientists would react as he did. They misunderstand the scope of the mystery.

Fred has changed since he has seen "his" UFO. He is getting more assertive in his opinions. He even raises his voice occasionally when Bill threatens to cut him off.

**Chicago. Wednesday 31 May 1967.**

Yesterday was Memorial Day. I spent the afternoon at Allen's house classifying the files, and I proposed that we meet with my computation center colleagues to draft a joint proposal. They have agreed to let me bring all the files to my office on campus this summer for an in-depth reorganization in preparation for the *next* step in our research, which we hope could be funded by the Air Force. Such a project, if it took off, could make me stay in the United States. We're not going to give up easily.

This evening Allen asked me to fly to Concord, New Hampshire, as an observer in a contact experiment Betty Hill wants to conduct with John Fuller, Dr. Simon and a man named Hohmann, who is an IBM engineer. Betty believes she has become a "transducer." She thinks she can make a flying saucer appear in the sky and land.

**Chicago. Tuesday 6 June 1967.**

Lightning war in the Middle East. The Egyptians want to "throw the Israeli rascals into the sea, exterminating them to the last man." The Israeli respond in kind. Everywhere soldiers are in control of this wretched planet. At the moment there is no danger of a world war. Nonetheless the feeling of helplessness is inescapable. How can man have a chance in the long run? How can rational thought be a significant factor in such a world? The kind of *wisdom* generated by the best minds is nothing but a moldy deposit growing on the hull of the great ship of State: something only fit to be scraped off in the next port of call. And scientists are pawns to be moved around by governments, only useful for inventing atom bombs.

I still must believe we can survive. Yet if the battle had turned against Israel yesterday I probably would not have the luxury to think about *all* this because the war could already have escalated and the greatest conflict in the history of the planet might be in progress. Will men of the future

try to understand our time, our folly? I imagine a subtle being who looks over my shoulder as I write, an unseen presence. Together we explore the flow of time. He assures me that there will be a future, whose distant echoes fascinate me.

The room is warm and humid. We are working hard, thinking of progress. And we are at the mercy of some Colonel who sits on the hatch of a tank, raising *columns of dust* in a forgotten desert between two dry seas. Life is in our throats, bitter, full to the point of vomit. Who is talking about progress? We have not even begun to glimpse the horrors of which we are capable. In a few years Israel and the Arabs, India and the Pakistanis, will have nuclear bombs. Then what?

Every minute a jet flies over to land at O'Hare. In the heavy atmosphere its engines leave a long and strident scream. There is a little bird down there in a hole in the wall. I wonder why he stays in town.

At the library this afternoon I searched for references on apparitions and demonology. I unearthed a microfilm of a book dated 1577. The original is kept at the British Museum:

A Strange and Terrible Wunder wrought very late in the parish church of Bongay, a Town of no great distance from the Citie of Norwich, namely the Four of this August, in ye yeere of our lord 1577, in a great tempest of violent raine, lightning, and thunder, the like whereof hath been feldome feene.

With the appearance of an horrible fhaped thing, fenfibly perceived of the people then and there affembled. Drawen into a plain method according to the written coppye, by Abraham Fleming.

A big black crazy dog rushed into the church in the midst of a horrible storm and broke the necks of several parishioners. As he jumped around in the nave among the flashes and thunder, lightning destroyed the bell tower. The author appealed to Divine Providence, to the wonderful anger of God. Poor Abraham Fleming, how I understand you! I would have done the same thing.

The sky of the astronomers is dead. I am not expecting that saucers will come down with the answers for us, as Betty Hill does.

The streets of Chicago are dead, too. And I have no illusions about what I will find in France if I return there. It is of the fresh and intimate

streets of unknown lands that I dream right now, clear skies after the rain . . . but research is my world, I belong where science is made. I feel no indulgence for the sellers of abstraction, for the philosophers who do not build things, real things, with their own two hands.

**Chicago. Friday 9 June 1967.**

Bob O'Keefe, my Psychology Department friend at Northwestern, is now using his students as subjects in an interesting case study on *assimilation and contrast*. He requested that I supply him with all the reviews of *Anatomy* and he has now asked his class to rate each piece according to two parameters: first, the position of the reviewer on a scale of no belief to total belief in UFOs; second, the position that the reviewer *believed I occupied* on the same scale after reading my book.

The results show a remarkable bias among the reviewers, with evidence for the classic "*either with me or against me*" delusion: the strongly pro or con reviewers evaluate my neutral or even moderate positions as far closer or far more in contrast to their own viewpoints than they really are, ascribing to me some extreme ideas that are nowhere in my book!

Based on no less than 125 reviews, Bob and his students have found almost a perfect correspondence between the bias of the reviewer and the position he or she *ascribes to me*. This psychological bias in *judging* was known to exist among cretins and political extremists. O'Keefe has now found it among the most prestigious reviewers of the major papers, who claim to be impartial intellects bent only on informing the public.

This morning Allen told me he had two reasons to be happy: his son Scott has just passed his Ph.D. preliminary examination at MIT, and his research proposal on supernovae has been funded. But he also has one subject of bitterness. When he went to Dayton on "Wednesday there was an immediate scene between him and Quintanilla. He asked to see the Project Grudge report. There was only one copy at Wright Field, and it was in very poor condition. He was told he could have a copy made but the Air Force would charge him a dollar a page: there are some 500 pages. Angry that he could not even have a copy of a report he had largely written himself, after serving Blue Book for twenty years, poor Dr. Hynek actually sat down and started transcribing the introduction by hand.

Later Hynek happened to see an interesting sighting file lying on a desk. He asked a secretary if he could take some notes from it. Quin-

tanilla jumped up: although the file was neither secret nor even confidential, it came from OSI (the Office of Special Investigations) and bore the words "Official Use Only." Quintanilla insisted that nothing could be copied because "information channels could not be compromised." Hynek got mad and mentioned all the rumors we had relayed to him about Blue Book being nothing but a cover, a smokescreen. Are the real UFO investigations run secretly under OSI, whose records are *classified*? This made Quintanilla even more furious. Hynek almost resigned on the spot.

Quintanilla finally gave in, taking the first step to improve the relationship; he invited Hynek for a drink in town, promising that he would extract the pertinent information from the case file. Later, when Hynek was alone with Morley, the lieutenant told him it was a fact that *he just didn't see all the reports*. Quintanilla frequently tells him: "We won't send this one to Hynek, it's better if he doesn't know about this particular sighting."

I am very interested in this turn of events. Hynek is more puzzled than ever. We already knew that Blue Book was far from getting all the official reports. For instance, if some unknown object is seen by a pilot or by a radar operator the data generally goes to NORAD, not to the Air Force. And when a case does come to an Air Force base it is only forwarded to Dayton if no "local explanation" is found. This obviously leaves the door open to any manipulation.

At lunch I stressed to Hynek that he was in a precarious position indeed. In fact McDonald is now on solid ground when he accuses him of being a willing strawman for the Air Force. Allen has accepted the job of scientific watchdog with Blue Book. ***If it turns out that the information he sees is deliberately biased, censored or distorted by the military, it is the entire research community of the United States which is being betrayed on a potentially crucial topic.*** The fact that the academic community may *want* to be misled doesn't matter: if Hynek knows about the manipulation and says nothing he is automatically an accomplice.

Later the same day.

A melodramatic letter has arrived from Bob Hohmann in preparation for our weekend experiment with the Hills. It talks about my role in what he calls *Phase Two*: that's when I am supposed to take command of

the interaction with the Aliens when they land. Betty Hill has written to him:

I follow your suggestions regarding the message and I hope that the next manifestation will take place closer than that of Saturday evening in Kingston. Very close, very clear and just above the trees.

Although the letter is signed "Betty and Barney" it is entirely written in the first person. Clearly Betty is the dominant mind here. It is this interaction we want to better understand by going to New England.

Janine is carefully reading Fuller's book *The Interrupted Journey*.<sup>14</sup> She finds in it some important facts that I had missed. The whole story of the abduction of the Hills by saucer people didn't originate with Dr. Simon at all but with this man Hohmann himself. This is the way it happened: when they came home to Portsmouth after their sighting of 19 September 1961 Betty first mentioned the incident to her sister Janet, who in turn spoke to the former chief of police of Newton. They were told to contact Pease Air Force Base, where practically nothing was done.

Two days later Betty found one of Keyhoe's books in the library. She wrote to him and the case was forwarded to Webb, a NICAP consultant, by Richard Hall, about one month after the sighting. In the meantime Robert Hohmann and C.D.Jackson had come to Washington where they had lunch with Keyhoe. The case was brought up. On November 3rd they wrote to the Hills and they met with them on November 25th, interviewing them from noon to midnight. They even asked them if they used fertilizers for their trees, because they thought saucer pilots might want to steal the fertilizer. Hohmann and Jackson also told the Hills there might well be life on Alpha Centauri. It is this team of self-styled scientific detectives who discovered that some sixty kilometers and some two hours were "missing" from the couple's itinerary. A friend of the Hills who attended that meeting suggested hypnosis.

On 25 March 1962 Betty and Barney consulted Dr. Quirke, of Georgetown. In the Summer of 1962 they saw Dr. Stephens in Exeter and he sent them to Dr. Benjamin Simon late in 1963. In other words, Simon didn't actually start hypnotizing the Hills until two years after the sighting.

**Chicago. Monday 12 June 1967.**

We have returned from New Hampshire without having met any extraterrestrials. When we stepped off the plane in Boston we found a heat wave in full swing and John Fuller waiting for us. He was jovial, ebullient, relaxed. We liked him immediately. We climbed into his Land Rover and we drove to Arlington, joking half in French and half in English.

We spent a long time stuck in the dreary Boston traffic, looking at run-down wooden houses and narrow dusty streets where sweating humans sought every chance to rest in the shade or to just lie there, waiting for the sun to go away.

The mansions of the Boston upper crust are located on the hill of Arlington. Dr. Simon's two big, mean African dogs made an effort to come after us, panted, and decided they were too tired to attack after all. The doctor was in the dining room in the company of two older women who immediately offered to feed us. We declined. Simon wore no shirt, and the only thing I really remember about him is a huge bulging stomach that bounced around the room ahead of him. He introduced us to various people and to his son the doctor. I explained why Hynek couldn't make the trip, and we just waited around for a signal that it was time to leave for Kingston.

Eventually John Fuller looked at his watch and moved towards his car. We drove through Woburn and Haverhill but we could still feel the pulse of Boston. The very notion of flying saucers ever landing in Massachusetts seemed preposterous.

A little road twisted among trees and we found the house. We turned and drove down to the pasture where Hohmann and his entourage had already pitched their tents. The sweltering heat moistened everything.

"There will not be any relief tonight," said John.

We changed to lighter clothes, but the mosquitoes attacked as soon as they saw our short-sleeve shirts. Ignoring them we walked over to introduce ourselves to Betty, Barney and their dog Delsey. We found Hohmann in the midst of a grand speech about the telepathic powers of Australian Aborigines.

Dr. Simons Cadillac arrived. I took Hohmann aside: "What are we doing here?" I asked. "What the hell do you mean by *Phase Two*? Or *Phase One* for that matter?"



"Betty has become a transducer when she was in the saucer," he told me flatly.

"What's a transducer?" I asked. Surely he didn't mean those little hardware boxes we put on telephone circuits.

"You know, she can communicate with Them, she has acquired that capability. The Humanoids."

He firmly believed *They* needed something from us. What exactly? That wasn't clear. But we did need something from *Them*: the revelation of some new truths.

"For the last ten days Betty has been sending messages asking Them to come back. A numeric code has been established to indicate the contact date: June 10th."

All that was perfectly childish, like the folding table in the middle of the field and the large chalk circle where we were supposed to gather to demonstrate our goodwill to the Aliens. Hohmann wanted Betty to broadcast another date when *They* would come back and show their craft to millions of people. It was like a bad movie. But what if something did happen? We came this far, let's keep an open mind, I told Janine.

I had brought a Geiger counter and a good little telescope. Hohmann's circle was drawn at the bottom of the field, where we had the least exposure to the sky. He was willing to help me move the table uphill. I oriented myself and I began a temperature log. I went to a phone and I called Allen, promising to alert him if anything did happen, and we went back down to the camp for dinner.

Darkness came at last. Group conversation returned to lofty cosmic truths while Delsey licked noisily what was left of the food. I set up the Questar. Time passed, bringing Mars into view, followed by Arcturus and Vega. The mosquitoes danced and everything got wet. Simon went back to Boston. Fuller couldn't stand still for five minutes, he had to drive to Exeter "to see if there was anything new there." What could possibly be new in Exeter?

I sat in the grass with Janine, feeling thankful for her presence but guilty for having dragged her along. The sky remained clear. Every hour I noted the temperature. The Geiger counter clicked away in the darkness. Artificial satellites crossed the sky, then a meteor and lots of lightning bugs: at every such incident Betty jumped up excitedly. But if I had met a group of little aliens on a lonely road, and they had dragged me inside

their craft, perhaps I would be as inclined as she was to see flying saucers everywhere.

At three o'clock in the morning everything was covered with dew. I folded up the telescope and we went to bed in a trailer behind the house. Three hours of uneasy sleep. Instant coffee. The people in Hohmann's group left. At last we had a chance to talk quietly to Betty and to Barney. They were nice and warm people once they didn't have to play transducers in front of a bunch of crazy scientists any more. They took us to the lake shore and later they let us listen to their recordings made under hypnosis by Dr. Simon, certainly the highlight of the whole trip. Why the fear, the sheer terror in their voices, unless the abduction was genuine? But at what level? I remember Simon's remark yesterday when I had asked him:

"Was their experience real?"

"No question. It was real to them."

"Doctor, let me ask it another way. If you and I had been driving with them when this thing happened, would we have seen a flying saucer by the side of the road and five little Aliens opening the car doors?"

"I have no way to tell you that. All I can say is, the abduction is part of their reality."

Jackson drove us back to Boston and we flew home, very tired. It was just as hot and muggy in Chicago as it had been in Boston. Olivier was sweating and flew into an uncontrollable rage as soon as we came into the apartment. I had to grab him and force him under the shower to calm him down. We took a shower after him, and I went to bed, my head aching.

This evening I am writing my report on the Hill case for Hynek. We have had a good night, a slow day. Olivier is babbling away, telling us long nice stories. We have learned a few things in Kingston but we know nothing more about the alternate world which Barney and Betty may have briefly glimpsed. They are sincere people facing a problem that is beyond their grasp. Barney is an intelligent, balanced man who understands the unique nature of his situation. But Hohmann has clearly planted many strange ideas into the couple's mind.

Simon is a sly old practitioner, an empiricist of vast experience. In my opinion he doesn't really care whether or not they have been abducted: that is not relevant to his psychiatric assessment. He places no value

in the time loss experience: "When you travel you don't pay attention to the time it is," he told us. "Also, remember these people were lost."

The whole business about Betty being a transducer began when she said as a joke: "The Aliens ought to pay a little visit to Mrs. So-and-so," and a few weeks later, sure enough, a strange light was seen over her house. Everybody became convinced that the extraterrestrials were picking up Betty's thoughts and acting on them. She claims to have seen a saucer at treetop level again, just the other day.

Janine, who has not forgotten the lessons she took from Piaget on child psychology, took the time to talk to the kids. She asked them what they thought the light in question was, above Mrs. So-and-so's house. They answered it was just a light, a tiny pinpoint source, "it could have been anything."

From Woburn to Portsmouth every bush, every telephone pole has become a saucer sanctuary since John Fuller's best-seller has appeared. Here is where Betty stood when she sent her first message. Here she saw "one star too many in the Big Dipper." It's like visiting the Holy Land. And there is a new social pecking order in Kingston. It's not how big your house is or the color of your Cadillac any more, it's how many saucers you have contacted. Betty and her sister Janet are the supreme arbiters.

Not enough has been said about Janet. She saw her first UFO in 1956 and she has been seeing them ever since. A few weeks ago a mysterious light crossed the road above her and she felt "the ray."

"How did that feel?" I asked her.

"Like when they take an X ray at the hospital."

"You can't feel an X ray at the hospital," protested Barney.

"Well, maybe you can't but I sure can," was the peremptory answer.

Even when we recognize that Betty has been influenced by the believers and that Janet and others now recognize a UFO in every starry light, the original 1961 sighting does remain unexplained. The hypnosis tapes took us back to that night in the White Mountains, on 19 September. Their speech found a deep, tragic, Wagnerian tone:

"It's big, GOD! It's BIG! What is it? WHAT IS IT?"

A scream of sheer horror, and Barney's voice:

"I've got to get my gun!"

From the slanting craft with its double row of illuminated windows a light filters towards his eyes, he perceives a voice:

"Don't move. Stay where you are. Don't move."

There are about ten occupants who work on wall panels. A single one stands at the window and stares at Barney. He manages to break away, running to the car where Betty is crying and where the dog is cowering under the seat.

Later they turn into a secondary road and *it happens*: the craft is blocking the way. A series of fast beeps vibrate the whole car. At that point, as John Fuller's book describes it, Betty reproaches herself for being so ignorant:

"How stupid I am! What if it was Morse code..."

Her voice abruptly changes. The Bostonian accent vanishes, replaced by a flat, deep, grave voice. No hesitation. Short sentences. Factual.

"They come towards the car. There are three of them. Two others behind them. They open the door."

During one of the last hypnosis sessions Betty states that the one who walked ahead of the group held something pointed towards her.

"How big was it?" I asked.

"It could have been a gun," she replied. "But it could just as easily have been a cigar."

That last detail reminds me of Masse, paralyzed at Valensole two years ago by a similar device a little man had taken from his belt.

I think something did happen to the Hills. But what was it? Should we believe the story of interaction with the operators inside the saucer, the ludicrous (or symbolic) medical examination that comes straight out of a horror film? Have the true facts of their abduction been overlaid by a false memory? Their adventure suggests a grave and terrible mystery.

### **Chicago. Tuesday 13 June 1967.**

The director of the computation center at the University of California in San Diego has called me again, and again I have declined his offer to join his research staff. I would lie if I didn't confess I am tempted. The beaches, the mountains, a happy active life for Janine and our children: yes, I want to take part fully in the novel, exciting frontiers of our creative time. But I also long for old experiences: good ancient books, rain falling over narrow cobblestone-paved streets, although I fear real cataclysms are coming to Europe, to sweep away the sand castles people have erected to protect their privileges.



Setting up a double refractor in Pontoise, about 1957.



Janine in Texas, with Antoinette de Vaucouleurs, 1963.



Dearborn Observatory in Evanston, December 1963.



My mother in Pontoise, April 1964.



Photo © 1964, *Denver Post*

Dr. J. Allen Hynek, mid-1964.



Working on the research for *Challenge to Science*, mid-1964.



Discussing the capabilities of *Altair* early in 1966.



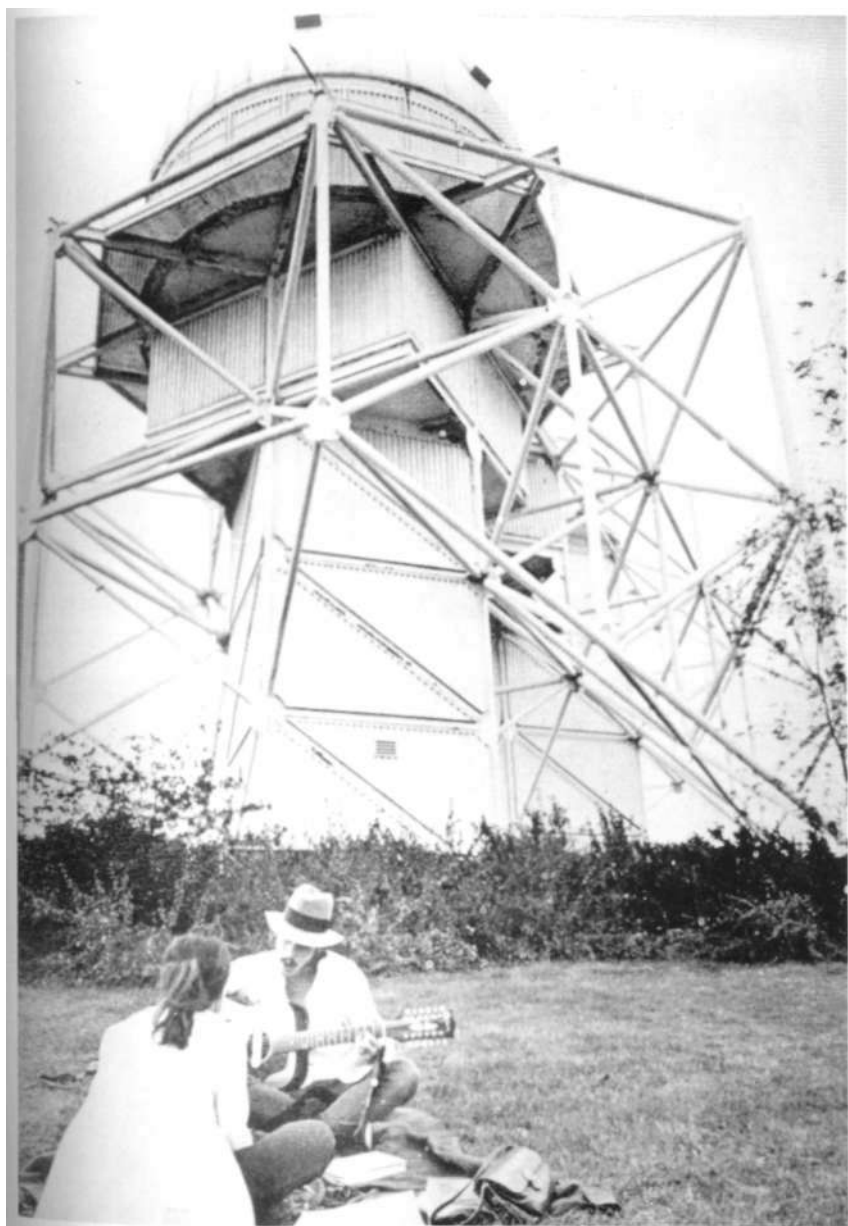


Photo © 1966, *The Daily Northwestern*

The new observatory at Northwestern.



In New Hampshire with Betty and Barney Hill, June 1967.



With Olivier at Saint-Germain-en-Laye, April 1968.



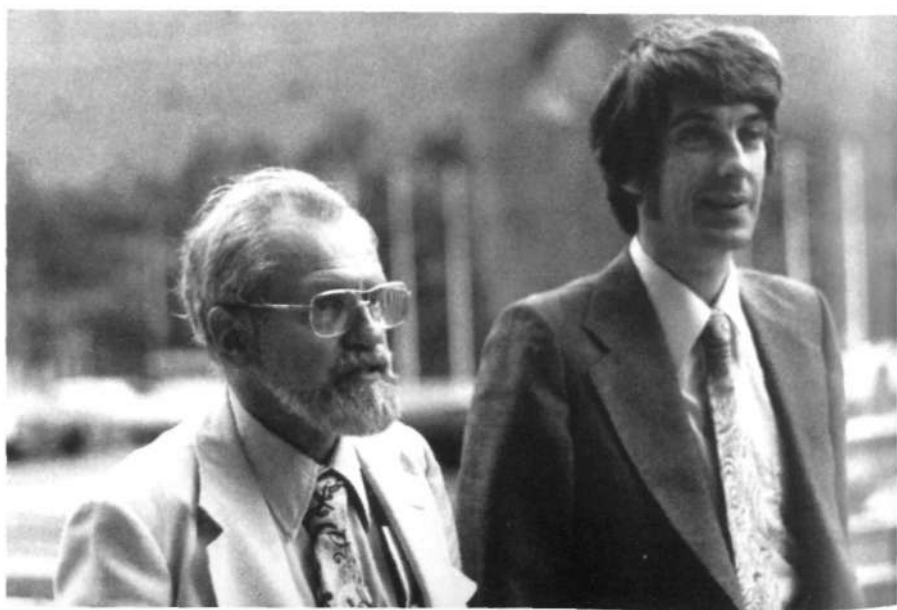
American delegates at a computer standards meeting, Washington, 1969.



With Aimé Michel at Paris Observatory, October 1969.



With my children in California, 1971.



With Allen Hynek in New York, 1978.

I read over the early pages of this Journal and I wonder how on earth I can think of returning to Paris: I was so unspeakably happy to leave in 1962, escaping from all the idea-killers, from the Paris intellectual slaughterhouse where the pundits of *Le Monde* were in control, mocking anything new, anything out of the ordinary.... My generation should have shaken all that a long time ago.

There is a genuine deep current that carries the world towards its future, from the convulsions of Mao's China to the secret California where everything is crashing through the old barriers. I feel this current carrying me forward, even if the heat weighs too heavily over Chicago tonight, even if these hasty words are too partial, and the dust that blows from the big empty Middle West too stifling.

# 15

## Chicago. Sunday 18 June 1967.

Step by slow, painstaking step, I am uncovering the real story. I made an important discovery yesterday. Hynek is away in Canada but he has given me a key to his house. I went there during the week to take the Air Force files away so I could go on sorting them. And I found something that dramatically changes the whole landscape.

Since the Marsh Gas episode Allen has been unable to keep up with the volume of new data. Copies of the files Quintanilla sends him end up in the small room where he works on the second floor of his pink house on Ridge Avenue. Papers are piling up everywhere: in the drawers, on the chairs, on top of the wardrobe. For the last few months he has been trying to put some order into this mess, but it is a hopeless task, so we finally agreed that I would transfer everything into my office during his absence to straighten out the files over the summer. Thus we hope we can sift out the garbage, extract the most credible cases and prepare a strong new research proposal in the Fall.

What a challenge! I sat alone in Allen's room and I contemplated the disaster. "If I don't do this, nobody will...." I thought as I started putting all the files dated before 1964 into cardboard boxes. I carried them to

the Buick. I drove over to the computation center and I began sorting. I have been sorting ever since. First I set aside the statistical summaries which cluttered the files; I eliminated numerous duplicates; I replaced the old stained folders with clean new ones, neatly labeled with places and dates.

The worst section of the files concerned the history of the Air Force projects themselves, from Sign and Grudge to Blue Book. Hynek had misplaced many of these documents. And it is in that section that I found a letter which is especially remarkable because of the new light it throws on the key period of the Robertson Panel and of *Report #14*. It is stamped in red ink "SECRET—Security Information." It is dated 9 January 1953. It is signed by a man I will call Pentacle.<sup>15</sup> It is addressed to Miles E. Coll at Wright Patterson Air Force Base for transmittal to Captain Ruppelt. It begins with the statement that the document contains a recommendation to ATIC regarding future methods of handling the UFO problem, based on experience in analyzing *several thousands of reports*.

This opening paragraph clearly establishes the fact that **prior to the top-level 1953 Robertson Panel meeting somebody had actually analyzed thousands of UFO cases on behalf of the United States Government.**

After pointing out that many of the reports contain insufficient information, and that it would be highly desirable to obtain "reliable physical data," the letter goes on with the blunt advice to cancel or at least postpone the meetings of the Robertson Panel until the full results of the analysis could be made available, a very natural recommendation. Failing this, they wanted to have a formal agreement between ATIC and the staff of Project Stork as to "what can and what cannot be discussed" at the Washington meeting with the five leading scientists.

In other words, the representatives of this top-level research group were against convening the Robertson Panel! But what was Project Stork? Whatever it was, its members were doing some excellent scientific thinking: They had noticed that the distribution of cases over the United States was not uniform, and this led them to identify areas of high reporting probability, which they proposed to set up as experimental areas.

In such areas they wanted to place observation posts with complete visual skywatch, radar and photographic coverage, and all other instru-

ments "necessary or helpful in obtaining positive and reliable data on everything in the air." They added, even more ominously, that many different types of aerial activity should be secretly and purposefully scheduled within the area.

What these people were recommending was nothing less than a carefully calibrated and monitored simulation of an entire UFO wave.

For whom did Pentacle work? Did the proposed experiment take place? Who were these people who calmly sat around the table with the CIA and the Air Force and who, many years before us, understood the need to acquire second-generation data?

Their plan for getting such data covertly makes perfect sense. But what this document shows is that the scientific community has been led down a primrose path, beginning with the Robertson Panel and its group of prestigious physicists.

Another team of analysts had already reached the point where they could form some scientific hypotheses about the phenomenon and they were ready to test them. Yet the high-level advisers who reviewed the data were not briefed on that part of the Air Force's research efforts, even though their own recommendations were also classified at the time. What was going on? What kind of a game was being played?

Hynek once assured me that if it ever turned out that a secret study had been conducted, the American public would raise an unbelievable stink against the military and Intelligence community. It would be an outrage, he said, an insult to the whole country, not to mention a violation of the most cherished American principles of democracy. There would be an uproar in Congress, editorials in the major scientific magazines, immediate demands for sanctions. This memorandum does not prove that it has gone that far. And yet . . .

Let us go back to 1953: the Intelligence agencies have determined that unknown objects are flying over the United States. If these are controlled machines they are far beyond anything we have. Public opinion demands some action. What could be simpler than assembling a panel of scientists? Perhaps not the best informed, but the most prestigious. They are shown a sample of the reports, pre-selected by the Air Force. They find no reason, of course, to revise the current edifice of science on the basis of what little they are shown. And once the panel has been disbanded and Public opinion quieted, what a wonderful opportunity for the military to

resume its research in secret, with its own scientists, its own laboratories. ... If such a research project exists it certainly does not need the Blue Book data. It could operate independently, at a much higher level.

I cannot discuss any of this with Aime Michel or anyone else in France, and certainly not with Rocard, since communications with him have proven to be completely one-sided. I do not even dare copy the whole memo into this Journal. Yet this document, if it were published, would cause an even bigger uproar among foreign scientists than among Americans: it would prove the devious nature of the statements made by the Pentagon all these years about the non-existence of UFOs. These official statements have been taken as Gospel truth by most foreign nations, discouraging their own efforts to start independent appraisals of the situation.

Olivier just came into the room to kiss me good night. I see our future as a fine, quiet beach at low tide, about to be covered by new waves, new insights, and a lot of turbulent waves.

### **Chicago. Wednesday 21 June 1967.**

Aime Michel recently proposed a new idea to Clerouin, who is now a Colonel: Why not bring me back to France under some new structure so I could work seriously on the UFO problem with a small group of assistants? Clerouin appeared excited with the idea and promised to bring it up with his superiors. He gave no sign of life for a whole month, then he had dinner again with Michel and with his friend Latappie, and he dropped the following harangue on them:

"We in the French Air Force, we are sick and tired of hearing all those unjustified attacks against the USAF experts who are impeccable scientists. Their investigations are top-notch\_\_\_\_"

"Wait a minute," said Aime Michel, "Do you think we're stupid?"

The Colonel would not listen.

"There is another point which is even more important. Why should we embark on trying to do a job nobody is asking us to do? The Pentagon is perfectly right to downplay the whole thing. **Society is working just fine the way it is, why should we encourage all this saucer business?** It might stir up social repercussions we couldn't control. So the longer we wait the better it is for everybody. Let the scientists do something about it if they want to."



Aime Michel believes that Cl  rouin didn't go to his French superiors at all but directly to his American counterparts. This is an area where the USAF continues to have control over what the French services do, in spite of all the lofty Gaullist talk about independence and *grandeur*. And oddly enough, the leftist and Marxist "rationalists" who are a majority among French scientists are eager to believe whatever the Pentagon says about the futility of the phenomenon: it matches their expectations. There is little we can do against such a powerful coalition.

McDonald has just sent me a statement he read before the United Nations committee concerned with outer space affairs. It remains to be seen whether his highly visible approach will serve the goals he is hoping to reach. It may simply backfire in his face.

This morning I went back to Allen's house. At the top of the stairs there is a little room which remains locked, and the key is at a certain nail. It is in that room that he prepares his lectures and works on his files. I returned all the documents on Blue Book history, which I have now separated from the sighting files. They are too sensitive to remain in my office on campus. I picked up the loose files I had been unable to carry away earlier. I spent most of the afternoon sorting through those papers until I was sick of them.

### **Chicago. Sunday 25 June 1967.**

When I met Hynek for the first time four years ago we discussed Aime Michel's book, *Flying Saucers and the Straight-Line Mystery*.

"I can't believe that if such events had really taken place in France your scientists would have done nothing," he said. "Surely a special committee would have been appointed, investigations would have been conducted. ..."

Well, just look at us. For the last several months a wave has been in progress here. And there happens to be in the U.S. a fully funded scientific committee with the highest authority, created specially to study the problem. What does that committee do? It argues, it dreams, it theorizes. Its top administrator Bob Low is getting ready to play the tourist in *Lrague*. Hynek will meet him there: *Le Congres s'amuse*. The scientists are having fun while the poor witnesses cry for help.

Fred is equally disgusted at the lack of response to the sightings. Yet at Northwestern, when Ben Mittman approached the Dean again with the

idea of a new research proposal on UFOs, he got the same old answer: "Its all bullshit."

In Moscow Kazantsev and his secretary Tikhonov have made new contacts among Soviet scientists. The Russians love organization above everything else. They have turned bureaucracy into an art form. So I am not surprised when Kazantsev announces:

You should know that Zigel, one of my close friends, has taken the initiative of forming a committee for the study of UFOs within the Central House for Aviation and Cosmonautics of the USSR. Air Force General Stoliarov was elected President of the committee. Tikhonov is secretary. Zigel is vice-president.

Kazantsev says that the excuse for the creation of this group was my suggestion for an international meeting under the aegis of the U.N. Clearly a committee is required in order to elect Soviet representatives, should the international meeting actually materialize! Hynek has given me a copy of a letter he has received from Abdel Ghani, the head of the space affairs group at the U.N., to forward it to Kazantsev. But I have little hope that anything concrete will happen in New York.

### **Chicago. Friday 30 June 1967.**

This has been a strange, agonizing week. I keep thinking about the Pentacle letter. Fred is the only person I have taken into my confidence.

"There cannot be any doubt any more," he said after seeing the document. "When they talk about *what can and what cannot be discussed in Washington* they are clearly referring to a manipulation of the Robertson Panel."

Since I found this letter I have been going back through the Air Force files with a fine-tooth comb. I don't want to jump to unsupported conclusions. The first step is *to* build a statistical index of all *the* data, *so* I now spend several hours a day punching cards: dates, places, names of witnesses for the 11,000 cases we have assembled. A trained operator could do this in a much shorter time than I can, but I have no money to hire anybody. Besides I want *to* fix these names in my memory. Statistics are fine, but the real work is what goes on inside the brain. And I'm not bad with a keypunch machine.

In all this it is becoming very apparent that Allen has been much too

timid. In many cases he even believed in the explanations he was producing. For eighteen years he was an amused, open-minded spectator, but he kept his thoughts to himself. "I'm not a fighter," he often tells me by way of apology. Still, he could have taken a more active role while remaining behind the scenes. As scientific consultant he could have assembled the most solid cases and he could have presented them privately to selected colleagues. But he also tells me:

"All those years \_\_\_ All right, so I didn't speak up. But I kept expecting that a really good case would show up, don't you see? Dammit, if this phenomenon represents a genuine new physical effect, wouldn't you think that some day it would manifest in an absolutely undeniable fashion, leaving the kind of evidence a scientist could take back into the lab and study? Yet such a case never happened."

He tried to remain friendly both with the military goat and with the academic cabbage, antagonizing no one. He is too clever to allow himself to be chewed up along with the cabbage, but he certainly will not grab the goat by the horns.

To his defense it appears more *and* more, as I *read* these *documents*, that he is right in saying he never had a chance against the big guns of the Air Force and the Robertson Panel, that any effort to put the phenomenon before the academic community would have been ignored: the topic was not mature, the timing was wrong. My *guess is* that Jim McDonald will not get very far either.

Hynek should have taken the time to re-analyze his files. If he had, he would have noticed some sinister incongruities. The New Guinea photographs taken by Drury *in 1953* are a case in point. I had come across a clipping indicating that the Australian military had forwarded the film to the U.S. Air Force.

"That's impossible," said Hynek. "The pictures are not at Wright Field in Dayton, I have never heard of the case being reported to Project Blue Book."

Yet I have just punched a card about the Drury film, based on the Air Force's own index. They even have a formal conclusion about the case: "Insufficient Data."

But it is preceded by an asterisk, which means that it is classified.

According to the Australian researchers who have seen it, the film shows an object emerging from a cloud and making a perfect double

right-angle maneuver at high speed. This is exactly the kind of photographic data which could prove convincing to the scientific community. The altitude of the cloud can be determined with good approximation, hence the size, distance and luminosity of the object can be computed. The accelerations can be calculated too. Is it for that reason that the Air Force has classified the film? Has someone been working on this data in secret since 1953? Where would this research be going on today? What would be its goals? Could the whole problem have been solved by now?

Three years before the Drury film, on 15 August 1950 to be exact, Mr. Nicholas Mariana shot a movie showing two UFOs in flight. He submitted it to the Air Force which returned it, he claims, after clipping out the first thirty frames, where a dark rotating line was said to be visible on the objects. The Robertson Panel thought they were only airplanes, but they never saw the beginning of the film where greater detail was noticeable.

Now I am reading a letter to ATIC signed by A.H. Rochlen, public relations vice-president, summarizing an independent analysis that was done by the Douglas Aircraft Corporation "within the framework of a study ... having to do with unconventional propulsion methods." The study was headed up by Dr. W B. Klemperer, chief engineer of their missile division. The report was distributed to R. M. Baker and A. M. Chop, the same man who was in touch with Keyhoe in the early years and eventually became the spokesman for Project Blue Book. What was he doing on the staff of Douglas and what was this study of "unconventional propulsion?"

In April 1964, on the very day when Hynek and I were visiting Wright Field, Lonnie Zamora saw the now-famous egg-shaped craft landing on four legs in the New Mexico desert. It bore an insignia which closely resembled the logo of Astropower, a company founded about 1961 as a subsidiary of Douglas, under the presidency of propulsion expert Y. C. Lee. I have now found a full-page, four-color advertisement for Astropower in the *Proceedings of the Institute of Radio Engineers*, special issue on computers, dated January 1961. Astropower "offers its services in research and development of advanced space propulsion systems to government contractors and agencies." Its program includes nuclear, chemical and electrical propulsion systems, solid-state elements and energy conversion. Mr. Lee seeks to fill key positions at Astropower with "scientists

## PENTACLE

having advanced degrees in plasma physics, nuclear physics, and thermodynamics."

Later I found another ad in the monthly *Notices* of the American Mathematical Society, where the same company was seeking to recruit statisticians and probability experts wishing to go into artificial intelligence development.

Was the Socorro object an advanced prototype of human technology rather than a genuine UFO? Have the government and its contractors been conducting secret research to study and emulate the characteristics of these objects since 1953, while it used Hynek and Blue Book as a screen to keep everyone else distracted?

All we have to do if we want to create a fine uproar is to show the Pentacle letter to McDonald and to NICAP. This would stir up a lot of trouble but it would solve nothing, of course. It would only generate more confusion. I even wonder if I should tell Allen himself about this document or simply put it back in his files where I found it, and forget that I ever saw it.

### **Chicago. Saturday 1 July 1967.**

A correspondent of mine in Missouri announces to me the death of Frank Edwards<sup>16</sup> due to a heart attack. Allen's secretary tells me that he already knows about it: a lecture agency has contacted him to fill out Edwards' public appearance schedule. Hynek immediately jumped up and agreed to do it. Yet a few days before going off to Canada he was telling Fred and me:

"It's finally over, all those lectures: only two more and I give all that up. I'm wasting too much time, the real research isn't getting done."

He was going to rest at his cabin at Blind River in Canada and start the study of the problem again from the beginning. Alas! The music starts playing, the spotlights are on, the drums roll, and there is Allen rushing onstage again.

### **Chicago. Sunday 2 July 1967.**

We have spent another weekend working on the Archives. Fred came to the computation center to familiarize himself with the system while Janine and I were punching cards. I told him about the Frank Edwards lecture tour.

"Allen is crazy to do it," he said. "He has become obsessed with publicity."

The time has come to seriously prepare our return to Europe. Yesterday I received my Doctor's diploma in the mail. I have not bothered to attend the graduation ceremonies. I have emptied all the drawers and cleaned house. I threw away most of the UFO magazines I had been amassing. I destroyed most of the obsolete orthoteny calculations, the computer programs, the charts. There are only two interesting research directions now: first, the long-term history of the phenomenon, which will tell us how and under what conditions it recurs through time. And second, the study of the landings and the humanoids, possibly leading to hard physical data. The kind of statistical analysis we have been doing can only be useful in providing a framework, in testing broad hypotheses. It is time to move into more specific directions.

The Robertson-Alvarez-Page Panel was manipulated by the American secret services. Nobel Prize winners can be fooled like high school kids. Blue Book is a smokescreen. I should use this knowledge. Or hide it.

### **Chicago. Monday 3 July 1967.**

I have decided to go through Blue Book's archives again with a microscope. This afternoon I went back to Hynek's house and I read the Minutes of the Air Force Advisory Board, the report on the incredible spirit seance of 1959, and I tried to understand Allen's own role in all this. Will posterity take the side of McDonald against him? Will people understand Hynek's patience, his exceptional integrity? It is important to read his letters, his exchanges with Ruppelt and Gregory, his tense correspondence with Friend and Quintanilla, to understand how sharp was the knife edge on which he had to stay balanced.

In spite of my disagreement with his recent behavior I remain struck by one thing: of all the scientists who have considered this problem, Hynek is the only one who has understood the simple fact that the witnesses were not lying, that they did live through an extraordinary experience. Never does he fall into the skeptics' pedantic arrogance, or into Menzel's stubborn dogmatism.

**Chicago. Saturday 8 July 1967.**

On Thursday Janine's mother, her sister Annick and her son (our nephew Eric) joined us here. When we are not driving around Chicago and Evanston with them, I go on stubbornly punching statistics.

**Chicago. Monday 10 July 1967.**

Today I had lunch with Allen and I was finally able to ask him some hard questions. How is it possible that he never saw the Drury film? I showed him the Blue Book index where the entry is clearly marked. He didn't have an answer, except that the Air Force decided which cases its "scientific consultant" should be consulted on. His position didn't automatically give him access to all incoming UFO data.

"Jacques, that brings up an important point: the Air Force has sent me a new contract draft and I don't know, frankly, whether to sign it or not. I want you to read it and tell me what you think."

We ordered our meal and I read the document over while we were waiting for the food. The contract, I was surprised to read, was not really with the Air Force but with the Dodge Corporation, a subsidiary of McGraw-Hill.

"What's McGraw-Hill doing in the middle of all this?" I asked without trying to hide my bafflement. "Is that some sort of cut-out?"

"Oh, they are just contractors to the Foreign Technology Division," Hynek replied. "By working through companies like McGraw-Hill, which is a textbook publisher, it's easier for them to hire professors and scholars to conduct some Intelligence activities, keeping up with Soviet technology for example. Many academics would be nervous about saying they were working for the Foreign Technology Division."

The contract puts Hynek under the administrative supervision of a man named Sweeney, who is not a scientist. And it clearly specifies Hynek's task as *evaluating* the sightings of unknown objects to determine if they represent a danger for the security of the United States.

We find here the same contradiction that plagued the Robertson Panel: Scientists are not asked to make a scientific study but to give an answer in terms of a specific policy issue. Oranges are mixed up with apples and cherries. Such an approach is absurd. Only a deep analysis of the sightings, conducted along scientific lines by people filled with the sacred fire of

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research, could lead to a determination of the phenomenon's implications and potential threats or advantages to the nation.

The contract also shows that the project under which Hynek works with McGraw-Hill is not called Blue Book but Golden Eagle. We were going from discovery to discovery now.

"What was it called before?" I asked.

*"White Stork."*

A light bulb went on. The Pentacle letter mentioned Project Stork.

"Let's go all the way back to the days of the Robertson Panel," I asked Allen, who was wondering what I was driving at. "What was the project called then?"

"Let's see, at the time I was a consultant under Battelle Memorial Institute, you know, the large scientific think tank based in Ohio. They were responsible for Stork. Later the contract went to McGraw-Hill."

So Pentacle must indeed have worked at Battelle.

"Those details may be important. Why did the project change in structure?"

"The Battelle people were terribly anxious not to get their reputation mixed up with Blue Book. Ruppelt was always talking about our consultants back East', although Columbus, where Battelle is located, is only sixty miles by car East of Dayton!"

We both laughed.

"Who was Miles E. Coll at ATIC?"

"I don't remember hearing the name."

"What about the following people?" I named Pentacle and seven individuals whose initials appeared on his secret memo.

He replied without hesitation:

"They were all administrators or staffers with Project Stork, including the man who sent me on a clandestine survey of astronomers in 1952, to find out discreetly what my colleagues thought of UFOs.<sup>17</sup> Pentacle himself was a leader of Stork."

"What were their relations with the Robertson Panel?"

"Practically nil. Battelle wanted to remain outside all that."

"Could it be that Battelle had certain elements about which Robertson, Alvarez and the other luminaries were not briefed?"

"I don't know. I never saw any indication of it."

Therefore he didn't know he had a carbon of the secret Pentacle letter



in his own files, presumably since 1953. He may have forgotten it. Fourteen years is a long time. And as he said, he "wasn't looking under the bed at the time." He added pensively: "Perhaps I should have."

**Chicago. Tuesday 11 July 1967.**

Last evening Fred Beckman, Bill Powers, Jerry Haslett, Janine and I met at the computation center. Mary-Lou Armstrong and Dr. Levine, a young physicist from the University of Colorado, came to town, returning from an investigation on the East Coast.

"A luminous craft was reported to have landed in a field," Levine told us. He laughed: "It turns out the witnesses had only seen spark discharges between the poles of a transformer during a foggy night."

"How do you go about evaluating such a sighting?" asked Bill Powers.

"The interesting question, of course, is to what extent it supports a given hypothesis."

"What do you mean?"

"Well, for example, take the ETH, the Extra-Terrestrial Hypothesis," Levine continued. "We investigate to what extent does this observation support the ETH rather than the hypothesis that we are dealing with a natural effect or a purely psychological phenomenon."

"Since when does the scientific method call for fitting observational data into the slots of pre-conceived hypotheses?" I asked. Levine quickly realized he didn't have a leg to stand on, so he brought the question to a different level:

"The reality of the problem for us is that our committee has to give Congress an answer about the validity of the ETH. Is this hypothesis (a) proven, (b) acceptable in principle, (c) improbable or (d) rejected?"

"In other words, you're playing multiple-choice questions with Congress?" I remarked sarcastically. He shrugged:

"Politically, that's what the American public wants to know. All people care about is whether we're being visited by aliens or not."

The discussion turned to the coding system used in Boulder. Dr. Saunders has responsibility for it. They encode every report with a series of keywords like "Bright blinking green light," and an estimate of the value of the testimony and the strangeness of the sighting. This covers both the explained and the unexplained cases and it is an unholy mess. Someone has already cranked the computer through some statistics that

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prove that the unexplained cases are blue more often than the explained ones, at a statistically significant level! This is rank amateur work.

"What's the status *of* the Winnipeg injury case?" I asked Levine and Mary-Lou.

"Someone was sent there by Professor Condon. He has brought back some data."

"Did he visit the site in person?"

"No."

"What about the sounds recorded in the state of Washington?" asked Fred.

Mary-Lou expressed some surprise at the fact that he knew about Jim Wadsworth's investigation there and the small wave of other sightings in the area.

"Nobody said anything to us about a wave," she stated.

Fred told her the whole story. A local investigator had wanted to take Wadsworth to meet the other witnesses but all Wadsworth was thinking *of* was catching the *next* flight back *to* Boulder, *so* he missed most of the data.

Mary-Lou told us in so many words that the committee was simply not interested in field investigations. They are still pursuing the theory proposed by Wertheimer, one of their psychologists, who has "discovered" that if witnesses in a car report that their headlights *fail* when they are close to a UFO this is generally due to a perception phenomenon which is related to Gestalt psychology: these people have simply been blinded by globular lightning so that they don't realize that their headlights are still *on*. . . . We all laughed at this, although Jerry and Fred felt more anger than amusement.

"How come the light bulbs in the headlights have to be replaced in so many such incidents?" I asked Levine sarcastically. "Is there such a thing as a Gestalt voltage surge?"

He retreated hastily, speaking highly *of* "the great *experience*" *of* our group, and saying nice things like "it would be desirable for the committee to benefit from your *knowledge*. . . . We will certainly recommend that more detailed studies be undertaken\_\_\_More money is needed."

It was my turn to interrupt him.

"Why not start now? With the money you already have?"

"Our analysis is not over."

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"But you have just admitted that your study is going nowhere. Why continue to waste your time and taxpayers' resources?"

Levine didn't answer. Mary-Lou seemed rather amused. I went on, getting angry:

"I hate to tell you this, but your computer coding system has no meaning. Saunders' catalogue is worthless. You've drawn arbitrary lines through your data space, without any calibration. Selection biases are overwhelming the signal you are trying to extract."

They backed down. They had no idea of the real scope of the problem when they began the job, they said. Condon didn't give a damn whether or not the phenomenon was real. Bob Low was an opportunist. But none of that excuses botching up the job so badly.

Fred, Jerry, Janine and I went out to have something to eat. We discussed the Hill abduction case and we went home very tired.

### **Chicago. Wednesday 12 July 1967.**

A *newsman* from San Francisco has called me three times. He finally managed to get me on the line this morning.

"Please make a statement for Jim Dunbar's program."

"I've already said everything I had to say. You'll find it in my books."

"But new sightings have happened since then."

"The Condon group has responsibility for studying them."

"What's their objective?"

"To find out if the problem can be approached scientifically."

"Why should the American taxpayer have to shell out half a million bucks to the University of Colorado when you've already answered that question in *Challenge to Science* which costs \$7.95?"

"Beats me. Ask the Air Force. They are the ones spending the money."

They would like me to comment on the air. "That would only take a minute." Sure. Then they'll hit me with a question like "Dr. Vallee, how long have you believed in little green *men*?" all in the name of entertainment.

### **Later the same day.**

At noon today Allen and I went back to the Red Knight Inn for lunch. He often comments on our first meeting there four years ago in this same dining room, with Janine.

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"A lot of water has passed under the bridge, Jacques," he said with a smile and a puff of his pipe. Yet the atmosphere has remained the same. The folded umbrellas and the hats at the rack on the wall, the advertisements that give the place an old-fashioned air, and the rental car office across the street, the toy store, the insurance company building.

Hynek told me he understood my eagerness to move faster:

"I should have asked more questions over my years with the Air Force," he said.

"I suspect there was a turning point in the whole project at the time of the Robertson Panel," I told him without mentioning the Pentacle letter.

"You may be right, Jacques. You're forcing me to open my eyes."

"Let's go over the early years again."

"Well, when UFOs first appeared, the Air Force did not even exist yet, as you know. Forrestal set up the initial project, which is referred to simply as Project Saucer. This was followed by Sign which was created in September 1947. When they realized they needed the assistance of civilian scientists, Project Stork was created under the responsibility of Battelle. I served as consultant."

"Did you visit their facilities in Columbus?"

"From time to time."

"Did you follow what they were doing at the time of the Robertson Panel?"

"I didn't go there during that period. To tell you the truth they ran the operation in such a cloak and dagger fashion I thought it was laughable. Perhaps I was quite wrong."

He drew on his pipe, watched the smoke rising over us, then he went on:

"The Robertson Panel put an end to Project Stork. The Battelle Memorial Institute wrote their famous *Report #14* *zx*. the end of 1953 but it was only released by Project Blue Book in 1955. The project was then called White Stork. It is much later, under Colonel Friend, that Blue Book became the responsibility of the Foreign Technology Division. Two years later the consulting project became Golden Eagle and the contract went to McGraw-Hill."

"How many other scientists are working for Project Blue Book within the framework of Golden Eagle?"

Hynek seemed taken aback by the question.

"To my knowledge I'm the only one. The other people are civilian experts on plasma, propulsion and aeronautics, who are analyzing material gathered by the Foreign Technology Division, primarily data about Russian aerospace technology."

"Then let me ask you this: If we were to discover that a secret study had been done apart from Blue Book, should we reveal it?"

"Why the hell not? A real crime would have been committed against science, against everything the Constitution holds sacred."

"Do scientists deserve to know the truth? For example, should they and the public be told if it turned out that some evidence had been withheld from the Robertson Panel, and we could prove it?"

"Why shouldn't we tell them?"

"I'm not sure they've earned the right to know. Look at the dogmatic attitude of someone like Condon\_"

The mention of Condon's name made him react:

"Did I tell you the latest about him?" he asked. "Levine and Mary-Lou Armstrong spent the night at my house and they told me some new things. The cold, hard fact is that Condon has no interest in UFOs whatsoever. Never did. From the beginning he thought it was all a big joke. Recently he entertained himself by attending a meeting of so-called 'scientific ufologists' in New York. That episode reinforced his impression that all believers were crackpots, jokers or fast-buck artists. He talks about nothing else for hours, repeating how ridiculous these folks are."

Evidently there are now some serious rifts within the committee, and growing opposition between Low and Saunders. The latter fancies himself as a great expert on computers, and indeed he is knowledgeable in psychometric statistics and variance analysis, but that has little to do with data-base management. Mary-Lou has suggested several times that he ought to come to Chicago and see what I was doing. He promised to do it but each time found some pretext to cancel the trip. I have written the committee to let them know I was going to build a complete index for the Air Force files: they never answered. Mary-Lou passed my letter on to Saunders and he put it aside: "I don't believe he can do it," he told her flatly.

**Chicago. Sunday 16 July 1967.**

Saunders must have changed his mind: when we met with Dr. Roach on Friday night he gave me a list of coded sightings "at the request of Dr. Saunders" and he suggested that I go back to Colorado as a consultant. In particular they would like me to write a computer program that would convert my whole catalogue into their new format.

In spite of that small personal vindication, the meeting with Roach left me sad about the state of the research. Fred's impression was even sharper:

"That's a scandal," he said calmly. "The public is expecting serious answers from a committee of experts. Yet the truth is, these people won't even have taken the trouble to become superficially familiar with the problem by the time they write their report."

Roach asked Hynek how the Colorado committee should handle Washington when their final report was ready to be presented:

"Allen, you're used to testifying before Congress. What kinds of questions do they ask? What is it they want to know? Do they expect the truth, or do they just want to show off before their constituents?"

Hynek shrugged and gave one of his direct, unambiguous answers:

"They strictly want to show off. It's all politics and posturing. Truth is the last thing they care about. Your presentation before Congress, if there is one, will be a circus. They will ask Condon, 'Well, Doctor, do you believe in extraterrestrials now?' And they will quiz him superficially about the Michigan case, Marsh Gas, highly visible stuff like that."

"In other words, they won't really be testing the scientific value of the report?"

"No, their only motivation is political. When I testified before them for the first time I was Associate Director of the Smithsonian Observatory. Our tracking teams had lost one of the satellites, just like that, lost it! The Congressmen were trying to make me say that it was the fault of the Air Force, who were supposed to provide us with the orbital elements. They didn't give a hoot where that satellite was. It was only an excuse, a political vendetta against the Pentagon."

Yesterday Roach and Hynek have had a long private talk. Hynek tried to convince Roach to stay with the committee, because he is the only real scientist there. Roach is not stupid. He can see that the final report

which is already taking shape in Boulder will be a pile of garbage. He would prefer not to have his name associated with it.

Exhausted with our long emotional talks on Friday, Hynek fainted while he was arguing with Roach. Fortunately he was sitting down. He didn't break his jaw this time. But that's the second time such an incident has happened. He assured Fred it was only low blood pressure, and he will be more careful with his health in the future. How fragile all this is!

### **Chicago. Monday 17 July 1967.**

Bob Low must be very well informed, because he is now trying to get rid of Roach. He has "not been able to find an office" for him on campus, so Roach has to work from home. McDonald continues to act like a bull in a china shop. Not only is he telling every journalist he knows that the United Nations want to undertake a "discreet" (!) study on UFOs, he is instructing everyone to send massive amounts of UFO magazines and books to the "Moscow committee for UFO studies." I have just received an alarmed letter from Tikhonov. The Russians are concerned at this bumbling attempt to help them: their committee has only been approved on a preliminary basis. Any premature publicity in the West will kill their fledgling efforts, he warns.

Things are also closing down in France. Aime Michel has been told that a memorandum on UFOs was going to be circulated among military bases, but this idea has not been mentioned again.

Ignoring all that, I go right on punching masses of cards, hour after hour, day after day. I want my general index of the Air Force files to be finished at the end of the Summer, whether Saunders believes I can do it or not.

### **Chicago. Wednesday 19 July 1967.**

Yesterday morning I spent two hours alone in Allen's house again. I sat in that little room where he works and I immersed myself in his universe. His books are there, his tapes (they do not contain as many UFO interviews as German and Italian operas) and his file folders. Many of them are labeled but empty, because he grabbed the contents as he rushed on the way to some lecture and never put them back.. There are at least six folders entitled "Follow-up" with only a few scanty notes in them. Everything is confused.

Through the narrow window I looked over the small garden and the garage. I imagined him at work here: his secret mind, amused and curious, looking for adventure, for novelty. His good heart, his simple kindness, all that is contained within these four walls; the precious naivete of a very great man.

The chaos of his files reflects the trouble in his mind, a trouble to which I contributed four years ago when I rushed in here, brandishing the French cases, stressing the international scope, speaking of global patterns. Then came Socorro, Michigan, the media storm. Hynek had to rush ahead, testifying before Congress, explaining his unfortunate Marsh Gas comments. At the same time he was building a new observatory at Northwestern, giving lectures everywhere, running the Department of Astronomy. His class is the most popular on campus.

Now he seems lost, disoriented. While the Condon committee was wasting time and money on irrelevant issues the real phenomenon appears to have changed again. If journalist John Keel<sup>18</sup> is right, there are thousands of "silent contactees" who have seen flying saucers at close range and wouldn't tell their story to NICAP or the University of Colorado for anything in the world.

### **Chicago. Thursday 20 July 1967.**

A week ago Hynek was interviewed by John Wilhelm, a journalist from *Time* magazine. Aggressive, inquisitive, precise, Wilhelm pushed him around, squeezing him into narrow corners. Hynek answered frankly, but the questions kept coming back about the handling of cases by Project Blue Book, about the false astronomical explanations, about the statements made to Congress by the Air Force, about the Condon study. When Wilhelm halted the interview to go get a sandwich Hynek called me at the computation center:

"It's not going well, Jacques. Not well at all. This guy wants to know everything. He is looking for blood. He is going back all the way to Project Sign. He keeps asking about the possible role of the CIA. Obviously it's McDonald who is sending him here. You know *Time* magazine: they need a juicy story. They need a simple answer and a scapegoat they can throw to the public. But here there is no simple answer. If they could put everything on my back, that would make a lot of people happy. I'm trying to be patient but there are limits——"



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I could tell in his voice that he was genuinely afraid. He has given himself over to this crowd with his many public appearances, his lectures. But the public and the UFO believers are clamoring for more. McDonald is giving them what they seek, dramatic accusations and simplistic conclusions. On one side is Hynek, an older man who is troubled, who constantly questions his own life in search of deeper truths. On the other side is a fiery champion who thinks he has all the answers: UFOs are extraterrestrial, we're being watched, even invaded. Perhaps he is right. But the way he is handling the problem stinks.

"Give the McDonald sighting *to this* guy Wilhelm," I *suddenly* said. "It's the obvious solution. Why is Jim hiding his own sighting, pretending he has just discovered the problem, if he is so sure of the answers?"

At six o'clock I called Susan, Hynek's secretary. Allen was still in conference with Wilhelm, behind *closed doors*. It was only much later in the evening that I was able to reach him.

"I followed your advice. Wilhelm was visibly shocked when I confronted him with McDonald's own sighting."

With a very tired voice he added: "After a day *like* this, Jacques, I wish I had never heard of UFOs." I feel the same way.

The pressure and ridicule the scientific establishment places on anyone who dares raise this forbidden topic is incredible in its vicious character, in its unfairness. Following Hynek's letter to *Science*<sup>19</sup> a biologist wrote in to record a personal sighting. Hynek requested more details. He received the following answer:

I could simply ignore your letter and drop the whole story of my UFO observation. But that would be impolite and I don't want to be. Therefore I am answering you but I do so with regret.

I have been subjected to fearful trouble since my short letter in *Science*. In reality I am not at all a sensitive person. I have spent thirty years in Federal and State biological research and I think I have withstood all kinds of criticism. But I must admit I no longer want to receive repulsive remarks from my friends (are they still my friends?), from my associates, from crackpots and others. The recent letter by Stibitz (*Science*, 27 January 1967) irritates me and causes more confusion than I intend to stand. So, let us forget this whole story, please.

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No wonder scientists do not find the evidence convincing: the very best cases, the reports from their own peers, don't reach them because people are too embarrassed to describe phenomena which contradict what they think science is.

### **Chicago. Friday 21 July 1967.**

It is in the evening that I feel worst: morose, stranded in the emotional desert of Chicago, and eager to leave. We are not without friends. I have tried to describe my anguish to them, but can they understand? We go to movies with them, we attend dinners and parties, but we form no lasting close relationships. Exceptions involve a few Europeans like Hans Rasmussen, a Scandinavian doctor, and his wife Inge, who have the sense of humor and the joy of life that is missing from most of our acquaintances here. They feel somewhat stranded in the Midwest, as we do.

A letter has arrived from the Shell Petroleum Company. They are recruiting foreign nationals, particularly computer scientists educated in the United States who are willing to return to Europe.

And the UFO phenomenon keeps evolving. Don Hanlon's research on the 1897 wave keeps turning up some extraordinary facts. When I compare some recent landing cases, or even the Hill abduction, with some of his old airship sightings I cannot help but feel disturbed at the similarities. And a new wave may be starting in France. Last Wednesday a young girl who was walking in the countryside with three children saw three little men running away from her, talking to one another in bird-like sounds.

Fred argues I should level with Allen and put the Pentacle letter in front of him before he goes off to the Prague Congress. But I don't think he is ready. His current confusion is obvious: back in Canada he is now writing an article for *Playboy*, of all things. That's certainly not the right way to place the subject on the map of respectable science.

### **Chicago. Saturday 22 July 1967.**

My Rosicrucian friend Serge Hutin would say that "the Cosmic has spoken." Everything suddenly appears in a new light. I am ready to leave America, and I am at peace with myself again. I find new beauty in the whole world: sky and creatures, winds and tides, *vents et mares*. . . Is it possible to love to the point of madness without knowing what it is we

love? An invisible presence passes through my life and caresses me this morning with indescribable tenderness. I submit joyfully.

**Chicago. Tuesday 25 July 1967.**

John Keel has written a strange letter to me. The first part is rational, serious and well thought out. Then he suddenly slips into a disturbing, prophetic style:

From all over the country people are calling me for help. Next week will witness a chain of events that will shake the planet. Religious fervor will engulf the world. A prolonged black-out will take place in the next few days, probably on the 26th or 27th. . . .

After Jessup and Smith, could it be that John Keel will become obsessed with the phenomenon to the point of prophesying disasters?

Hynek writes to me from Canada, sending me an action plan he calls the "Blind River Manifesto." Unfortunately I disagree with almost every point it contains.

A telegram has arrived from Shell. They invited me to call them in New York. I found the conversation quite refreshing after the stream of bureaucratic letters I have been receiving from various scientific and technical institutions in France. We do want to return, but not until I have a job that makes sense, with people I can respect.

**Chicago. Friday 28 July 1967.**

Allen called me from O'Hare this afternoon. He was on the way from Canada to New Mexico. He told me that Keyhoe had just fired Richard Hall from NICAP. Hall, who is divorced, recently married a NICAP secretary.

I don't see what's wrong with that!" I told Hynek.

Me neither. But upon his return from their honeymoon in Las Vegas they found they no longer had a job. Hall tried to get hired by Condon who turned him down. Things are boiling over in Boulder, too. Mary-Lou Armstrong had a big fight with Bob Low, and now she only works half-time. Must be the Summer."

**Chicago. Tuesday 1 August 1967.**

Ben Mittman came back on campus this morning and showed me an encouraging letter from Rowena Swanson at the Air Force Office of Scientific Research. She says of my proposal that it is "one of the most sensible statements I have had the pleasure of reading about the work that can and should be done with data acquisition systems."

She is especially interested in the stellar data-base and the follow-up to the Altair work, so she would like to see us go ahead with the setting up of an information science center.

Hynek is back on campus, too. I met him in the hallway of the computation center. He seemed tired, ill at ease, disoriented. When I showed him how his files had been reorganized into the three shiny new cabinets, he was amazed at the progress made in a short time.

**Chicago. Wednesday 2 August 1967.**

Sticky heat, crying kids in the street. Things are dragging along. Another Chicago summer. Fred and I had lunch with Allen. When I brought up Battelle he changed the topic of conversation. So I stressed the old sightings, 1947, 1948. I have been studying them under the microscope for the last two weeks. He patiently repeated what he had told me before:

"Back then I was certain that all those phenomena could be identified. Of course when you read the reports now you see things differently."

He made an apt comparison, drawing a parallel with astronomy:

"If you know what you're looking for, you can take an old plate of the night sky and pick out Pluto. Of course it was there all the time before Tombaugh discovered it. People just didn't know it was there, so they didn't see it. Well, it's the same thing with UFOs."

We turned to the subject of hypotheses. Hynek would like to write a set of axioms from which he would derive the whole study of UFOs. A laudable goal. His first axiom would have to do with communication among the worlds, a very old hermetic concept.

"I simply cannot conceive of a universe where communication among remote planets is made impossible by a theoretical limitation. Everything man can imagine, he can realize sooner or later. Well, dammit, I really want to know what's going on in Andromeda. Nothing will convince me that's impossible."

I agreed wholeheartedly. It may take a hundred or five hundred years for us to learn how to do it, but eventually we will.

Second axiom: Mystical experiences demonstrate the possibility of mental projection. Hynek quoted the tests done with Ted Serios<sup>20</sup> and some recent LSD experiments.

"The limits of the mind are poorly known, we underestimate the human psyche."

Here I cautiously agreed with him, although Fred and I always grit our teeth when Ted Serios is mentioned, because the experiments seem inconclusive. If we want to bring in psychic abilities there are better examples. The Dutch research with Croiset the paragnost, which is being conducted at the University of Utrecht, is much more solid.

Third axiom: The physical plane is not the only level of existence. The hermeticists speak of an "astral plane." The spiritists talk about materializations, *apports*, ghosts that go through walls. Who knows if UFOs are not observational devices that are materialized into our world by the denizens of another?

I brought us back to more immediate matters.

"Since our 1952 files are incomplete," I asked Hynek, "why not contact Battelle to request a copy of their punched cards, or a listing of the cases used in *Report #14*?"

He seemed embarrassed.

"Well, that might not be easy to do. They are more and more involved in military contract work. In fact the Pentagon has prevailed upon *Time* magazine to eliminate any reference to *Report #14* from Wilhelm's article. You're not even supposed to have heard the name Battelle in reference to UFOs. If they sign my new contract I'll go straight to the historical files to check up on all this."

Fred and I exchanged a quick look.

"What was the attitude of Robertson and his people when they got that panel together? What questions did they ask? Did they demand to know where the sightings came from, who selected them?"

"I can only speak about the sessions I attended," said Hynek. "I had the distinct impression they didn't care."

"Allen, let's be serious," interjected Fred. "If somebody came over to the University of Chicago this afternoon and told me he had a bachelor's degree from West Texas College in animal husbandry and he was

going to teach me how to take apart my electron microscope I would throw him out. How could Robertson, Alvarez, Page and other scientists of that caliber listen to some Air Force officers as if they were hearing the Gospel? They were in control. Their reputation was on the line. Why didn't they demand to be briefed exhaustively about the Battelle work?"

"It's very likely they just never knew about it," said Hynek, pensively drawing on his pipe. "The CIA and the Air Force may not have told them."

Incredible. Fred and I spent the rest of the afternoon rummaging through old papers, and we spoke sadly about the future. Fred told me about his experiments with the brain, and Aldous Huxley's observations under mescaline. The human mind relies on a delicate chemical balance. When that balance is altered, other worlds open up. Is there a single solution to the equation of thought?

We also discussed my imminent departure from the United States. Fred told me I shouldn't do anything hasty. But if there is a secret study in Washington, what good is it for me to continue all this hard work? I might as well go on with my own life, my own career. After all, I am a rather good computer scientist. I can make an honest living. I don't need all this political crap.

"Do you really think there's a secret study?" Fred asked.

"Yes," I said, "although it may be nothing more than a bunch of space cadets engaged in a misguided, technocratic effort at duplicating the UFO propulsion system. It may be coupled with an in-depth analysis of traces and material fragments."

I thought of Astropower, and of the McDonnell-Douglas company, who is rumored to have a secret team, employing a physicist named Stanton Friedman to collect physical data in a hush-hush manner.

"All those people are glorified rocket engineers, they can hardly put their arms around the real problem."

"Don't underestimate them," says Fred. "It's impossible to suppose that they would study UFOs without thinking about their origin, and about the beings that pilot them..."

Fine. But I would like to know the answers to a different set of questions. For instance, to what extent are these strange humanoids related to the beings so often reported in our own history? If the same phenomena

have been around us for centuries, and if it is only a cultural accident that makes us interpret them in terms of spacecraft and extraterrestrials, then it's a whole new ballgame, isn't it? How would we prove it? That research can only be carried out in Europe, where the best sources for historical material are located.

Will I have the courage to leave? Evanston itself is a very silly town, but every time I set foot on campus it is as if I stepped into a magic circle.

Hynek told us about his visit to Colonel Freeman when the Air Force had just signed the contract with the University of Colorado:

"He was laughing, rubbing his hands together. Obviously he regarded me as one of the boys, someone who was 'in on the big joke. He acted very freely in front of me. He's sure Condon will write a report that will conclude that UFOs don't exist and he'll be rid of the whole thing at last. There was no question in his mind that the Colorado people would reach a negative conclusion."

### **Chicago. Thursday 3 August 1967.**

This morning I finished punching 1963 and I spent the afternoon screening 1948, where I found yet another series of remarkable, forgotten cases. Now I *can* say without *fear of contradiction* that I know the Air Force files better than Quintanilla. I do have the information at my fingertips. And more significantly, *I know what isn't there*.

This afternoon Hynek called from Wright Field to ask if we had a copy of the August 1952 Bellefontaine, Ohio, case. The answer is negative. But the information is not in Dayton either, which is embarrassing because the file has been formally requested by the University of Colorado. The Air Force will be forced to admit that some important pieces of data are getting lost, an accusation I had already made in Boulder last November.

I also spoke briefly to Quintanilla, who is always very urbane with me. I described my ongoing work on the Blue Book index and asked his permission to go on with these statistics. There is no problem, he said, everything has been declassified.

We have just learned that one of Allen's colleagues, Northwestern astronomer Carl Henize, had been officially selected as a scientist-astronaut by NASA.

**Chicago. Tuesday 8 August 1967. 19:00**

Today Fred came to my office to read the reorganized files. I am getting to the end of the card punching: two long metal drawers full of cards. I have written a validation program to detect duplicates and make sure the index was clean. It also generates some basic statistical breakdowns on a weekly basis.

We no longer have a sitter for our son, so this morning with a heavy heart I left Olivier at a day care center, two dark dusty rooms on Clark Street. I am eager to move away from here. Rowena Swanson has told us that federal funding for new research such as our proposed information center was blocked because of the expanding Vietnam war.

Allen leaves for Prague tonight. He will be gone until September 5 th. I saw him in Evanston last Saturday and we had coffee together. At my suggestion he had gone to Sweeney to request a copy of Battelle's card catalogue from *Report #14* days. He was coldly told that they were no longer in existence. Sweeney pretended to be outraged: "It's a crime, it's unthinkable\_\_\_" But Pentacle is still with Battelle, and he has told Quintanilla that the cards had actually been thrown away.

**Chicago. Thursday 10 August 1967.**

Ben Mittman took the news with concern today when I told him I was leaving. I am presenting him with a complete Fortran-based Infol compiler that is a few steps ahead of the Control Data version developed by Bill Olle and his team.<sup>21</sup>

The new French Institute for Information Science has expressed interest in hiring me ... whenever they officially become operational. And Dave Saunders has called me with some new ideas.

"Can you come to Boulder as our consultant?"

"When?"

"As soon as possible."

"I'm working full-time on a proposal for our computation center here."

I am beginning to like Dave Saunders, but I am still suspicious of anything the Condon committee does. If I became connected with their work I might provide some implied measure of support for their conclusions in a project which has been biased from the start.



"Dave, if you don't mind, I would prefer to have Allen Hynek included in our discussions. Can the meeting be delayed until he returns from Prague?"

"Well, perhaps you could both come to Boulder when your schedule clears up, say around September 15th?"

We agreed tentatively on that date. But I have a feeling that I will be a long, long way away from Boulder, a long way from the United States by September 15th. He went on: "Anyway, we'll see each other in Washington when you give your talk before the American Psychological Association."

"I won't be going to Washington."

He seemed shocked. "Why not?"

"Well, don't you think it's going to be another one of those meetings where everybody talks and nothing concrete gets done?"

The committee has started to do some statistics, but they work from a very heterogeneous sample which is mostly garbage. They want me to write a program that would convert our 3,000 screened and documented cases into their code, and naturally they would also like to get their hands on my more recent catalogue. But simply mixing up a lot of data from various sources is not going to provide a workable data-base.

Chicago. Tuesday 15 August 1967.

The first seven volumes of the *Archives of the Invisible College*, each about three hundred pages thick, have come back from the bindery. They contain some unique documents, original letters, analyses of confidential sightings, and the most interesting Air Force reports, after all the trash has been screened out: as close to the real UFO phenomenon as we can come.

My friends think they can keep me here, that at the last moment I will decide to stay. They are wrong. I have lost interest in what happens here, in the outcome of the Condon committee study.

I draw my strength from Janine. She is more ravishing than ever, but it is her inner beauty that fascinates me. Today we took a trip downtown together to see the city for the last time. We watched the splendor of Chicago. Little blue workmen perched on steel beams at the thirtieth floor of a new building, sparks flying from a welders torch, a back alley filled with broken glass, an orange truck in the shade, jackhammers on

granite, Janine's needle heels on marble tiles, the new Picasso sculpture, the waves along the Lakeshore, red concrete trucks rumbling along, green steamrollers on a new expressway, the setting sun splashing over our windshield. Picasso is not appropriate here. Chicago is a Dali, a giraffe with drawers, a soft machine, a burning rhinoceros eating the wind, spitting fire like a mad dog.

### **Chicago. Saturday 19 August 1967.**

We are packing up all our books into boxes to be shipped across the Atlantic. With the end of the Summer, the Europeans are coming back to their offices after their long vacations: the job offers I receive are becoming more precise. Thomson wants me to work in their Bagneux research center, South of Paris. But everything in their letter reeks of bureaucracy, of rancid procedures. Shell, on the other hand, has a concrete proposal. They will pay my way to Paris for an interview: Can I fly there early next month? I feel Chicago receding away from us, already becoming a foreign city again in my mind. Rain falls on dirty sidewalks. I drive along dilapidated Clark Street. Over the car radio, Johnson makes a contemptible speech to explain why he has to bomb the length and width of Indochina. The pale, haggard silhouettes of drunkards stagger in front of my car.

I will take Olivier to France with me on my way to the Shell interview, and I will come back alone to help Janine pack. My short-term priorities are to find a secure place for the general card index, for this Journal, and for the Archives.

### **Chicago. Saturday 26 August 1967.**

It is ironic that all the civilian research groups in American ufology are collapsing precisely at the time when the University of Colorado is affording them their first glimmer of respectability. In Tucson Coral Lorenzen has become so obsessed with flying saucers that she locks herself inside her house, according to people who have recently visited APRO. In Washington, since he has fired Dick Hall, Major Keyhoe is going broke. He has launched another one of his heroic appeals to the pocketbooks of his members, assuring them that the solution to the problem is in sight at last. Is it?

I am still struggling with the ethical issue of what I should tell Hynek. Should I show him the Pentacle letter? After much debate, Fred and I

have reached the conclusion that Allen alone could make use of it. Right now he is in Prague, trying to get information on UFO sightings in Communist countries so that he can mention them in *Playboy*. Somehow I don't think this is the best scientific way to approach the problem.

All the people we have consulted say it is simply "inconceivable" that the military hierarchy would rely on Blue Book alone to investigate a phenomenon which can interrupt radio communications and interfere with missile base radar signals, as happened recently in North Dakota.

We keep hearing of old secret government projects that are not documented anywhere. A former engineer with U.S. Intelligence in Germany has told us that the National Bureau of Standards had conducted an investigation of UFOs under Professor Dryden as early as 1943, along with an investigation of German research on jet aircraft. They were already aware that UFOs interfered with internal combustion engines at a distance. They suspected electrostatic effects.

In 1963 a Polaris nuclear submarine suddenly came up to the surface, interrupting its stated mission, which called for a long submerged cruise in the Atlantic. All personnel were told to remain below. A few superior officers went up to the tower. They are said to have come back down with three humanoid bodies in clear plastic bags. The sub dived again and rallied to the East Coast at top speed. The vessel had accomplished none of its stated objectives, which included the test firing of several missiles. As for the beings, they looked like shaved monkeys. Perhaps they were indeed monkeys, recovered from a classified space experiment?

There is also an employee at an Alabama military base who told an extraordinary story: he had unloaded several gray-blue humanoid corpses from a helicopter to place them on a waiting plane whose propellers were already turning. He was told the plane was going to Colorado. Naturally, it has not escaped our notice that all this could well relate to a super-secret space project involving primates as test subjects, and that the "crashed saucer" tale could be the actual cover story rather than the underlying truth. As Fred puts it so well: "Government is an obscenity."

### **Chicago. Sunday 27 August 1967.**

There is no proof that any of the above stories actually relate to UFO pilots. In that respect the Pentacle letter is very valuable because of what

it does not say: it mentions no crashed disk, no hint that humanoid pilots have ever been recovered.

**Chicago. Monday 4 September 1967.**

Deep inside, I still believe strongly in the value of science. Yet the kind of scientific process that will crack the UFO mystery open has not been developed. Contrary to Hynek's hopes, I believe research will continue to stagnate, and that the United Nations will do nothing.

Our American friends, even the most enlightened, look at me with some suspicion when I tell them that a war like Vietnam will do more damage to this country than it will to Indochina, no matter who wins in the end. This does sound like an absurd statement, since there is no combat on American land. Yet the harm the war does to American society is deep and irreversible. You can see it everywhere.

**Chicago. Tuesday 5 September 1967.**

Allen has called me, proposing lunch. He has returned from Prague with the strong feeling that astronomical opinion is changing. De Vaucouleurs and Guerin have told him they were impressed with the reports of landings. Kuiper has told Hynek he was going to submit an article to *Science* on the subject of extraterrestrial life.

Hynek gathered a rather heterogenous group of colleagues at his Prague hotel. He even gave them a nickname: *Commission Zero*. It was composed of most of the attendees who had an interest in UFOs: Bob Low and Frank Roach from the University of Colorado, Morrison from MIT, Sagan and Menzel. But why did he neglect to ask Guerin and De Vaucouleurs to join? Sagan invited Shklovski, who declined. "There is no UFO problem," he said.

Roach described a luminous object he had seen and photographed himself. He said he had not been able to identify it.

Menzel jumped up. "It was an airplane, of course!" he said.

"Don," replied Roach sadly and calmly, "I am always impressed with how easily you can solve a problem without having all the data!"

He also related his radio conversation with a forest ranger in California who was clearly terrified by what he saw.

"An incompetent observer," commented Morrison, "the testimony is worthless."

Hynek says that as long as Menzel was in the room, nothing good could come from the meeting. He also learned that Condon was definitely out of the picture. When Frank Roach went to tell him about his own sighting he said:

"I'm sorry Frank, but I'm really too tired."

Allen, on the other hand, is in splendid shape. He gave a lecture on UFOs at the Planetarium in Prague. A man came to him furtively after the talk and presented him with a stack of newspaper clippings from the local press. But he added that it was the military, not the Czech scientists, who did all the investigations.

Allen now realizes that my departure is imminent and that nothing will change my mind.

"Will you promise me something?" he asked. "You've often said that you would drop everything if you could work on this problem full-time, right?"

"That's still true."

"Will you make this commitment, that if I can get the right level of support to create a real research project on this topic, you will come back from Europe and help me run it?"

Yes, I told him, I would make that commitment. I don't think anything would keep me away from such a project, if it were seriously financed and built on a solid basis of hard science. I would know how to run it. My objectives are set. I already know what I would do the first day, the first week, the first month, the first year. And I'm not talking about mere computer statistics, either.

### **Chicago. Friday 8 September 1967.**

Tomorrow Olivier and I leave for Paris. For the first time in our lives Janine and I will be separated by the ocean.

Kazantsev has sent the first copy of *Young Technology* carrying our joint article. I took it to Mittman and together we managed to translate a few paragraphs. This is quite a coup, the first article in the Soviet press which is open to the reality of UFOs:<sup>22</sup>

The authors of the present article believe that the hypothesis that UFOs are machines built and used by an alien culture must be considered scientifically.

This article in a magazine printed at over one and a half million copies was immediately picked up by the trade union newspaper, *Trud*, which has an even higher circulation.

When I saw Hynek I gave him a copy, telling him that according to Kazantsev this issue of *Young Technology* had quickly achieved the status of a collector's item in the Soviet Union.

I have bought a golden frame. I have placed into it a beautiful reproduction of *The Lady and the Unicorn* tapestry which I intend to give to Allen as an ultimate gift, in Fred's presence, before I leave. Between the picture and the cardboard I have inserted the Pentacle letter. That document, after all, comes from his personal files, and I have no right to keep it from him, nor do I want to carry it to France with me. I will make Hynek read the document, then we will replace it under the painting. Let him decide what he will do with it. In this form, beautifully framed yet completely invisible, he can just hang it on a wall and forget it.

## Part Four

# MAGONIA

# 16

## **Paris. Rue de la Mesange. Tuesday 10 October 1967.**

No one was waiting for us at Orly. We enjoyed keeping to ourselves this little *secret of our return to France*. We smiled at each other, our hands touching, feeling young and full of expectations. We arrived early at the apartment on rue de la Mesange. We waited at the door for my mother *to come back from the Mouffetard street* market. We helped her carry her grocery bags upstairs.

Now Annick has come over to pick up Janine, and the two sisters have driven off *to Normandy* where our *son* is. I have spent the day walking through Paris alone, looking intensely at every small detail. I felt a sense of pleasurable devotion when I saw the Île Saint Louis again. Let anyone who hasn't loved this city make fun *of my* weakness.

At the foot of the Petit-Pont a flat barge carrying an old steam crane on its ancient deck was unloading sand. Inside the steel cabin a man was rummaging in the fire with an iron hook. From the quay a worker was sending down a load of beer bottles along a cable.

## **Paris. Rue de la Mésange. Thursday 12 October 1967.**

I have lost track of time. For the last three days I have been walking all over Paris. I am eager for Janine to come back and help me look for a place to live. I have been buying books, books and *more* books.

## **Paris. Rue de la Mesange. Friday 13 October 1967.**

Janine comes back tomorrow. I have spent the afternoon with *my* uncle Maurice in his quiet apartment on rue du Cherche-Midi, behind the church of Saint-Sulpice, admiring his work and his dreams.

Before giving Shell formal notice of my acceptance of their offer, I have decided to talk to a few other French companies involved in the development of sophisticated computer programs, to make sure I'm not missing anything. Today I met with the head of software for Control



Data France. I tried to convey to him my interest in the new generation of languages that are appearing in the States. Would his bosses in Minneapolis allow his group to develop new compilers? I asked. He did not know how to answer my question.

An American friend has sent me a copy of an amazing article about UFOs published in the very authoritative *Science* magazine last month. Authored by William Markowitz, a physics professor at Marquette University, it contains such idiotic remarks as "the published reports generally describe objects about five to one hundred meters in diameter, which land and lift-off without the use of launching pads or gantries. No similarity to the giant undertaking of a launching from Cape Kennedy has ever been reported." He deduces from this that UFOs cannot be under intelligent non-human control if the laws of physics are valid!<sup>1</sup>

**Paris. Rue de la Mesange. Friday 20 October 1967.**

I should describe an experience I had with the Plan Calcul today that has left me puzzled and amused. This Plan is a conglomeration of government-funded projects. Some of them are little more than disguised handouts to the big companies, others are research programs administered by various agencies, notably the Institute for Research in Informatics and Automatics, known as IRIA.<sup>2</sup> I tried to reach their people all day Tuesday. Since my mother doesn't have a telephone (there is a three-year waiting list to get one!) this meant going down four flights of stairs and walking three blocks to the Post Office every hour or so to try and get through. At the Ministry I was told that IRIA had just been assigned a building near Versailles. One look at the map and we felt enthusiastic: it would be great for Janine and Olivier to live there, West of Paris, so close to the countryside and the forest. The problem was the phone line: always busy, no way to reach anyone.

"The Plan Calcul is in the midst of such a boom," I thought, "that the switchboard must be literally swamped with requests."

So I bravely decided to do things the American way—going there in person to meet their top people even if I had to camp on their doorstep to be admitted among the throng of their admirers.

At the railroad station I bought a sandwich and tried the phone line again from a public cabin: still busy. So I let the little train take me away through the Western suburbs. To my left was a young fellow who set

aside *L'Humanite*, the Communist daily, to roll himself a cigarette. In front of me an older gentleman was reading the conservative *Le Figaro*. I relaxed, feeling at home again.

From the train station in Versailles I called the Plan Calcul once more. Still busy! I demanded a check of the line. It was functioning normally, I was told. I went out to the fresh air and the sunlight. It was a fine October afternoon. The woman who was selling newspapers didn't know where Plan Calcul headquarters were, but she did know how to get to the town in question. Take the B bus to the end of the line, then you walk.

And suddenly it is as if nothing had changed since my childhood. It is as if I were eleven or twelve again. I am in the B bus which climbs up the rue de la Paroisse. The whole world is simple, a charming mosaic of little *cottages* with their gardens and their rough *stone* walls and their roses, their stone crosses and their *plaster* angels. Silence. Like a time machine plodding back to the distant past, the B bus turns into the Boulevard du Roi and rests at Le Chesnay. I am the only passenger left. It's lunchtime in this quiet, conservative suburb. I jump out.

"Rocquencourt? It's over there," the driver says with a vague sweep of the hand, showing me a narrow alley between two hedges, ironworks, barking dogs, dusty sunshine.

He adds: "It's quite a ways away."

Who cares? I grab my briefcase, which contains a few reprints of my recently published American articles and some **Infol** listings and I walk, keeping a weary eye on the black clouds that now line the horizon. I am not far from my goal, I am told.

"Do you see those old barracks over there?" someone tells me.

He is directing me to the former headquarters of a company of firemen who have recently moved to more luxurious accommodations. Yes, that is **IRIA**, those wooden shacks. It looks like the set for "Stalag 13." The famous new research institute occupies three small, silent offices there. A secretary lifts her *eyes* from a romantic novel and greets me with a smile.

"You tried to call us on the telephone? Ah, poor Monsieur, we have just learned that we have been cut **off** for two days. Fortunately somebody has asked the Post Office to check our line, otherwise we would still be without communications."

It was me, calling from Versailles! So I have already made a contribution to France's Plan Calcul, by reconnecting their one and only tele-

phone line. It is a measure of the *intensity* of their business that the managers did not even notice that they had been without a telephone for two days. No one will be in the office before 2:30 p.m., the secretary tells me, so I walk back to the village to get another sandwich, feeling silly in my dark business suit and tie.

It is a bit after three o'clock when the managers return from lunch. My presence is now announced with proper official formality. Through the open door I can hear a bureaucrat speaking to the woman who says: "It's someone who has just come back from the USA..." I catch the abrupt reply:

"Let him talk to Louis."

It turns out that Louis is an underling who was there all the time, of course, but nobody thought of him. He is quite pleasant and hospitable but I have already made up my mind about the whole meeting. This way of treating people, of handling outsiders, these offices that stink of bureaucratic neglect, of obsolete ideas, represent everything I fled in 1962. I ask a few questions, *to be* polite, *to pretend* I am interested in what they do.

"What computer hardware will Plan Calcul be using?"

"That hasn't been decided yet."

They have created this institute to fill the gap left by the universities which prove incapable of turning out good software research, but they are putting the same hopeless bureaucrats in charge of it. Plan Calcul is only the old tired coterie preserving the same petty privileges under a new label.

I left a few reprints of my American articles with Louis. He got up politely when I took my leave and he saw me to the front steps. I felt so light as I walked away, so happy to realize that I would never work there, that I started whistling along the hedges and the rosebushes or suburbia.

Paris. Rue de la Mesange. Monday 23 October 1967.

I feel a *fierce* need *to* attack new things, to look for the undiscovered. The grave and deep nature of this world must be knowable. It is a waste of one's life to look for anything else.

I have brought up the physiological effects of UFOs in a discussion with my brother the doctor. He doubts that the instances of paralysis

reported in cases like the *Valensole* landing two years ago represent genuine paralysis through action on the nervous system. To him they sound more like psychological effects. Masses sleepiness after his sighting could be a psychological reaction too, he tells me.

**Paris. Rue de la Mesange. Wednesday 25 October 1967.**

By taking another week to extend my network of contacts I have also taken some risks. I feel exhausted and we are rapidly running out of cash. What I am gaining, on the other hand, is a good overview of the French software world. There is practically no research activity in my current specialty of data-base management. Mr. Lepoisson, a man who could have been my boss if I had joined Thomson and who will also head up some part of Plan Calcul, tells me that EDF, the giant French electrical utility company, is about to create a massive index of its technical publications. They have decided to do it the hard way, under a conventional framework. I wish them good luck.

I *only spent* half an hour with Mr. Lepoisson at *Thomson* headquarters. I found the old building just as I imagined it, with layers of fading paint on the walls and centuries of bureaucratic passivity peeling off the ceiling. Technical directors talk to you indifferently in boring, drab offices where nothing happens, where nothing could possibly happen. They wear their brown pants and the butterfly tie cousin Gustave had offered them on their wedding day twenty-five years earlier. When I mentioned the new techniques for large data-base management Mr. Lepoisson shrugged and replied that it was a trivial problem which had already been solved by his management consultants. He did offer me a job in the end, but when I reacted in shock at the low salary figure he replied with a fine superior smirk that young men often returned from America with exaggerated notions of their own worth to society.... I certainly would never find elsewhere a salary much higher than what he had quoted me, he assured me.

I answered calmly that Shell offered me fifty percent more. For the first time, his *reaction* was genuine. It was bitterness mixed with *contained* rage.

I have called Plan Calcul again, just to close the loop.

"The problem in your case, *mon cher Vallee*, is that you have not been introduced by one of our Directors."

"But I saw Monsieur Lepoisson yesterday *afternoon*. . ."

That changed everything. People suddenly spoke to me as if I were actually related to them rather than an alien from across the sea, an unknown quantity. But they only became truly friendly when I hinted, for fun, that I might agree to work without pay for a while.

On Friday, to complete my explorations, I will meet with the director of the scientific computing center for Armaments at the Montrouge Fort. Today Janine and I visited a two-bedroom cottage which is for rent in Saint-Germain-en-Laye.

**Paris. Rue de la Mesange. Thursday 26 October 1967.**

Aime Michel and I went rummaging through the flea market near the Pantheon this morning. We looked at dusty books and old fabrics exuding the wondrous stench of bygone centuries. We spoke of the ills of the nation, the weaknesses of old man De Gaulle and the rigidity of French science. We came back to rue de la Mesange for lunch. I read to him passages from Paracelsus' *Book of the Sylphs*, which left him fairly stunned because of their similarity to the UFO entities he has been describing in his books.

Three letters have arrived from abroad: Olavo Fontes wants me to go and work in Brazil, a little too late. Charles Bowen is all excited at my sudden departure from Chicago and suspects it is hiding great truths. Gordon Creighton is urging him to jump into a plane and come to Paris to interview me and learn "my secrets." Indeed Creighton, who is a very subtle man, has understood that it was not simply a vague nostalgia that made me come back to Europe, but some specific discovery that had disgusted me with America. They want me to continue contributing to the *Flying Saucer Review*.<sup>3</sup> The third letter comes from Jim McDonald, who is wasting my time.

We will take the cottage in Saint-Germain. Janine has told me we will have another child in a few months. She likes this place with the narrow green shutters and the walled garden, where Olivier and the baby will be able to play safely. The forest is two blocks away, at the end of the street.

**Paris. Rue de la Mesange. Friday 27 October 1967.**

After all those visits to pompous bureaucrats, the Armaments guy was dynamic and realistic, a refreshing surprise. I told him that my visit

was only made out of courtesy, since I had already decided to accept another position. He said I should keep in mind the possibility of working with them if I ever wanted to pursue self-organizing systems and artificial intelligence in data-bases.

I went to rue de Berri, near the Arc de Triomphe, to tell the managers at Shell that I would take the job they offered me in their computer data-base project.

Hynek writes to me that he has travelled to Columbus to see Pentacle and his team at Battelle. But it seems that instead of confronting him, he meekly inquired about the contents of the old letter, and naturally he was rebuffed. He says it himself: "I don't know any more now than I did before." And he adds:

There are, very simply, two explanations, and I could defend both of them. Of course I didn't take the famous letter, but I quoted enough from memory to get them very worried. They insisted on the fact that their cards had been destroyed and there was no listing, and all that had taken place with the approval of ATIC.

Technically Allen is right, of course. The letter could have simply meant that Battelle didn't want to come before the Robertson meeting until all the facts were in. The Air Force was in a hurry to get rid of the problem, didn't give a damn about scientific truth, and went ahead with the panel. But it is more likely that there was indeed another project, a truly secret one, and that the panel served as a smokescreen, to keep civilian scientists away from a serious study of the observations.

**Paris. Rue de la Mesange. Sunday 29 October 1967.**

I have found Aime Michel rather dogmatic, attached to old ideas. He has not developed their implications further:

"We are witnessing the first example of contact between two societies in which the inferior group, namely mankind, will not have died as a result," he says. "This contact with the extraterrestrials will become open when all of us on earth believe in Them. This is why they show themselves a little bit at a time: it is up to us to break down scientific and official opposition to their reality."

He is in danger of becoming an evangelist for the Saucer cause rather than an analyst trying to find the truth.

"How do we know that the true purpose of the 1897 wave wasn't to be rediscovered in 1965," he asked, "just like the laws given to Moses on Mount Sinai may have been designed precisely to make the Jews unable to become assimilated into other cultures, a fact that has given us Karl Marx, Sigmund Freud, Albert Einstein?"

I argued that we could only gain greater insight by listening more closely to what the witnesses are telling us. I reminded him of the Paracelsus piece I had read to him last Thursday. What about all the forgotten accounts of Little People, of Elementals, of Leprechauns? If these beings are part of the same phenomenon we see now, what does that mean for their nature? Are we necessarily dealing with extraterrestrials? And isn't it premature to assume, as he does, that we are the inferior culture?

**Paris. Rue de la Mesange. Tuesday 31 October 1967.**

We took Olivier to Saint-Germain to show him the little house and to sign the rental agreement. I wish I could earn enough money to buy such a place, to give Janine and my son the security of a shelter of our own, the assurance of a little warmth. I would also like to be able to buy more books, to have more time to write. But this is a good beginning, a new adventure. Living it through Olivier's fresh eyes makes it even more exciting.

Some evenings, when I have just closed some book like Anatole France's *Life of Henry Brulard*, the spirit who watches over dreamers like me sits down next to my bed and says:

"Will you always demand new topics? I had already turned your head with forbidden science, with paralyzing visitors and cosmic enigmas. You have gone beyond them. Now it is elves you need, dark forests, a whole landscape of sulphurous witchery. You seek occult destinies, you want to know the meaning of ancient archetypes\_\_\_\_Your old boss Paul Muller was right, my friend, you do think too much."

I feel like an Adept on some mystical retreat, ready to cut off relations with the outside world, like the Rosicrucians of old who were required to retire from public view periodically, for years at a time. To be sociable I really should answer Bowen, Fontes and Hynek. But I am not inclined to do it at the moment. This retreating movement is a deep imperative in the alchemy of my existence. I feel that here in Saint-Germain I am renewing a very old link. To live consciously, as someone who

is spiritually awake, someone who chooses to direct his life instead of just experiencing it as a series of external events: such is my ambition.

**Paris. Rue de la Mesange. Monday 6 November 1967.**

This is my third day of work, creating a version of the Infol language that will run on the mainframes of Shell. The company has two large computers in the basement of its majestic building on rue de Berri, both of them expensive Univac computers. The large room is quite a sight, with its long rows of tape drives blinking, the operators efficiently gliding between the consoles and the assistants wheeling out stacks of reports as soon as they come out of the high-speed printers. All the data-bases are manipulated by hundreds of programs written in Cobol, the traditional business language. There is no facility for direct interrogation of any of these items, and every change requires weeks of planning, months of implementation. A high-level language such as Infol could make it possible to get information in and out quickly, to establish correlations, to keep client lists, contracts, marketing data, economic tables up-to-date with much less effort. But Infol has been written for another type of computer. So my first task is to rewrite the compiler.

I have written to Ben Mittman on the subject of software and to Tikhonov on the subject of UFOs. Hynek is finishing his article for *Playboy*, where he will make the dubious claim that the Russians are ahead of the United States in UFO research. He thinks this is a particularly clever ploy: is there a "UFO gap" like there is a "bomber gap" or a "missile gap?" he asks. I think the people he is trying to impress in Washington will simply shrug. The despots in Moscow will frown, and they may clamp down on what little research was beginning to flourish around Kazantsev and Zigel. I have warned Tikhonov that they should not expect too much from the Condon committee. I did not elaborate. He is smart enough to read between the lines.

Coming out of my alchemical silence, I also wrote to Hynek on the Pentacle letter, pointing out that "it throws into doubt all the statements made afterwards (by the Air Force), all the reports from official sources\_\_\_\_" And again I bring up the topic of the missing photographs. Why is the U.S. Government hiding these records that Allen himself has never seen, yet are carried in the Blue Book case index? Where the hell are they kept?



**Paris. Rue de la Mesange. Sunday 12 November 1967.**

We are getting ready to move to Saint-Germain. I have this strange feeling, that I have died and that another "me" is getting ready to slip in from the icy depths of the galaxy, to take the place of the being who had so religiously kept this Journal until now. In Van Vogt's haunting novel *The Adventures of Null-A*, successive layers of being are revealed as the main character keeps dying, only to be replaced by more sophisticated versions of himself kept in faraway sarcophagi under the guardianship of wise men. That is the way I feel, except that I have not met any wise men in a long, long time.

Berthold Brecht has said: "He who knows the truth and doesn't proclaim it is a murderer." But what can be said, then, of the man who knows the truth and proclaims it? Isn't he a greater criminal? Or a greater fool?

**Saint-Germain. Monday 27 November 1967.**

The workers leave the office at 5:30 p.m. I'm hungry as I go down to the wet streets. At Saint-Lazare station I swallow a big cup of coffee at the counter, with a crusty *croissant*. There are tickets to be torn off from the weekly travel card. Every day, in order for me to go from our house to the station, I must give two tickets to the bus driver. For the train, another little rectangle. To eat some sauteed veal with noodles at the Shell cafeteria, eight little coupons. What kind of life is this, a life measured in little pieces of cardboard, discarded one at a time?

If there is a "real" reality, it lies elsewhere. Not in Moscow, Paris or Chicago, not on this poor planet. It lies in a place where there are no coupons, no tickets, a place where people can think anything they like.

**Saint-Germain. Tuesday 28 November 1967.**

The company draws everything it can out of us, day after useless day. Yet we accomplish nothing. I feel very tired. Today we had a formal meeting about corporate files. It was a ludicrous waste of energy. It reminded me of the days when I was computing satellite orbits in Meudon, and when I spent long periods just listening to the dying drum before walking over to restart the dynamo, cursing the whole system. Like the Meudon astronomers, the managers here simply don't believe that

computers *can* manipulate their data in new ways.

For example, a well-educated man I will call Martin is in charge of the file of service stations. He is quite pleased with his way of doing things: if he knows the access number of a particular service station, the machine will regurgitate everything there is to know about it, including the owner's name and the color of the geraniums in front of the pumps. But if all he knows is that the owner's name was Dupont in 1965, he has to call two secretaries into the office, give them sharp pencils, and let them read the entire printout of the file until they find this Dupont. So why can't the computer itself look for D-U-P-O-N-T, I ask?

"Ah, but *Non, Monsieur Vallee*, that would be a name search, don't you see, and that would be, how shall we say, *linguistic*.... Computers are for numerical operations, and everybody knows that they cannot think."

There follow a few citations from Kierkegaard and some allusions to the concept of the Self in Sartre, and Monsieur Martin rests his case. Yet I never said the Univac in our basement could think, I only said that I *could make it* look for a name just as easily as it could search for a number, but nobody will listen, it's too scary. These people only know about advanced computer techniques through the garbled accounts of American research they read in fake intellectual journals like *Le Monde* or *L'Express*. They mix up recent reports on artificial intelligence, on automatic language translation, on software trends.

I hate those long draining meetings where executives parade and posture, resolving nothing! All the real decisions are made informally in the hallways, between two doors. The director who hired me for this department, a gifted man who had a clear vision of the inadequacies of current computer techniques, is about to be gently pushed out. The concept of a modern data-base system for this company is too revolutionary, it threatens too many ideas.

In previous years I always awaited winter with such joy! Now I do not feel any enthusiasm. On the contrary, I am apprehensive about the drizzle and the fog of every new day, the push-and-shove of every trip in the crowded bus, the crowded train, the crowded metro. There is so little for me to do when I finally get to work in that magnificent office building near the Arc de Triomphe!

**Saint-Germain.** Tuesday 5 December 1967.

I have not been completely forgotten by my friends after all. Some strange noises still *filter* into my retreat from the outside world. A man named Andrew Tomas who founded the earliest civilian UFO research group in Australia is now in Paris following a *two-month stay* in Moscow, where he was doing library research. In our phone conversation he presses me to meet with him, saying that he has a message for me from **Kazantsev**. Tomas is hinting that the Moscow group may soon be disbanding.

Allen has sent me his *Playboy* article.<sup>4</sup> The text gives an impression of confusion both in his goals and in his methods. He keeps apologizing needlessly. And this unholy association of Hynek's lofty astronomical arguments with the magazine's glossy pictures of luscious wenches in various states of sexual availability is not going to help his cause with his conservative colleagues back at Harvard.

**Saint-Germain.** Wednesday 13 December 1967.

These are bizarre times. The joy of Paris is gone. The men and women we see in the streets are bitter, unhappy. We work hard on our little house: painting, replacing broken window panes. Janine is sewing. I have glued a large map of the Paris area over one wall of the little room that serves as my office. I am cutting and nailing shelves, re-aligning my precious books, playing with Olivier, building casdes and extravagant cars out of Lego blocks.

The social situation is getting tense in Paris. Electrical and gas workers are on strike. My respect for Shell as a company is growing, however. They run a very mature, intelligent organization, except in the computer field, where the technology eludes them completely. I continue to implement software among the indifference of my managers, who have no intention of ever using these new tools.

I am also concerned about the apparent change in Aime, who five years ago was full of ideas for research, hypotheses to check, studies to conduct. Today he refuses to admit that we are dealing with a problem whose nature we simply do not understand. He seems to be hoping for the ultimate landing, a grandiose and apocalyptic manifestation that will prove to the world that he was right. Does he expect the Cosmos to disgorge a whole tribe of Martians just to vindicate us?

**Saint-Germain. Saturday 16 December 1967.**

Aime Michel wants to publish an article in *Planète* to expose the Condon committee. Such an article would be counter-productive, I argued. Instead we should give the public a positive outlook on the question, and some tangible facts. He complained about the deviousness of the United States.

"Why is the U.S. always blamed for everything?" I asked him. "I don't even believe the French Air Force is telling the truth about UFO research here."

"Well, we have Colonel Clerouin," he insisted.

"A puppet. Has he ever told you anything reliable?"

He seemed annoyed. "Who has the control of the files in France, then, in your opinion?" he asked.

"Probably an organization that reports directly to De Gaulle."

He whistles: "Those are not people you can approach just like that. I ought to talk to Bergier. Perhaps he will have an idea."

He went home, taking with him a bunch of *notes* about the Prague astronomy meeting, the Condon committee and the shadowy role played by Bob Low.

**Saint-Germain. Sunday 17 December 1967.**

I went to Neuilly today to visit Andrew Tomas, an Australian researcher of Russian origin who is in Paris for several months, finishing a manuscript.<sup>5</sup> It turns out that he was in Moscow when my article with Kazantsev was published and when the Stoliarov committee was created. Tikhonov and Kazantsev have asked him to brief me confidentially on the true state of affairs, but they don't want me to repeat anything to Hynek, whose credibility in their eyes has been shot down by his *Playboy* article about a UFO gap. This puts me in an awkward position.

As I already knew, Kazantsev and Zigel have created an "Org-Komitet," an organizational committee, with Tikhonov as secretary and Stoliarov as President. This committee was under the House of Astronautics and Aeronautics, a structure made up of retired pilots, young people and aerospace enthusiasts, not professional engineers or "real" cosmonauts.

The Stoliarov committee had an office in one of the Kremlin build-

ings. It only needed two signatures to become official. One of these signatures had to be given by the Air Minister, so Kazantsev and Zigel duly presented themselves before him and explained their purpose.

"Very interesting," said the official. "How can I help your research?"

"One thing that would be useful," proposed Zigel, pressing his luck, "would be to ask your men if they have seen anything in the air that hasn't been reported."

"Excellent idea, Comrades! It's such a simple thing."

The Minister pushed a button and a staff officer came in, snapping to attention.

"Order to all Air bases in the Union of Socialist Soviet Republics," the Minister started to dictate. "Report at once any observations of unidentified flying objects made by your personnel." Zigel and Kazantsev left the Kremlin elated, secure in the promise that they would be kept informed.

Forty-eight hours later their committee had ceased to exist. The Minister had been swamped with fifteen thousand observations, making the officials realize just how many sightings have happened. Military secrecy fell over the whole business.

Andrew Tomas told me all that in English, as we sipped the excellent coffee prepared by his landlady and as we looked over a small frozen courtyard where a pigeon alighted from time to time, looking for bread crumbs. He added some details about the impact of the publication of my article with Kazantsev:

"Do you realize what happened when it was picked up by *Trud*, the labor union newspaper? *Trud* has a circulation of twenty-two million daily copies. Word quickly got around that it included an article about UFOs, and that issue disappeared from the streets in a very short time."

He also told me a long, intricate story of a London woman who is said to have intercepted a Morse message between an extraterrestrial and a British physicist. She went secretly to the appointment, saw the whole thing and revealed her presence. The rest of it is an undocumented tale with the usual garbage about Aliens who come from very far away and learn our language while they sleep during their long trip. It happened in 1962 and the woman's name was Mrs. Marchwood. I listened politely to all this, took my leave and went to catch the bus that would bring me back to my computer.

**Saint-Germain. Christmas night 1967.**

Ten years ago I was beginning this Diary. My father was living his last days. Only a vague presence of him is still left: a certain strong tenderness I find in nature around me reminds me of him. Other beings have come and blessed my life: sweet Janine and Olivier. As I write I can hear my son in the next room, telling long stories to his teddy bear.

We are little people, with a small rented house, a little garden. We do not even own a car, or a telephone. We keep our secrets well. Nobody notices our happiness.

This afternoon I took a long walk alone towards the Marly forest. In Fourqueux I climbed all the way up to the windmill. From there I could see the whole of Paris and its suburbs. The big heavy bulk of the black clouds was rushing across the gray sky. The tepid wind blew wet on thick crusty layers of dead leaves, the beaten earth, the trembling clusters of mistletoe in tall black trees. I could imagine myself in a few years, exploring this little corner of the planet with my children, taking them for Sunday walks, drawing charts for them.

Yet all of a sudden something snapped and rebelled within me. I faced the harsh wind that pulled tears from my eyes. Have I not plotted out the map of Mars for Project Mariner? Here I am, planning to become comfortably buried in the dreary life of a French bourgeois, drawing little charts of some sleepy suburb: the creek that flows towards Chambourcy, the last little sliver of woods near Saint-Nom-la-Bretèche\_\_\_\_ Have I forgotten Syrtis Major and Sinus Meridiani? I must choose: in which time, on which scale do I want to live? Back in the United States, Saturn rockets are climbing straight up in the sky. And here I am, wondering if I will ever own a little cottage of my own some day, how I can save enough money to buy a car, in two or three years\_\_\_\_

So many people have asked before me, "Who am I?" I can only answer "I am you" when I see the elderly man who stopped me in the street to ask me which day of the week it was, the tired worker, the learned bishop, they all have a brother within me. From this crowd I seem to carry inside myself rises a simple, corny, inebriating feeling of love for the whole planet.

On weekday evenings, when I come back from Paris, the blue bus from the station stops on the square, and I get out, hungry and frozen

with fatigue. A few office workers get out at the same time as I do. The bus leaves us there. On the little square I am worthless, harmless, alone.

**Saint-Germain. Sunday 31 December 1967.**

The present should not be tolerated. In the tiny home office I have built in our spare bedroom, I have gathered a whole world of maps and books, and fanciful toy machines with blinking colored lights. They encourage me to go on with my work in the gray morning.

I am reading Camille Flammarion's *Popular Astronomy* for 1881:

At last we have reached the scientific era that every friend of progress was wishing for . . . the shadows of the night are vanishing gradually. Clarity comes into the souls of men. This is a manifest, eloquent, indisputable sign of the current state of minds and of their aspiration towards true science, positive science; towards true philosophy, scientific philosophy. It is sweet indeed to see such noble tendencies win over our great human family as it makes its slow progression.

Perhaps Flammarion's naively hopeful words should be contrasted with a more recent text by Jacques Bergier which appeared in *Planète*.<sup>6</sup>

Our modern societies, with their foundation on democratic principles and their free circulation of information, may in fact have delegated essential responsibilities to a handful of men whose knowledge and power are kept hidden from the man in the street.

Consider revolutionary warfare, where the leader is a *shopowner* or a farm worker; consider science and technology, where language has become an esoteric jargon, where those who have the true keys of civilization remain strictly anonymous; consider the field of propaganda, where it is impossible to locate those who are plotting new collective psychoses. And let us not even talk about the police . . . the very nature of things is drawing us towards a *cryptocratic* State.

Poor Camille Flammarion! His view of progress could not have anticipated the White Storks of classified science! Tonight the souls of man are as dark as the long shameful guns which have briefly interrupted their thunder in Vietnam: New Year's Eve. In our little house near the forest we

will sleep quietly, the three of us, soon to be four in number. No great feast for us. We need no wine, no friends tonight. Only the intimacy of our love.

**Saint-Germain. Thursday 18 January 1968.**

"What is the role of the Delegation for Informatique?" I asked a member of this exalted body which oversees Plan Calcul. By meeting with people like him I am keeping my network of contacts alive in case I decide to change jobs.

"We report directly to the Prime Minister," he began with a competent tone that made tricolor flags wave in my mind in anticipation of great deeds. "This gives us authority ..." he smiled ironically before continuing: "... to write to the other departments to exercise certain pressures. You see, the stationery of the Prime Minister always makes a big impression on other people. So we use it to influence their decisions when they are about to buy a computer. We try to make sure it is a French machine."

I cut short our meeting and I walked away with disappointment bordering on disgust. What they call French machines are nothing but refurbished designs from the United States on which a nationalized company, the old Machines Bull, does little more than paste a blue, white and red label. There are many smart computer people in France. With some decent management behind them, European hardware experts could put together a world-class computer. Instead, taxpayers' money is being used to finance the marketing mistakes of State-controlled companies whose history has been marked by a series of failures that would have destroyed any normal business long ago.

It so happens that I currently share my office with one of those excellent French specialists. I call him Mouse-Face behind his back because he is slight and sly, with a cynical sense of humor. A few months ago he was developing software at Bull. They had planned an ambitious line of machines designed to compete against IBM at lower prices. After years of waste and politicking they have accomplished little more than producing computers that have lower performance, higher costs and poorer software. Time-sharing, which is the wave of the future in computer applications, is not supported by Bull while IBM is already introducing it on its newest machine, the 360 model 67. Who will buy the Bull product? Five or six administrations, unless they have enough clout to resist the



pressures that are placed on them by the Delegation for Informatique writing threatening letters on Prime Minister stationery.

A letter from Hynek says he is thinking of coming to Paris in March, when he will be travelling to yet another astronomy meeting in Prague. He may bring Fred Beckman.

### **Saint-Germain. Monday 22 January 1968.**

Aime Michel came to visit me in Paris this afternoon. We went to a nearby bistro. I have given him Kazan tsev's address, and they have started an exciting correspondence. Now Aime has written to one of his contacts that he is now in touch with both the U.S. and the U.S.S.R. on the UFO question, and that this "puts him in an awkward situation as a French citizen." Hence he would like to disclose this to a higher governmental level.

It is a childish tactic—the French call it *faire l'âne pour avoir du son*, or "pretend you are an ass in order to get some rye." But given the current state of affairs, it just might work and get him in contact with some of the French Government agencies that have not yet tipped their hand. Indeed his friend has answered: "I can put you in touch with the SDECE," adding cryptically, "That might enable you to expand your files of sightings."<sup>7</sup> Does that mean that the French secret service, the SDECE, is maintaining its own UFO files? An officer is supposed to call him soon.

### **Saint-Germain. Friday 26 January 1968.**

"My name is Commander Granger," said the man as he shook Aime Michel's hand yesterday. He was *young*, blond-haired, with an open *face*, "the kind of fellow to whom you feel like telling your whole life story." Well-trained: he took no notes.

"If I understand you right, you belong to a small group that is in an interesting position because it has contacts both with the Americans and with the Russians."

"Strictly on a personal basis, I should add," said Aime Michel, "since we know very well that in France nobody takes this topic seriously."

"You are wrong to think such a thing!" said Granger. "There are some high-level people who follow the issue. Myself, I have always suspected there was a link between UFOs and military reconnaissance satellites."

Aime Michel gave him his most astonished look: "Satellites? In 1947?"

## MAGONIA

"Well no ... obviously, not back in 1947."

"Frankly, we have always hesitated to talk to French authorities, because we felt that other nations would find out about such contacts right away."

Granger was piqued: "Well, Monsieur," he said with dignity, "it is not for other nations that I work."

"And if you did, would you tell me?" asked Aime with a smile.

He then told Granger about the precarious situation of Project Blue Book (a situation every *ufologist* already knows) and the predictable Condon committee fiasco. He was struck by Granger's lack of knowledge of the simple administrative channels followed by UFO reports not only in the U.S. but in France. For example he thought that the Gendarmerie had orders to report every sighting. His real interest was in the area of satellites, especially infrared satellites. He didn't care about UFOs at all, concluded Aime, who had nothing to tell him about satellites, so he brought the conversation back to his own interests:

"Back in 1953 the U.S. Government gathered together five eggheads who leafed quickly through a few sighting reports during two afternoons and concluded there was nothing to the whole business. This conclusion was immediately accepted by the Air Force without any discussion. Yet the military intelligence services of the same U.S. Government had been studying the same reports for five years without finding any explanation. Now, you're in that business, *mon Commandant*: doesn't this seem a little bit odd to you?"

Granger just laughed.

"*Anyway*, we are *not* asking anything *of you*" concluded Aime. "We only want to be sure that we are not acting stupidly in our correspondence with the Russians. Your presence here is a guarantee that we have fulfilled our responsibility as citizens, by informing you."

"What would you like for the future?"

"Again, we are not asking for anything. And we have nothing to offer, unfortunately, since France is not interested in the field—"

"Well, if both the Americans and the Russians are doing something, we've got to do something too, of course."

"Some of the best specialists in the field are people like Plantier, Guerin, Garreau, Vallee and myself, who are French, but we work without any official recognition or support. We see three possible scenarios.

First, you could find me a job with some Ministry where I would have an administrative cover to spend some serious time on the problem. Second, you could help us formulate some guidelines that the Government could implement to improve information collection, such as an order for the Gendarmerie to report any local sighting known to them. Third possibility, a small group of scientists could be discreetly created to continue research."

Granger promised to report on this conversation and to let Aime Michel know through Rocard if his superiors felt the contact should be pursued.

**Saint-Germain. Saturday 17 February 1968.**

As I return from a three-day management training seminar organized by Shell in Rambouillet, Janine shows me a surprising number of items that have arrived in the mail: a complimentary copy of Ted Bloecher's interesting book about the 1947 wave, dozens of letters (a physicist wants some details from recent observations for an article he is sending to *Science*, another man proposes the theory that UFOs are linked to geological fault lines) and two UFO magazines from America. Why are people so slow in forgetting me?

To dispel my feeling of distress I often walk through Paris in the evening, pausing to observe and to absorb. Yesterday in the gardens behind Notre-Dame I followed the course of the whitish clouds racing along the rugged stone roofs in the cold night. Last week, at the Beaubourg plateau, I rested my back against an old wall where posters torn from the decaying plaster were beating in the wind. At the edge of tears I saw the moon racing; its reflections on the uneven cobblestone answered my despair: what am I doing here? The promoters who are about to tear down Les Halles promise to build fine gardens for us some day; we will have beautiful walkways with unbreakable plastic steps, multi-level cultural centers made of concrete slabs. We race away, the moon and I.

**Saint-Germain. Thursday 22 February 1968.**

In the middle of the afternoon I came home, skipping the second part of an "information seminar" organized by a consulting group on the flimsy topic of decision tables. Shell had delegated me to this assembly of friendly young technocrats. They gave us plastic briefcases.

I now realize with a shock that, if the French lag behind the Americans, it is not because we are poorer or less well equipped. Many leading French companies would be regarded as large firms on the **American** scale. My employer, for example, owns late-model, multi-million-dollar computers, housed in a magnificent facility two blocks away from the Arc de Triomphe, in some of the most expensive real estate in the world. We have a large *systems* staff that would do honor to any U.S. enterprise. The gap is neither financial nor technical. The gap is a mental and cultural one. Two months ago I would have fiercely denied this. But all the presentations I heard today were based on timid interpolations of half-understood American models, and speculation about American developments—like Monsieur Martin's refusal to let our computers perform "linguistic" searches.

In the newspapers are pictures of the destruction of the Hue Citadel in Vietnam by Lyndon Johnson's guns. That image is in every mind: the sepulchres of venerated medieval rulers blown up by U.S. bombs; ancient cultures razed by the blind and pitiless advance of that American civilization which is condemned to destroy the past, that American civilization to which we belong, even if it is fashionable among French intellectuals to deny any connection with it.

Every morning when we wake up and turn on the radio we find that the world in which we try to live has been covered with a little more blood.

In Chicago, *no matter* how dull things were, I had the feeling that I was making a contribution that mattered, that others would work better and faster thanks to my own efforts, that their lives would change. Here I have one of the most desirable technical jobs available in France, but I am not helping others make progress. This morning I heard a manager make a little speech in which he thundered against those Frenchmen who refused to work below their level. "They demand an attractive job, as if jobs should be attractive!" Everybody would be happier, he argued, if they simply continued to labor at those things they were trained for.

### **Paris. Thursday 7 March 1968.**

This morning I saw Aime Michel again. One of his friends has heard that the French Air Force was about to set up a real UFO investigation group.

"You ought to be able to check this with Clerouin," I told him.

"It may just be a trial balloon. Did I tell you what I heard? The French military did a counter-investigation of the *Socorro* landing: They concluded the object was a postal plane, a mail-carrier!"

"We are wasting our time with those turkeys. They swallow anything the Pentagon is telling them. The Russians are not doing any better, you know. I have just heard from Kazantsev. Maybe he wrote to you too? A committee within the Soviet Academy of Sciences has met to discuss the issue. They have concluded that the anti-science propaganda that surrounded the problem was aimed at sowing trouble among the proletariat."

### **Saint-Germain. Saturday 9 March 1968.**

Great news: Aimé Michel now confirms the fact that a research committee is about to be created by the French Air Force. The order has come down from none other than General Ailleret, who is De Gaulle's Chief of Staff. In addition, Granger has sent Aime a note to set up another meeting in Paris, "as long as his presence was not interpreted as official approval."

### **Saint-Germain. Sunday 10 March 1968.**

General Ailleret is dead. His *aircraft* crashed five minutes after it took off from the island of Reunion. Nothing is known about the circumstances. A nurse who was on board is said to be the only survivor.<sup>8</sup>

### **Saint-Germain. Monday 11 March 1968.**

A telegram has arrived from Chicago: Fred Beckman will be here on Wednesday morning along with Hynek. Andrew Tomas writes to me that Tikhonov and Kazantsev miss my letters, "but now that the news is bad perhaps it is better *to* remain silent." He adds:

I have before me the 29 February issue of *Pravda*. Three members of the Academy of Sciences condemn UFOs, following a recent report against "saucer propaganda" by Artsimovitch. They say there isn't a single fact that proves their reality. They quote Menzel, and they claim ~~that~~ U.S. scientists are unanimous in saying the subject is pure hogwash.

## MAGONIA

Tomas points out that in any other country such an article would not be cause for concern, but in the Soviet Union it means that our friends had better be careful.

### **Saint-Germain. Wednesday 13 March 1968.**

Hynek and Beckman's flight from Chicago was four hours late. I only saw Allen between two planes, on his way to the meeting in Prague, but he promised to stay longer on the way back. Fred remains in Paris. Maman has agreed to give him the guest bedroom in her apartment, since he doesn't have much money to spend on a hotel. Janine and I have spoken with him all evening. We came away with the feeling that no progress had been made in Chicago since we left.

### **Saint-Germain. Friday 15 March 1968.**

Yesterday morning Fred and I met with Andrew Tomas. Afterwards we walked over to an exhibition of naive American art. Paris is gray and gloomy, the news of the world is worrisome: financial markets have been in turmoil for the last three weeks, the monetary system seems to be crumbling, the London gold market is closed.

### **Saint-Germain. Sunday 17 March 1968.**

Hynek arrived at *noon*, all excited *to* be in Paris again. He wanted to live "like a native," so I took him to a small hotel near Place Monge. While he was getting settled there I had lunch with Maman and Fred, and I went out again an hour later to pick up Hynek. Upon returning we witnessed this wonderful scene: Fred still at the table, seated in front of an assortment of cheeses, encircled by five old bottles, his lips moist with Armagnac and his eyes glistening with pleasure.

Hynek told us about the turmoil he had just witnessed in Prague, the extraordinary changes taking place in Czechoslovakia, the hopes for reform. With visible regret Fred finally rose from the table. I took them both to show them the ancient Roman Arenes, and later we went to see the Gauquelins a few streets away. Allen, who had courageously seconded my recommendation to bring their book to the American public, stayed with them to discuss the "Mars Effect."<sup>9</sup>

**Saint-Germain. Monday 18 March 1968.**

Hynek acknowledges he feels an emotional need to get even with Menzel, Condon, Klass and the rest of the skeptics. His colleagues' attitude towards him is changing to the point of contempt, and this pains him. He is not taken seriously among astronomers any more. Perhaps that is more a reaction to his style, his brash articles in magazines like *Playboy*, than to his ideas. But does he really have a choice, given the fact that scientific journals are refusing to print anything, anything at all on the subject? Even our own *Challenge to Science*, which is a dry and factual scientific monograph, got no review in the academic press.

At the recent ceremony for the tenth anniversary of *Sputnik* they could not avoid inviting him to speak, but the organizers took him aside and told him to be sure not to mention UFOs. He felt so humiliated that he almost stalked out of the room. He only stayed there out of consideration for his wife and his former staff members, who had come to see him again. But he had a heavy heart.

He also told me about his meeting with Pentacle. He did not see him alone, as I had recommended, but in the presence of four of his Battelle colleagues. When he started reading from his notes, Pentacle snatched the paper out of his hand and said it was an old story, it was all over and forgotten. Pentacle got rid of him as fast as possible, but he did not return his notes. Always fearful of confrontations, Hynek left Battelle with his tail between his legs. Such a violent reaction may in fact indicate that something important is going on. Why should Pentacle worry so much about a simple letter written fifteen years ago?

I took Allen to Les Halles for supper. He was terrified when he found himself among the crowded little streets, in the midst of so many odd and suspicious characters, and he remained nervous in spite of my attempts to explain what made the area so unique and so wonderful. I argued that poverty should not necessarily be equated with crime or squalor, that the poorer areas of Paris could have more dignity and shine with a more special light than the grandest of palaces.

After I converted him to my views, with much quotation from Victor Hugo, I had to confess to him that the newest crop of French technocrats was about to raze this wonderful den of decadence to build a cultural center and a shopping area out of plastic and aluminum. At least, after Les

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Halles are gone, I will be able to keep the memory of Allen Hynek walking with me through these dark and convoluted passages from another age. I told him of the esoteric significance of the Church of Saint-Merri, the alchemical history of the Square of the Innocents and the gargoyles of the Tour Saint-Jacques which look down on the Great Work of Nicolas Flamel. Our conversations continued as we passed among the shops, the bars and the haunts where the prettiest prostitutes in Paris used to pirouette on the sidewalks. All that has changed. To the laughter of Paris, prudish Madame De Gaulle ("Auntie Yvonne") has decided a few months ago that Catholic morality demanded the "cleaning up" of this traditional garden of indulgence. Acting on behalf of every pious and pure person in France, Madame la Generale has made strict new rules to keep *Les Girls* in their hotels, out of the streets.

We had dinner in a little cafe where we sat next to truckdrivers and elegant aristocrats coming out of the theater.

### **Saint-Germain. Wednesday 20 March 1968.**

Hynek went back to see the Gauquelins to discuss astrology and destiny while I had a long talk with Fred. I told him frankly I saw little hope for the future of UFO research. Perhaps we should write a theoretical piece, Fred and I, with his friends from Argonne Laboratory, to outline what we think should be done. But in trying to publish it we would surely run into the Condon committee brick wall.

### **Saint-Germain. Thursday 21 March 1968.**

A wasted day. We met in the morning. Hynek has spent most of last night visiting every tourist trap in Paris with Mrs. Ackerman and he is very tired. Fred is not faring much better. They both seem exhausted and ill.

There are mounting social tensions around us. Yesterday rioting students broke the windows of American Express on rue Scribe. Peace talks between the U.S. and North Vietnam are supposed to start soon in Paris. Vaguely discussing these developments, we ordered some chocolate and Hynek started writing a letter. He was so distracted that we could not get a single meaningful word out of him. He only told us that tomorrow he had to make sure to get to Orly early enough to buy a few toys and some duty-free booze. I became discouraged and left. There was so much we should have discussed!



**Saint-Germain. Saturday 23 March 1968.**

They are gone. I felt bitter when I came to meet them at the hotel yesterday. As we waited for the cab to the airport Hynek asked:

"Are you still getting sighting reports from the public?"

"What would I do with them?" I said.

He seemed taken aback. "How do you mean?"

"Well, we don't have any way of studying them or of following up, do we?"

"It's important to keep these reports," he mumbled vaguely, "because it's science."

He must have realized he had just said something silly because he added:

"In the coming year I want to build up a file with a number of cases that are air-tight, something I can throw against the *Condon* Report."

"Isn't it a **little** late for that?" I asked. "We have been talking about 'air-tight' cases for years. The Condon Report is going to be a public relations fortress built up by the military, the press people, the **scientific** establishment, accompanied by a fanfare of editorials in major newspapers and the scientific press. How do you expect to counter all that?"

If he tries to respond to the report with his own arguments, Hynek **will** be greeted not just with skepticism but with irritation, no matter what good cases he cites as evidence.

Perhaps he wants to correct the bad impression left by his careless attitude yesterday. He now tries to get a serious conversation going, realizing that we only have a few more minutes together, and it may be months or even years before we see each other again. But how could we hold a serious conversation now, when the taxi has been called and could show up at any minute?

"Jacques, you gave me an excellent idea the other day. If I have a chance to answer Condon before Congress, I will say that his report **is** biased and incomplete because he didn't take the Pentacle episode into account. If they ask me to explain that, I will demand to go into executive session and I'll tell them the truth about the Battelle work."

I shrugged. "Maybe it would work."

We are not making any progress. Hynek is clearly afraid to go back and confront Pentacle. I will be very surprised if he demands to see the

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Drury film.

"What should I say if the Air Force tells me they didn't make a copy of it before returning it to the Australians?" he asks.

"Laugh in their face. Demand to see their correspondence with the Royal Australian Air Force."

"And if they say everything has been destroyed?"

"Go back to your UN contacts. Ask them to send an official request to the RAAF to get the original. It's worth trying."

"You have an answer for everything."

"Your cab is here, Allen."

Fred shook my hand warmly, promising that he would "think about the things we discussed."

I was sad to see them drive away. During this visit I have had too few glimpses of the other Hynek, the profound and clever mind who appreciates the limitations and the biases *of science*. Before leaving he did remind me of the commitment I made in Evanston: that I would drop everything, return to the United States and be his lieutenant if he ever managed to obtain support for a "real" research effort on this subject. I reassured him on that point. Yes, I would gladly drop everything, set aside my computer science career, go anywhere to work on such a project on a full-time basis. But it would have to be for real. Not another Blue Book, not another Condon circus, not some academic pipe dream.

Allen is always surrounded with rich acquaintances attracted by his celebrity, his media charisma. They keep hinting they might give some funds for a private effort some day, but real support never materializes. These are wealthy people who want to be entertained but who understand little about the constraints and the hardships of real science work: idle widows who like to parade with a highly visible professor in their entourage, or industrialists with their own pet theory of extraterrestrial life who want someone to confirm their private fantasies. But Hynek is the eternal optimist, and his personal magnetism may eventually motivate some of these contacts to fund a real effort. So far, however, nothing has happened. No wise man has offered support, no secret agency has emerged from the shadows, no tycoon of industry has flown to Chicago with his key advisers to help us assemble a team that could crack this mystery.

**Saint-Germain. Sunday 24 March 1968.**

We woke up in a gorgeous new world today. I am sorry Hynek missed it. All he saw of Paris was grayness, haze, gloom. The French Spring suddenly arrived last night, putting a sweet and heady fragrance in the air, golden sunshine everywhere. At the end of the day a brief, gentle rain washed the earth, creating fresh smells, a breeze like the breath of newborn angels.

Olivier left for Caen today with my mother. We came back from the station, Janine and I, mystically close, carried through the streets by the same thoughts, the same love. The sun sweeps the forest where leaves are sprouting, and when we lie in bed it comes indiscreetly through the white drapes with the pink medallions and kisses us. A hush hangs over us, the expectation of new life about to appear, the treasured time of birth. Mine is such a secondary part in this sacred process—I envy Janine, who is initiated to such mysteries.

**Saint-Germain. Thursday 28 March 1968.**

We are faced with a technical dilemma in our use of computers. The pressures of business are forcing our company to install remote terminals in the South of the country to allow our regional offices to reach the central mainframe to update financial and marketing files in real time. Accordingly the firm bought some American equipment, but the French Government exerted its usual arm-twisting in the name of Plan Calcul, and demanded that we install an equal number of "French" terminals, a thinly disguised subsidy to various local firms whose presidents probably went to the same Grandes Ecoles as the Minister. We had to do it, because they blackmailed us by hinting they would deny us the telephone circuits we required. The result is that we now carry twice as much equipment as we should, at twice what it would cost a U.S. firm to do the same job. And the French terminals keep giving us trouble: many of them don't work at all and two of them have actually caught fire. I must be missing something. In what way does this help the French industry?

**Saint-Germain. Friday 29 March 1968.**

Aime Michel has sent me a confusing letter, full of news and of ideas. He has initiated a number of experiments with a woman clairvoyant

who claims to be able to "read" the personality of individuals from a simple photograph in a sealed envelope. She has already produced a psychic profile of Allen, but it is so general it could apply to any man with a beard.

His plan is to continue these sessions, including some standard targets to calibrate the medium herself, and eventually to expand these psychic "readings" to the UFO pilots themselves, whoever they are. It's an interesting project.

### **Saint-Germain. Wednesday 3 April 1968.**

Yesterday I was supposed to attend a seminar about a new programming language. I left the office early, intending to browse through some of the occult bookstores in the little streets around Odeon before going to the seminar. Near the Seine I *found* a shop I had never *seen* before. It was full of fascinating books. The man who owned it said he could locate for me any item in demonology, science fiction and other sulphurous subjects. He was willing to discuss sylphs and salamanders, elves and the ghosts of Flammarion's time.

Afraid that I would buy too many books if I lingered there, I left the shop after a while. I walked along rue Saint-Andre-des-Arts and paid a surprise visit to my uncle Maurice on rue du Cherche-Midi. Suddenly, talking *to* him began *to* appear much more important than listening *to* a group of consultants pushing some new software scheme.

Every visit to my old uncle is a discovery, a marvel. A self-made man without an advanced education, he took classes in draftsmanship when he luckily came out of World War I with little more than a foot injury, and later he went to dental school, which has enabled him to make a good living. But he is most adept at precision mechanical and electronics assembly. Always building some new device in his three-room apartment, his most recent idea is to analyze his dreams by recording anything he might say while he is asleep.

In his living room, with the window wide open over a courtyard whose walls are draped with ivy, he played some of the tapes for me with their curious disconnected sentences, their words out of context. We also discussed astronomy. He is responsible for my interests in that science, and the telescope I have often used to scan the night sky was a gift from him. He always has something new and unexpected to say, even at seventy-

six years of age.

A light rain started falling. The wind was biting as I walked back along the Seine.

**Saint-Germain. Thursday 11 April 1968.**

I spent the morning solving a tricky computer problem of remote interrogation of data-bases. I found a simple and elegant method to accomplish it. I returned from lunch fairly pleased with myself, with two hours to kill before my new software came back from compilation. My colleagues were only thinking of one thing, and that was the planning for their annual Summer vacation. So the time had come, I decreed, to execute a long-delayed project: visiting the Bibliotheque Nationale to browse through the masses of documents having to do with apparitions in general and Elementals in particular, a literature that presents some striking parallels with modern UFO lore. I have never had the leisure to do serious research on this formidable and complicated topic.

I had never visited the Palais-Royal and the area of the Bourse where Stendahl had lived, where Colette died. At the Nationale I filled out the customary application forms. From there I walked all the way to the Châtelet. Once I had reached the Seine I decided to see Notre-Dame in the tender green of the trees, and from there I started loitering along the Quays, where I found Tyrrell's book on Apparitions. Reaching the Institute on the Left Bank, I cut across the old streets to Odeon, still on foot, finding more bookstores everywhere, and I finally sat at the terrace of a bistro to rest my tired legs and eat something before returning to my computer.

Along the way I kept wondering, what is the point of staying in Saint-Germain? We do not feel that we belong in this snobbish town where neighbor doesn't talk to neighbor. Added to this are the poor services, the need to walk all the way to the post office in order to use the phone, the bottles of butane gas to be dragged home for the antiquated heating system—We had not noticed any of this when we moved in, so pleased were we with the forest and the charm of our little doll house.

Research is what I want to do. What I seek above all is new knowledge, new understanding and eventually new wisdom. All day I have been ruminating about these topics, and about the long-term meaning of these computers of ours. They are changing industry and science, but

will they ever change Saint-Germain, where every house is surrounded by a high wall?

**Saint-Germain. Friday 12 April 1968.**

A journalist has asked General Nguyen Van **Hinh** what the French Air Force was doing to investigate UFOs.

"Nothing about this is secret or even confidential," said the General, who added some interesting administrative information. There is a "Prospective Bureau" within the Air Force where advanced topics are considered, including "numerous documents about UFOs" coming from bases of the four military regions, and "related phenomena reported by civilian or military crews, the Gendarmes, individual citizens and the press." Perhaps it be possible for us to work with them.

Unidentified flying objects appear spontaneously, often out of thin air. Yet they take the form of physical, perfectly tangible machines. Close to them the witnesses often see short humanoids with a large cranium, as well as human beings identical to us. But what about cases when these "entities" appear by themselves, without a craft? What about the cases of 1897, the landings of the Airship in the Midwest? What about the beings seen by honest folk in the days of Paracelsus? The great doctor had confessed his puzzlement before such stories.

Medieval folklore also mentions other reports: beings of light seen in the clearings, at dusk, close to "shining tents." They are rare indeed, because the witnesses were afraid of being accused of witchcraft by the exorcist, just as our modern witnesses are afraid of being accused of "obscurantism" or irrationality. Let's not even mention the cases of Fatima or Ezekiel: what we have here, whether we like it or not, are paranormal apparitions. I am reluctant to come to this conclusion: I have been trying to avoid it all these years. Yet I cannot refuse to consider it indefinitely. I have classified, analyzed, eliminated everything else.

My own sighting of 1955 in Pontoise is still present in my mind. I would be ashamed of the human race if we did not respond in some honest, authentic way. I also feel it is crucial to place the evidence on record, whatever it is, so that people will be informed in case of new developments. Aime Michel is right when he says that it's not enough to write books. But we disagree on what to do next. He wants to address the public, to hasten the moment of ultimate contact.... I do not follow

this line of reasoning. I believe that the skepticism of the bosses of science is irrelevant. They are the same people who, ten years ago, were equally skeptical of electronic computers, of artificial satellites. History has shown they are powerless to stop new ideas. The real problem is that we do not have enough reliable data on the nature of the phenomenon. We have no proof that we are dealing with extraterrestrials in the sense the ufologists imagine.

Yet the so-called "saucers" have too much impact on our culture not to be controlled by some intelligence. It is hard to imagine that it is only the result of some psychic projection coming out of mankind itself, as the psychologists suggest.

## **Saint-Germain. Tuesday 16 April 1968.**

Aimé Michel is back in Paris. I continue to have a deep feeling of friendship and admiration for him. His motivations include a need for vindication in the face of those who *deny* him the few resources he requires. He is not getting much help from the few powerful friends he *does* have, even though Rocard is *one of the* finest minds in French science.<sup>10</sup>

An interesting letter has arrived from Hynek. He is trying *to* find out what happened to the Drury film after all. He also tells me that John Fuller is about to publish an article in *Look* to expose the Condon committee as a major scandal. Anything can happen now.

## **Saint-Germain. Tuesday 23 April 1968.**

The U.S. Post Office has just forwarded to me a letter from good old Professor Menzel, who remains obsessed with flying saucers. Speaking of Father Gill's remarkable sighting in New Guinea, which I quoted in *Anatomy of a Phenomenon*,<sup>11</sup> he writes:

Gill was afflicted with myopia, probably a severe case, and failed *to* have his glasses on. Squinting severely distorted what he saw and, at best, the view was that of *an out-of-focus Foucault test* of his own eye. To verify my theory, I increased my own myopia by using a circle lens of 0.75 diopters and looked at a distant, bright porch light. As I squinted, my own eyelashes, irregularities in the layer of liquid on the surface, and simple diffraction made a beautiful UFO, with the legs on the bottom and people on the top.

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The argument is childish: What about the few dozen other witnesses who were with Father Gill on that day? Were they all myopic too?

Saint-Germain. Thursday 9 May 1968.

I have resumed my correspondence with Don Hanlon, with whom I feel I can communicate on the level of mutual trust. He writes to me:

The last time I was in Chicago, Allen, Bill and Fred mentioned that you had been rather silent recently, and in fact they thought your activities were suspicious. I expect Allen's visit has clarified all that.

This throws a strange new light on Hynek's visit to Paris: Could it be that he and Fred came to France to find out if our little secrets were still safe? Now I feel sorely disappointed in their lack of trust.

John Fuller has just exposed the Condon fiasco in *Look*. He reveals the evidence of a deliberate bias discovered by Saunders, Levine and Mary-Lou. It is worse than a bias, it is a smoking gun. An official University of Colorado memo by Bob Low, dated August 9th, 1966, discussed the research proposal they were getting ready to submit to the Air Force in the following terms:

*The trick would be* to describe the project so that, to the public, it would appear a totally objective study but, to the scientific community, it would present the image of a group of nonbelievers trying their best to be objective but having an almost zero expectation of finding a saucer.

Now I have learned how the expose unfolded. It appears that a meeting took place in Boulder, gathering all the critics of the Condon committee including Hynek and McDonald. But it is only after Hynek's departure that Saunders told McDonald about the memo, which had been discovered by staffer Roy Craig. Jim caught fire, charged ahead and used the memo immediately, in his typical "elephant in a china shop" fashion. This precipitated a crisis: Saunders and Levine were blown and fired from the project "for incompetence."

I find it interesting that the key players did not take Hynek into their confidence. He only learned of the memo through the article in *Look*. A general feeling of mistrust hangs over the whole scene. This is typical



of American ufology; why can't these people ever work together? Fred, who perhaps could enlighten me about the situation in Chicago, has not written *to us since* he came *to Paris*.

Last Monday I joined an interesting meeting of Shell specialists in The Hague as a representative of *Shell Française*. The gathering was organized to define strategic priorities for the Group in the software arena.

One of the top Dutch managers set the stage by explaining that there was growing international demand for better software within our sister companies. The meeting lasted two days and concluded with a fine dinner at an executive's home in a suburb of The Hague. Our host went around the table to ask each one of us what we felt was the best investment the company could make in the computer field. The unanimous answer was "a generalized data-base management system." In other words, *exactly the* kind of software I have been developing in Paris. Another scientist in Holland is working along the same lines. Our host looked very stern and there was silence.

Then he stated, "The Group is aware of this requirement. But we are not in the software business. We are in the oil business. All we can do is put pressure on Univac and IBM to develop such software." He is wrong, of course: the computer companies are only interested in selling "iron," not in developing new fancy tools for us.

We bowed our heads, as befitted young and well-educated Europeans when the boss had spoken. But there was a young Texan with us, and he felt no such constraint.

"Well now," he said loudly, "down there in Houston we spend about sixty million bucks a year on programming. I reckon we're in the software business."

I could have kissed the guy.

When I got ready to fly back from Holland I suddenly realized at the airport gate that I had foolishly locked my passport inside my suitcase.

"Well, don't you have some kind of identification?" asked a courteous Dutch officer, sensing my growing panic.

I fumbled in my wallet and pulled out my Shell personnel card. When he saw it he almost snapped to attention and motioned me to board. After all, anybody can get a French passport, but not anybody can be on the international staff of Shell.

Upon my return I found a deep change in the air. There is new tur-

moil in Paris: the students are restless, skirmishes have erupted with the police. There are random strikes in various industrial sectors. France is sinking into a deep malaise.

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## **Saint-Germain. Monday 13 May 1968.**

A General Strike has begun in Paris. I was in the cavernous computer room at Shell when the central power went off. Our two mainframes, with their flickering lights on the large consoles, and the rows of tape drives with their green and red panels glowing in the darkness, presented a magnificent sight. Calculations in progress were not interrupted: emergency power supplies kicked in immediately.

The amazing new fact is that the students and the younger workers in revolt are ignoring the directives issued by their own Unions, which they accuse of being just as bureaucratic, antiquated and rotten as the structures they were supposed to fight. The absurdity of the Regime has finally given birth to this surrealistic student movement that claims it will expose and tear down the fancy stage of French politics. So far it has done no such thing: it is purely an anarchistic uprising, without any constructive vision. Can it grow and become significant enough to play a role in the future? Where will it feed itself? Are we going to witness another historic upheaval ten years after the Thirteenth of May 1958, when the Public Salvation Committees of the Far Right precipitated a crisis secretly manipulated by the Generals men to bring him back to power? Perhaps this is the end of his reign. Yet the images deployed in this new, spontaneous storm are naive, idealistic and pompous, like the circle of red flags they formed around the Arc de Triomphe.

6:10 p.m. The big demonstration is over, according to the radio news I just heard. But it was only a beginning, everyone senses it. We keep listening with a thrilling sense of anticipation mixed with alarm. Suddenly the heart of Paris is beating in new ways. Something extraordinary is indeed in the air, even if it still expresses itself in tired old cliches. Everything now depends on the reactions of the factory workers, who

are heavily unionized. Their leaders are trying to take control away from the wildcat strikers who are challenging them.

6:30 p.m. The bulk of the demonstrators has just reached Denfer-Rochereau, near Paris Observatory (around the corner from Guerin's office), where they are supposed to disperse. Many people are going home, but the hardcore revolutionaries and some "anarchist" leaders call for renewed demonstrations. Student leaders have met with the chief of staff of the Prime Minister, who conceded all the minor demands that did not challenge the policies of Georges Pompidou. He denied all the others. But the students don't really seem to know what they want. They are calling for change, any change. The truth is that most Frenchmen are utterly fed up. I thought it was just me, a symptom of my own uneasy attempts to find my place again in the old country.

### **Saint-Germain. Saturday 18 May 1968.**

Last night the Saint-Lazare train station presented the spectacle of a huge steel carcass where the heads of the suburbanites quivered like caviar in a box as they waited for a few rare trains. Another surprise strike had disorganized the schedules, emptied the ticket offices, erased the wide departure boards.

Today the entire nation is in shock. The rioting students have changed all the rules. Other groups are following their example, learning from them. Employees are on strike at Renault, the trains are rare and people wonder: are these the growth pains of a new French society, or simply a rough moment as we are shaken up by the same waves that are upsetting the planet, demanding new freedom everywhere? The Czechs have their own revolt, American students are up in arms, China is in the grips of the Red Guards.

Should we try to tell the students who have taken over the Sorbonne that much of the turmoil of our age is precipitated by the new information machines? That this is part of an evolution of the planet's nervous system, beyond all political considerations? They would only laugh at us. They equate computers with what they call the "system of economic oppression" they want to destroy. They don't realize that the world has already bypassed such ideas just as it has bypassed the old bosses they want to send into retirement.

## MAGONIA

Saint-Germain. Sunday 19 May 1968.

Vague visions of our future: we will certainly not remain in France. We will move again, perhaps to Northern California, to *the* San Francisco area. Yet we are in no hurry to leave. Janine is due to give birth in a few days. And I still have research to do, my own private research in the dusty stacks of the huge National Library, if not computer research for French industry.

Saint-Germain. Monday 20 May 1968.

The time for Janine's delivery is very close, so I am reluctant to leave her side for fear of getting stuck in the huge traffic jams that block every road in and around Paris. Gasoline *is* drastically rationed. This morning, on the square in front of the Chateau, I waited for the Army trucks that are supposed to replace the striking trains. The convoy had been stopped somewhere, it never arrived.

The woman who sold newspapers was worried:

"It is going to get better soon, won't it, Monsieur?"

No, it isn't going *to* get better, because this particular movement is uncontrollable, unpredictable, unpremeditated, spontaneous. The leadership itself doesn't know what it wants or where it is going.

All we know is that the example *set* by the students *is* spreading everywhere in France: many workers have now seized control of their own factories, *locking out both the management and the Union leadership!* The strikers themselves are producing and distributing gas and electricity. There was a run on the banks this morning, and gasoline distribution is about to pass under the complete control of refinery workers.

8:20 p.m. This is fascinating. I was wrong, I misread the movement around me. An extraordinary upheaval is indeed in progress now. On Wednesday night, in the small town of Cléon in Normandy where a small firm *turns out auto parts for* Renault, two hundred *young* workers locked themselves inside the factory after hours. They kept the director in his office. On Thursday the younger employees of the major factory at Flins had heard *of* the action and the wildcat strike spread there: at the lunch break they locked the gates. When the Unions attempted to take over leadership of the movement, they were violently rebuffed by the workers themselves.

In an altogether different field, namely medical research, the same inspiration has found sudden resonance: the younger staff is fed up with the *Mandarins*, the old tyrants who run the major hospital departments like medieval fiefdoms. Thus Professor Soulié at Broussais hospital has just been sent home by a vote of his nurses and of the doctors on his staff. This is not simply a political movement, this is not ordinary labor discontent. We are seeing a reshaping of French society. Can it be effective? Can it last? Only the cobblestones of Paris know the answer.

**Saint-Germain. Tuesday 21 May 1968.**

**12:30 a.m.** Janine my love, I think of you in your hospital room. Tonight I came home alone. I was able to reach my brother on the phone in spite of all the turmoil. Always ready to help, he came over and picked up Olivier. I said good-bye to his little sleepy face as we rested him on the back seat of the car. And you my soul, you are in pain now, you suffer alone and I feel helpless.

**2:30 a.m.** Catherine was born an hour ago. When I reached the Saint-Germain hospital Janine was still in the delivery room. How tired and happy you were, my darling, when I kissed you! Our little girl looked at us both with her blue eyes, pulled on her ear, and spat.

**Saint-Germain. Thursday 23 May 1968.**

**10:15 p.m.** The labor Unions have tried to recapture the leadership of the social movement again, like a drunken cowboy trying to control his stampeding horse. Yesterday the CGT Union, under pressure from its Communist bosses, strongly disavowed the student demonstrations.

This afternoon I attended an in-house meeting for the Shell management and staff! Our president made no attempt to hide the seriousness of the situation and the grave concerns of French industrialists, which border on panic. Indeed, all our production units have stopped work, he said. We only have a few days' worth of gasoline consumption in inventory and seventy days' worth of fuel in the refineries, three-quarters of which has been processed. But no one could get to it without crossing the line.

A very recent event illustrates the unprecedented nature of the strike. The Shell refinery workers committee received an urgent message from the striking workers at the main Saint-Gobain glass factory: they were running

short on fuel and requested to be resupplied. The message, which bypassed the management of both companies, was very urgent indeed: when the temperature of a glass oven falls under a certain threshold the whole vitrification process stops. It is not a matter of re-heating the oven when the fuel supply is resumed. Instead, the entire factory has to be rebuilt around a brand new oven. This meant that unless Saint-Gobain could be quickly refueled, this major firm *would* be unable to produce any *glass* for a year or more.

What were the Shell workers supposed to do? They debated the wisdom of keeping Saint-Gobain in business. The majority expressed the view that since France would soon be under a Workers' Republic, it should have a first-rate glass industry. Accordingly, a special convoy of tanker trucks was organized. They opened the gates, and they kept Saint-Gobain in business.

The management of Shell is ready to negotiate an end to the strike. The stockholders want the company to get back to work at any price. The Unions agree: they would like nothing better than a settlement. The CGT quotes the words of an old Stalinist boss of the forties, Maurice Thorez, who said "one must know how to end a strike." But the strike is still going on, whether the Unions like it or not.

The historical reality is far different from *anything these* people have seen before. This evening a new, seemingly irresistible movement is springing out of the Latin Quarter. As I write this I am listening to the radio. Barricades are going up everywhere. The largest one is at the corner of Boulevard Saint-Michel and rue de l'Ecole de Medecine.

On the old Place Maubert, barrages have been built out of wooden crates. Indeed, Annick and I drove through the square at four this afternoon *on our way to see Maman and Olivier*. It was full of *these wooden* crates because the striking garbage men are no longer picking them up after the morning produce market. Now the students are setting them on fire to slow the advance of the riot police.

We found Olivier playing at the Arenes de Lutece, the ancient Gallo-Roman circus behind rue Monge where I took Hynek a few weeks ago. It was filled with kids released by local schools where the teachers, too, have gone on strike. Old ladies, as always, were sitting on the benches to catch a few rays of the sun while they were knitting among the confused youngsters on forced holiday.

When we came back from rue de la Mesange with *my son*, who had stayed there since Catherine's birth, we found a curious, colorful, excited crowd in the streets: it was the same explosive atmosphere I remembered from 1958, with buses full of riot police parked on the bridges, blocking access to the Right Bank. The sun was lingering in the cold sky. The crowd was restless.

At the Sorbonne and at the Odeon there are non-stop public arguments. "The Power has been taken over by Imagination," claim the new slogans. "It is forbidden to forbid." It's all a crucial experiment. I had assumed too soon that the recipe for change had been lost here. The creative genius of the French has been temporarily freed up at last following the bankruptcy of the bureaucrats.

Yesterday morning I went into a bistro for coffee and croissants. There are certain things which never go *on* strike in France, food and drink prime among them! At the table next to me a group of middle-aged workers were arguing loudly about "the events" around a bottle of red wine. It wasn't a question of right or left, of belonging to the Unions or not, someone said. It was the young against the ancient order: the young workers were our hope, and the young managers too, against De Gaulle and the Bosses, but also against the Unions, the Dinosaurs, "all the old encrusted fools," proclaimed one man.

"Listen to the young of today," he added, a cigaret dangling over his lower lip. "Do you understand what they say?"

"My daughter is fourteen," another one broke in, "believe me if you want, I can't even help her do her homework."

"There shouldn't be anyone over forty in the Government."

Last Friday, coming back from Le Bourget airport, the cab driver had told me the same thing in more direct terms:

"The young, the students, they have shown that they had the balls less mushy than all the old fogies who run the Unions."

Not everyone in France agrees with this view, of course. I have heard a shopowner thundering against one of the student leaders, Daniel Cohn-Bendit:

"As if we needed more Germans in our country, and a Jew too!"

Cohn-Bendit is the leader of the March uprising in Nanterre. Currently in Germany, he is barred from crossing the border into France. The students have gone back into the streets with signs that read: "We are

all German Jews!" It *seems* that *every* time the *demonstrations settle* down or run out of steam, the Government or the Police do something utterly stupid that ignites the riots again.

**Saint-Germain. Friday 24 May 1968.**

De Gaulle has condescended to speak, giving the people the benefit of his beautiful rationalizations. What else could he do? It is disheartening to witness this old soldier promising to the young France of 1968 eternal happiness in his tired arms while his riot police savagely beat up the kids of Paris at every opportunity using their wooden sticks and throwing tear-gas grenades.

The entire country is at a standstill. It is an awesome era, a suspended time as a modern nation stops dead in its track. We are lost in the time dimension, drifting along. Power is up for grabs.

8:45 p.m. New barricades are being erected. I am listening to a press correspondent who watches the *scene from* an apartment window in the Latin Quarter. The demonstrators have attacked the police. Trucks are overturned. The first grenade explodes. There are barricades around the Prefecture in Lyon. Riots have begun in Nantes.

10:50 p.m. Small groups are milling around aimlessly in Paris, without any specific objective. One such group made a move towards the Bourse, then wandered off... everyone wants to be down in the street tonight, to take part in the big change. But a change for what? To build a new world, claim the demonstrators. What kind of world? Under whose direction? No one is able to answer that.

11:10 p.m. The students have erected a real barricade on the Left Bank, made up of cobblestones, of iron grates, of overturned cars. There is a fight in progress on Place Saint-Michel. Thousands of demonstrators reach rue *Soufflot* with red flags and black flags side by side. Others have decided to return to the Bourse, the stock market area. In Lyon, more barricades are going up. Dozens of people have been *wounded* by the police, the radio reports scenes of looting.

11:15 p.m. High barricades are rising all over the Latin Quarter. At La Nation people, young and old, have gone out of their apartments to debate with the striking students around a heap of burning crates.

I wish I could go over into the city and join in this fantastic happening, but this is Janine's last night at the hospital. Tomorrow we bring



Catherine home, and my first responsibility is with them. So far Saint-Germain has not been affected by the turmoil. But this upheaval I follow minute by minute over the radio has taught me how little I understood, how poorly anyone had measured the depth of the French *malaise* of these last few months. I do not believe those who speak out now are especially qualified or competent, but history has thrust them forward. Not to be in the street tonight is to miss a great lesson, an opportunity to witness a raw process that is reshaping the entire nation in a few days.

**11:45 p.m.** The radio reports that the riot has turned ugly *on* Place Saint-Michel, that tear-gas grenades are exploding. A woman is screaming at an upper-story window. There are reports of people wounded on the Right Bank too. In Lyon a police official has been killed by a runaway truck loaded with stones. There are fights in the streets of Bordeaux.

### **Saint-Germain. Saturday 25 May 1968.**

Now there is talk of reforms to come. The government states that yesterday's riots were due to "the mob." Yet the general strike continues. Gasoline reserves are getting very low. We have difficulty finding such basic items as mineral water, or milk for Catherine's bottles. People have panicked. They are stocking up on sugar, oil and coffee.

Janine and Catherine are home with me tonight, and I feel very happy. My thoughts go back to our life in Chicago, and I dream of what we might find some day in California. If I really believed that something new was about to happen in France I would stay here to help build it. Erecting barricades is one thing, consolidating a victory and managing a new regime is another. I am just a bit too cynical to think that the current movement is capable of deep changes.

A tremendous psychodrama is in progress. It is healthy and generous, but the outcome may be just a lot of wind and idealistic proclamations. The movement gives no evidence that it has a vision of the future it wants.

In the middle of all this I still get news about UFOs. Galia writes to us from Moscow that one of her friends plans to visit Paris soon, and will bring us some information. New developments have also happened in Brazil in the affair of the "Lead Masks" which took place in August 1966. There is still no explanation for the large luminous object seen above the hill of Morro do Vintem by a society matron and her chil-

dren around the time when two technicians died. But the two victims have been identified as electronic technicians Miguel Viana and Manuel Ferreira da Cruz. They had been seen by several witnesses the evening before their death.

**Paris. Wednesday 5 June 1968.**

Today I was able to walk around Paris at last. All traffic has come to a standstill. The strikes have frozen the city. In the Latin Quarter, strikingly beautiful Anarchist women dressed in black hand out leaflets: they are calling for the ultimate uprising. On the boulevards a happy crowd goes about its business on foot. At Les Halles the confusion is complete. The naughty girls that "Auntie Yvonne," De Gaulle's sour and bigot wife, had tried to ban from the streets have reconquered their sidewalks. They victoriously swing their purses and display their generous cleavages among the debris of the rotting mess.

I ordered a cafe creme at the Capitole, my old hangout of student days. It is from this bistro, one evening in 1962, that I called Aime Michel to tell him about the statistical relationship between his files and the catalogues of Guy Quincy. It is this remarkably robust correlation that gave us the impetus to start a global analysis of the problem. I have come back to this area to locate a shop which is run by a family who has relatives in Russia. One of these relatives, a girl named Marina, has just arrived from Moscow. She is the friend of Galia mentioned in her last letter. I am hoping she will tell me what is going over there, and will explain to me the things a Russian citizen cannot write in a letter.

Marina is a smiling, attractive girl in her mid-twenties, loaded with presents for us from Galia. She tells me that the Moscow group has not disbanded, contrary to what has been said in the West. Indeed they now have two hundred members, with secondary groups in Kiev, Minsk, Tallin and Novosibirsk. They are still hoping to receive a formal authorization to become a State Institute for the study of unidentified flying objects.

**Saint-Germain. Saturday 15 June 1968.**

The great movement *is* over. The strikes have ended. Nothing has changed on the surface, even if the recent upheaval has revealed deep crevices underneath, gaping chasms, profound fault lines. I have resumed

my attempts to get a job with an organization interested in advanced computer techniques. I find a friendly reception everywhere. People like the fact that I know the United States well. I could go back to America on their behalf and bring back new products they could distribute here under license. But they all say the same thing: no research. Research is too risky, too expensive. Only the Americans can do it, they think. Wait for IBM.

My work on rue de Berri remains easy and flexible. All the ambitious plans made last year by the managers who hired me have been shelved or postponed. Talk of a "data-base group" was far too scary. Instead we have set up a committee on "basic data," which meets regularly to inventory the files and argues endlessly about abstract structures, but does not implement anything.

Sometimes I discuss this state of affairs with our field maintenance engineer. I discovered this mystery man one day as he was kneeling behind a tape drive, a screwdriver in hand. He comes from Kansas City and speaks no French. Univac has assigned him to the Shell account, so he practically lives in our computer room. This fellow is the man who keeps our center operating. He personally drives to Orly once in a while in order to pick up the new disk drives and the new printers he installs for us. Nobody takes the time to talk to him. He thinks the way we use his powerful machines is very funny.

I have the freedom to wander off into Paris while my programs are running. I spend as much time as I can at the Bibliotheque Nationale, reading, taking notes. I walk among the bookstores looking for works on hermetic traditions and folklore.

Last night, as I came home on the train loaded with rare esoteric volumes, the contours of a new book took shape in my mind. It would draw a formal parallel between the UFO phenomenon of today and the medieval traditions about elementals, elves and fairies. Indeed my current research shows these beings to be strikingly similar in their behavior to the alleged ufonauts. The UFO phenomenon, I will argue, is *folklore in the making*. No one has yet established the relationship between the landings of flying saucers and this mass of popular traditions about aerial beings down through the ages, their relationship to man, their role in mysticism. I am thinking of calling this book *Passport to Magonia*, after the magical country situated above the clouds, alluded to in *Comte*

de *Gabalus* and in the works of blessed Saint Agobard, written in the ninth century.

I have resumed my correspondence with Bill Powers, whose theory of the mind is progressing rapidly. And I am secretly dreaming of a day when I could do advanced research again.

**Brunville-par-Bayeux. Tuesday 16 July 1968.**

We are back in the fat land of Normandy. At night we hear whispering, crackling sounds, as if some giant aristocratic lady strolled invisibly through the countryside in her great velvet dress. The weather is cold and the grass is damp. The ponds fill up after the briefest shower, the whitish morning fog hides the hills. At seven in the morning Madame Fleury, our neighbor, milks her cows in the pasture under our window. A train rumbles in the valley. Last night the sky was so pure it was like a dream.

From her cradle my darling Catherine smiles at the flames in the ancient fireplace, at the red lanterns of Bastille Day hanging from the wooden beams. Olivier watches Madame Fleury milk the cows and he plays in the fields with his cousin Eric. Janine's sister has bought and renovated this big rambling farmhouse which dates back to 1642. The stone walls are two feet in thickness, built like the ramparts of a fortress. We are ten minutes away from the coast. In this conservative landscape cattle and dairy products are the only industries, wealth comes from the land and the poor are silent.

There are many temptations for us here: we could buy an old house like this one, overlooking the fat pastures; we could dream our lives, watching the great beautiful sky, raising a family of Norman kids as my paternal ancestors did, close to the earth. There is nothing wrong with such a plan. But we will not do it.

France has begun sinking into a new phase of the Gaullist regime, a paradise for the new rich, the *parvenus*, the promoters, the technocrats. The Think Tank companies, the purveyors of "gray matter," are making renewed offers to me: their idea is not to do research, but to grab new American products and to appear as great heroes by being the first ones to apply them here. It is a very stupid bet. It means that those French researchers who are capable of independent thought must leave if they want to flourish and create.

Whatever was left of the dreams that exploded so brilliantly in early May has now collapsed. The people who were endlessly talking on every street corner have run out of things to say. The Unions and the *Patrons* sat down at a big long table, shook hands and agreed. Everybody got a raise.

Any hope of a European vision is gone for now. There is no open avenue of dialogue, no concrete way to start implementing a strong Europe that could counter-balance the two major powers.

New elections have brought an overwhelming Gaullist majority back to power. Pompidou is gone. The General has appointed Couve de Murville as prime minister.

We are spending a fortnight here in Normandy, enjoying the easy hospitality of Janine's family, dunking great *tartines* of bread and butter into huge bowls of coffee mixed with hot milk. When we return to Paris I will go on buying books, saving what I can save of the old treasures, egotistically assembling an esoteric library to support my future work. And we will move West again, with fewer illusions this time.

I have typed up about half of *Passport to Magonia*. Compiling and analyzing this material, I see that we cannot hope to understand UFOs with the methods of physics and statistics alone. We are facing a form of intelligence that cannot simply be tracked with radars and cameras. It was already here before science was invented; it will still be here when science becomes superseded by other ways of gaining knowledge. Our soul is the only tool that is of any use in this search. Our soul, alas! What is left of it?

### **Brunville-par-Bayeux. Wednesday 24 July 1968.**

At sixteen I knew more about myself than I do now. Certainly I have learned much about other people, other countries, other ideas, but in the midst of all that jumbled knowledge I am in danger of losing the secret state of mind which enabled me to measure how truly small we were on a cosmic scale. Today, as I walked back from Bayeux through the fields, I had the feeling I was resuming the use of a part of my mind which had not worked for ten years. Indeed I have written, classified, coordinated, invented, programmed, managed and implemented. But I have failed to contemplate.

My greatest treasure: our children, Janine's love, and that which is still childish in myself. I have spent the last three days in the middle of a

grassy field building a plywood airplane for the kids, ten feet long, yellow and bright red, complete with a big propeller that spins proudly by itself in the wind, to the astonishment of the local farmers. I am young and a bit crazy, and that observation reassures me when I see what wise and serious people around me are doing.

The waste I witness in France makes me sad. Those who don't have an independent source of wealth are suffocating economically. I've done some calculations: if we took our savings of the last few years and we invested them here, we could buy a car, get a telephone, make a down payment on a small apartment. But we would quickly sink under the taxes and the fees those few possessions would bring down on us, although my salary puts me among the relatively affluent layers of the middle class. The French State penalizes you as soon as you start trying to live. The truly wealthy, of course, have many convenient ways to get around such frustrating problems.

On the stage of French society certain characters survive from one scene to the next, increasingly contrasted as if they were illuminated by the setting sun.

A celebration is planned here tomorrow. We expect doctors and lawyers with their wives and mistresses. No great inspiration rises from their ranks, no great sin, no great dark passion either. American novelists, who take such voyeuristic pleasure in describing experiments in immorality, would eavesdrop in vain around these alcoves. In many ways this is still the France of Francois Mauriac, not the France of Francois Villon. At dusk I can hear the dinner bell at a nearby manor.

On Wednesday I will be back in Paris, in my tedious office. Through the window I will see the vast gray courtyard, little rusty roofs spotted with pigeon shit and a few offices on the other side, where people constantly mill around, looking busy.

I have decided to use what is left of the Summer to complete the first draft of *Passport to Magonia*. After the holidays I will quit my job. A French Think Tank has made a firm offer to me. It is run by a man with a reputation as a clever politician who managed to buy the Control Data machine for which the Americans had denied an export license a few years ago.<sup>12</sup> But the job I am offered involves no new research. They all sing the same cowardly song.

I am tired of going through Saint-Lazare station every morning and

every evening, with the realization that thirty years from now, if I stay in France, I will still be taking the same train, seeing the same people.

**Brunville-par-Bayeux.** Friday 26 July 1968.

When the media strike was over, when television journalists were forced to return to work, a political commentator capitulated with these words:

"If I were twenty-five I would leave this country. But I am only a poor old man. Let us acknowledge our defeat. Let us rejoin the ranks of the mediocrity."

Coming back to France last year would have been one of the dumbest decisions of my life, if it weren't for the opportunity it afforded me to witness first-hand the historic upheaval of May, and to gather unique documentation for *Passport to Magonia*. Patiently I make the rounds of all the firms that use big computers. I find the same attitude everywhere. When you cannot do research you cannot do development either. Soon you lose the ability to do simple engineering.

Such failures are unimportant to French business leaders. Political contacts are everything here. Knowledge of technology and the ability to develop new products are irrelevant skills for the ruling class. They don't tell you: "We have here the man who developed the first Algol compiler, and the genius who solved the transfer equation for neutrons in nuclear reactors." Instead they try to impress you by bragging: "We have a fellow who knows everyone at the Department of Public Works, and one of our directors went to Polytechnique with the Minister of Health, and So-and-So has a father who has influence over the military budget." In the last few months I have found a solution to a significant problem in the management of data-bases: how to design large information systems that are self-organizing, reordering their data as a function of the questions the users are asking. Nobody seems very interested. Politics, not technology, is the name of their game.

At our office the director for computing recently invited me to an elegant and select lunch with a small group of in-house scientists. I discovered that our executive dining room was one of the finest tables in Paris. We were there to greet Paul Rech, a French expatriate who is an operations research specialist at Shell Development in California, near San Francisco. As we were popping champagne corks one of our bril-

liant mathematicians expressed his pleasure at Rech's visit:

"We have had some very interesting talks," he reported to our boss.

"Perhaps for you. But I doubt if Mr. Rech is going to learn very much among us."

The comment was flippant and unfair: our applied mathematics group has recently succeeded in solving a basic problem of optimization in integer variables that will save the company a good part of the cost of moving its railroad cars around, a problem that had stumped their American counterparts. Three months of savings represent the entire yearly budget of the applied mathematics group. Tomorrow the same computer program could save a similar percentage for the world-wide fleet of supertankers, millions of dollars a month.

Paul Rech, with whom I spoke afterwards and discovered a real sympathy, understood very well what was going on:

"It's typical, the reaction of your executive. He doesn't recognize how much his own organization is worth."

He seemed pleased to find someone with whom he could discuss America. I explained the events of May to him. We left the building and we walked across Paris to have coffee near Saint-Germain-des-Prés. The weather was warm. We sat outside at a terrace and watched the barricades which are still standing: it will be months before the work crews can clear the streets of rubble and return the Left Bank to normal traffic.

Paul told me about California, and I listened eagerly. We discussed computers, those strange machines from America that are responsible for the crumbling of the old structures in Europe. I remember Guerin writing to me "a good researcher behind a telescope will always be more economical than a machine," a terribly misleading statement. Today's computers are still only embryos, the first prototypes of a new social nervous system.

We have seen enough of France. There seems to be no cause we can serve here, no future in which we can participate, nothing we can build. In French society at large my skills are useless. To remain here is professional and intellectual suicide. To leave is to be reborn. With a lingering regret, though. France is a mistress who still inspires lust as one leaves her bed at dawn. I am especially sad about the failure of the dreams of May. That France of the Possible we briefly saw flash before us will never materialize.



# 18

## Edinburgh. Wednesday 14 August 1968.

Tea for two in Edinburgh, where an international computer science meeting is being held. Janine and I are making new plans, even as we meet old friends again. The conference was a perfect excuse to see Ben Mittman from Northwestern, and Jean Baudot who came over from Montreal. I had dinner with Bill Olle, the man who directed the implementation team for Infol, the first generalized data-base management language. It turned out he had heard that I had written a compiler for his language in six months.

By the time we were ready for dessert he had offered me a job in his new group at RCA. I hesitated, reviewing all the negative points: RCA is a newcomer to the computer business. Can they really compete head-on against IBM? More specifically, can their new Spectra computers take on the IBM 360 Series? Their facilities are in New Jersey, a region of America that has little attraction for me. It is of California I dream.

On the positive side, here is the chance to work with a team that thinks about the same problems at the same level I do. I would rejoin my intellectual family, the brotherhood of software developers that forms a sort of Freemasonry. New Jersey could be a stepping stone to other places.

After the Congress itself we took the time to climb Arthur's Seat. Janine bravely went up the side of the old volcano in her Parisian dress and high heel shoes, a scarf over her head to protect her hair from the mists of Scotland. From the top we could see the city and its fairy-tale castle, the little lakes and the Firth of Forth.

We have made new friends in the bookstores of Edinburgh: here, in the country of the Little People and the Good Neighbors of legend, we spent hours rummaging through treasures like *Celtic Magazine*, *Scots Lore*, the history of the Picts, and a collection of the early volumes of *Knowledge* magazine. My documentation for *Passport to Magonia* has taken another step forward.

**Saint-Germain. Friday 23 August 1968.**

The short roots that still tied me to the French soil are withering. Yet I cannot deny I still react to a poetic feeling in the air of Paris which moves me to delight. I have many friends in the crowd that walks along the Boulevards, and I love the shades and fragrances of the countryside. What I cannot stand in France is the greed and the spite of the upper class, even if I might have approached that exclusive circle some day.

I have gone back to the think tank that had offered me a job. If I did stay in Paris and joined their company, would I be free to lead a software research team into uncharted territory? The manager told me he was personally skeptical about generalized data-base systems: they were still beyond the horizon of the possible, as far as he was concerned. He wasn't even sure they were needed.

"Then, as you can see, it's better for me to go back to the States. I can always come back here when the need for such systems has become obvious to everybody!"

"*Mon pauvre ami*," he answered condescendingly, "if you leave you will never come back here. You won't be able to find a place again. All the good *situations* will have been taken."

It was my turn to be flabbergasted.

"Taken by whom?"

"Well . . . by those who, in France, will begin such applications \_\_\_\_"

People like him live by a simple rule: there is only one world and the center of it is Paris. Nothing matters except as judged by the standards of Paris, and no life is desirable unless lived in Paris. There are only a few "good situations," as he says, and these choice jobs are held in reserve for those who know how to maneuver cleverly within the system. There is no thought, as in America, of being rewarded for creating opportunities for others. Their idea is to grab the greatest possible number of privileges and then to slam the door shut, blocking the way for others, keeping them as far as possible from success, even if that means restricting one's own landscape, narrowing one's vision, sacrificing one's own range of opportunities, postponing one's achievements.

Such tactics don't work in the computer field because the technology itself will sweep aside its own leaders.

Now I can either leave Paris without looking back, or I can stay in

France as a member of the technocracy and never think about California again. There is also a third alternative, a compromise which many French intellectuals have chosen. It can be painted a variety of colors. Some like to brood in Left Bank cafes, saying: "Yes, I am a *rate*, a failure, but I am staying here to save what can be saved of civilization." Others claim: "I stay for the sake of my old parents," or "for my children," "for my insect collection," "for *my* little house in Aquitaine to which *so many* family memories are attached."

Another delusionary compromise is the revolutionary dream: "I stay here to help build a better world once this rotten system falls apart."

There is also the erotic fantasy: "My work isn't important to me, I *only* live for sexual freedom. What would the girls of Paris do without me?"

These people are spoiled. They have understood nothing of the upheavals of May, they have missed the fact that the whole social matrix has rattled and shifted under their very feet. No one can control the world any more. Every nation wants freedom, even if that means facing the tanks, as we just saw in Prague.

"Play for the Czechs!" demanded the public at a recent London concert given by a Soviet orchestra. After playing the *Symphony for a New World* the first violinist, a Russian, left the stage in tears. In Prague itself the same evening, a Red Army soldier committed suicide inside the University complex his unit had just seized. Ironically, while the world is passionately reaching for liberalization, the U.S. is about to put into the White House a conservative politician, Richard Nixon.

At the *office*, near the Champs-Elysees, the passing of *hours does not* concern me any more. This time flowing away around me is not mine. Last week, I spent a few days of poignant tenderness at my mother's apartment while Janine was back for a visit to her parents in Normandy. Maman lives beyond the Pantheon, in a six-storey building without an elevator. We spoke late into the night. Above and below us children played and cried, people came and went, doors closed with a thump.

Night came majestically in the apartment, draping its shadows over a desk my father built, over the red carpet and a fine black cabinet enhanced with copper angels and a gold border, and over my mother's paintings on the walls. She doesn't seem to change. She is a schoolgirl, delighted with all the adventures of everyday life as well as the grandiose claims of mod-

ern science, for which she has not developed any skepticism. Nothing unusual *or* funny escapes her sharp, ironic blue *eyes*. She attends an evening English class where she talks to other students fifty years younger than her, dispelling their sadness and their concerns for the future with her contagious enthusiasm. Recently she took my brother's two sons and his daughter to a science show about meteorites. Never did a better **grand-**mother fall from the sky.

**Paris. Thursday 12 September 1968.**

This morning I stopped for coffee at a sidewalk terrace on the Champs-Élysées and I went over our reasons for leaving America last year: a need to refresh our emotional beings, to find new poetry, to breathe the passions of Europe, to break up the routine of work in Chicago. I have satisfied all this, witness this manuscript I have just completed. *Passport to Magonia* is a book written in **English** and destined for the American public, yet I could never have written it in America.

I walked back by way of the *Herald Tribune*, where I successfully tracked down a reference to an aerial object seen in 1908. Then I came back to this office to stare at the ugly courtyard below my window. Day after tomorrow I will be in Bill Olle's office in New Jersey for a series of interviews with his team.

**Later the same day.**

Every Frenchman has a virtual placeholder, a little rectangular invisible box which represents his place in society. It comes into existence at the moment of birth and moves under its own power throughout **life**. Its path is defined by one's parents, their social position, their contacts. This past year I have discovered that it isn't important to actually reside inside one's own box. You can step off the plane at **Orly** and find some perfect strangers readily speaking to you as if you were their lackey, while others will treat you immediately as their boss. I could come back again twenty years from now and find that my box has accumulated seniority and privileges as well as restrictions; whether or not I am here, whether or not I work, love, experience and think, succeed or fail, it will follow a career path carefully charted for it by unknown forces. Some people find this reassuring. They will actually go on strike to demand such "security." But to me it is a terrifying vision.

It is only during the May insurrection that I have seen the French really living, with an unpredictable, passionate life, full of virtual powers. For a short time they felt inebriated by it, they delighted in their new-found freedom. Then they got tired and scared of their own audacity, and they sought shelter again under the skirts of their old General. A few contemptuous words from him and higher wages for everybody was all it took to force the system back into the normalcy of mediocrity.

I could easily stay here to manage a small technical project in poetic surroundings, but I know very well that I will not do it. Surprisingly, it is in today's *Figaro*, a conservative paper, that I find a striking image under the pen of Valéry Giscard d'Estaing:

France is a hexagon filled with pyramids. Like the Mexican pyramids, ours have multiple levels. Our social structures, thanks to their remarkable vigor, have succeeded in placing the same social class on the same levels of the various pyramids: it is the same people who manage, who execute or who suffer (whether they happen to be) in Administration, in Education, or in Industry. This historical monument is condemned by the forward rush of contemporary events.

It is a very appropriate metaphor. Yet I see no evidence that the pyramids are about to crumble. On the contrary, they seem more stable than ever. From their summits the high priests are heard chanting every day at sunrise.

### **Cherry Hill, New Jersey. Sunday 15 September 1968.**

The plane to Philadelphia circled Boston at sunset. No cloud in the sky. The Atlantic coast was below us, spreading its woods, forests and lakes, Manhattan looming ahead.

Now it's a quiet Sunday in the Garden State. From my hotel window I can see a perfect lawn and pretty flowers. Beyond the tall trees, long quiet limousines seem to float along the expressway. Bill Olle has promised to call me before noon. How wonderful it is to be in a country where the telephone actually works! Yesterday I called Allen Hynek. He told me he had in his hand my very latest statistics based on the Air Force files, which I had sent him a few days ago. He warmly recommended that I take the RCA job:

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"You can't imagine all the things that have happened since you left! I just got back from Boulder.... It would take too long to tell you. Have you seen the latest *Playboy*? Did I tell you I am writing a report about Project Blue Book ...?"

Next I heard the voice of Don Hanlon, sensible and vibrant. He has been discharged honorably from the Navy, but he just broke his right hand in a boxing match in Chicago.

### **Saint-Germain. Monday 23 September 1968.**

Last week I met the whole RCA data-base team in Cherry Hill. I discovered that most of them were Europeans. Bill Olle himself has a doctorate in astrophysics from Manchester in England. There is a fellow named Burkhardt who left IBM-France when they stopped working on advanced software. Others have come from Sweden, from Holland. It would be a lot *offun to work* with them.

The decisive meeting for me involved a discussion with Alonzo Grace, an engineering manager for RCA, who explained to me their computer strategy:

"In past years this company has made the key discoveries in color television, and we operate some of the largest communications networks in the world. IBM has no such experience to its credit, it has a lot to learn in those areas. Future projections indicate that ten years from now the most important issues in the computer marketplace will happen to be exactly in our domain of expertise: networking and display of large quantities of data at a distance through communications devices. We are not there yet, of course, but RCA can use its expertise to take a shortcut to that future world, well ahead of IBM. We will intercept them at the pass, so to speak."

It is a brilliant strategy, but *will* they have the resources and the *will* to execute it decisively?

### **Paris. Thursday 26 September 1968.**

Today a dull gray light comes down the well. It throws an uncertain reflection on the bluish office walls. I do not raise my eyes. To avoid seeing the plastic partitions covered with dust and grime.

On Tuesday afternoon the fat black telephone on my desk, which looks like a regulation revolver, rang suddenly. Bill Olle's joyous, energetic

voice intruded into my dull reality. He gave me the final details of the RCA offer.

My manager must suspect that I am looking for another job because he took pains to explain to me that my technical proposals "were under serious consideration."

He tried to make me feel better, as managers always do, by giving me the usual bromide: "You are not a number here, we treat you as an individual. You are part of our big family." He added that I should be patient, by 1972 the technical developments I was advocating would probably become a reality. "So maybe I should mark my calendar, and make plans to come back in four years?" I thought to myself.

Now I can hear the director of the programming section in the next office, cutting his fingernails: tick, tick, tick. After that he will take up personnel management issues: salary reviews, performance evaluations. If time permits he may even tackle some technical problems in codification, in which he is something of an expert. He dreams endlessly of universal identification systems. A good, harmless fellow, the kind who periodically proposes to the Academie Francaise to reform the spelling of every word in the dictionary, in the name of rationalism.

This morning I broke the news to my mother that we would go back to the States. She did not appear very surprised. At noon I will meet Janine to buy a tapestry we have seen in a shop window. We plan to take just a few precious things with us. I have found a copy of Dom Calmet's *Dissertation on Apparitions*.

### **Saint-Germain. Saturday 28 September 1968.**

Marcel Granger expresses the mood of many Frenchmen when he writes in *Adieu a Machonville*:<sup>13</sup>

No more green spaces, no more old mossy houses, no more fishermen with their lines, no more games of *pétanque*, and the Beaujolais no longer tastes the same. In the streets people don't argue any more, there is no time. On Sundays we are bored to death. The Saone river stinks of shit and despair.

With the eyes of a mere traveller on the earth I look around me. Is it right to leave again? On one side is the great creative wind of freedom, the immense potential of America. When I look for something to put in the

balance on the French side, what comes to mind is not science or art, which have become mere servants of the State, but the humble street scenes. That old woman I passed on rue de la Verrerie, for instance, whose hand was shaking so much she could hardly hold her grocery bag. I do not think of friends, they are so few: in one year here I have gotten to really know only a handful of new people, and only one has invited me to his apartment. I had never realized how stuffy French society could be. Our elegant neighbors in Saint-Germain only meet one another in church on Sunday morning. The ladies wear white gloves. The little girls carry clusters of marigolds.

The delightful temptation of the return to childhood is ever-present here. In the disquieting evening I look at the ancient crumbling walls, the stately cathedrals, and I feel reassured, even if I have long ago left behind the childhood faith that impressed me with its Latin hymns, its stained glass Saints\_\_\_\_Some of that respectful awareness of a higher level of being lingers within me, in a mystical tone that still echoes through *Passport to Magonia*. I needed the vibrant little room I turned into an office, opening on a garden covered with dew, with the presence of centuries around me and the light touch of elfin hands on my shoulders and the weight of scholarship around me, even if the Latin cantatas have been replaced by the Moody Blues' *Nights in White Satin*, which Janine and I are running endlessly on our little record player.

### **Saint-Germain. Sunday 29 September 1968.**

I have written to Aime Michel to tell him our decision:

I am leaving for good this time. I sincerely hope that you're happy and that you have been able to save some of that ancient culture which, for some silly reason, remains important to both of us. You guard it fiercely on your mountain like a spirit watching over a treasure. Perhaps you have found that higher lucidity I sometimes grasp briefly, and which is compatible with happiness. You are genuine and true, like flying saucers. Maybe that's why people don't believe in you any more than they believe in them.

Now I am rather glad that Rocard did nothing. It is better not to have unrealistic hopes.

He replies:



Ah *merde*, I expected this from the beginning, that you would split again! This country, which is the smartest in the world at the basic level of the average plumber, the farmer, the mailman, has managed to acquire the most pretentious, cynical and rotten elite one can imagine.

There is still a little hope, however, on the UFO side. I am coming to Paris on Saturday. The *Barbouzes*<sup>14</sup> want to see me. I am told they are fed up with the continuing accumulation of UFO cases, and they are starting to have serious doubts about the good faith of their American friends and colleagues. They are wondering if they haven't been fooled from the beginning.

### **Shell Building. Thursday 3 October 1968.**

At the *Mandragore*, where I found a two-volume set of Lenglet-Dufresnoy, the owner is an expert on Gothic fantasies with demonological overtones. However, like many occultists, he is ill-informed on UFOs, an observation that led me some years ago to abandon the Rosicrucian organizations that had attracted me as a teen-ager. His first reaction, worthy of a Rationalist, is to doubt the testimony of the witnesses, whom he calls hoaxers and drunkards. It is easier to mock them than to confess they may have seen something none of us understands.

I was still pondering this observation on the skepticism of occultists when I reached Viviens bookstore on rue Mazarine. The shop was closed. A pale young man all dressed in black velvet was staring at the books in the window. We struck up a conversation, and I said that I was interested in the history of apparitions. He told me, as in a dream:

"A long time ago, I might have been thirteen, I saw several small beings in the countryside, at the edge of a clearing, a whole band of little men, all dressed up."

Did he notice anything else? Any unusual light? Did he look for traces? No. He pushed the incident out of his mind. How tall were the gnomes? He put his hand flat in the air, less than one foot off the ground. No, there were no UFOs in the vicinity. He was not interested in flying saucers, he said, only in Magic. "Transforming the being, isolating the spirit in its tower," he insisted feverishly, with the eyes of a cornered animal.

**Shell Building. Monday 7 October 1968.**

Aime Michel spent the day with us in Saint-Germain yesterday. Once again, some significant developments have "almost" happened.

I saw the true Aime Michel again—alive, witty, eloquent, curious to examine our little treasures: Renner's paintings, the seventeenth- and eighteenth-century books I have been gathering, and the *Archives of the Invisible College* I had wanted to show him for a long time.

He told me all about his latest meeting with the *Barbouzes*. They weren't the same ones he had seen before, the colleagues of Commander Granger. Instead the people he has just met were from a different service, which I didn't even know existed. Our mutual friend Jacques Bergier<sup>15</sup> who has extensive knowledge of this shadowy world had given him pointers to establish the contact. They confirmed to Aime that they had some embarrassingly detailed sightings in their own files. They have finally come to the conclusion that what they heard from their American colleagues was pure bullshit and that Project Blue Book was simple window-dressing. They wanted to talk confidentially to someone who had researched the phenomenon.

"Could I bring one of these gentlemen discreetly to your house for a good long talk?" asked Aime.

"I suppose so," I said with some hesitation, "but only if it's clear they are not on another fishing expedition. And I won't betray any confidence."

"Of course, of course," Aime said, "I wouldn't expect you to do that. I know you well enough."

He was having dinner that same evening with Bergier, who has just left *Planète* after a violent political disagreement with Louis Pauwels, who is drifting to the right. Bergier is a ferocious anti-fascist and has the scars to prove it: during World War II he was deported and tortured by the Gestapo, who rightly suspected that he was a spy. In fact Jacques Bergier played a major role in the Marco Polo network of high-tech espionage which located Peenemunde, the Nazi rocket base.

When Aime reported our conversation, and my reactions, Bergier deflated his hopes. He had made a few more inquiries. There was nothing genuine behind such conversations with the *Barbouzes*, he said.

"Don't waste Vallee's time. These people want to see you because

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they are paid to be curious; of course they want to talk. But they are not serious."

### **Paris. Tuesday 8 October 1968.**

Yesterday, in Dorbon's shop, the owner allowed me to consult his card index because I am a good customer. It was a clever move on his part; many of his books are not on the shelves but in the back of the shop. I went away carrying Papus' *Magie Pratique*, Flammarions *Invisible World* and Murray's *Dieu des Sorcières*. He also told me of a forthcoming sale at auction of the library of Stanislas de Guaita.<sup>16</sup>

### **Same day, afternoon.**

Bill Olle has called me. This saves me from having to walk over to the post office and stand in line to send him a telegram. The devil of a man has even found a house for us in New Jersey: three bedrooms and two bathrooms.

The bad weather has gone away. Clouds race through the sky, coming from the West. Suddenly I find them fascinating. I lean out the window to watch them, those clouds flying in formation over Paris.

### **Saint-Germain. Thursday 17 October 1968.**

Aime Michel tells me "I'll never be able to leave for America." He says this with despair in his voice. He is the most "French" mind I know, yet he would go away tomorrow if he could. At work my resignation is official. A string of colleagues come through my office, and they all say the same thing: nobody can get anything done here, anything new.

### **Saint-Germain. Saturday 19 October 1968.**

Yesterday afternoon, *Chez Doucet* near the Luxembourg gardens, I had coffee with my old uncle.

"And your mother?" he says softly. "She must be sad that you're leaving again."

"She expected it, you know. And we will be back often. To see her, to see you."

"It's not the same thing."

When I grew up here I was firmly convinced that class structure was something the English had, with their Establishment boys in their prop-

er jackets and their Oxford ties and the races at Ascot. But we French had had the Revolution. Hadn't we proclaimed the Universal Rights of Man? We were all equal citizens.

Returning from the United States after five years was a major shock. It opened my eyes to the fact that there are still Lords and indeed there are servants in France. The scientists, the people who run the technology, the cadres are today's *larbins*. Stylish, liveried lackeys to be sure, well-fed and well-clothed, but they do what they are *told*, while the Lords above them have access to another world of subtle, silent attentions, a world full of privileges where there is so much money the very word need never be mentioned.

The only passion that could keep me here *is* the luxury of rare books. In the Reading Room at the Nationale I can assemble Bodin and Wierus around me and call a meeting with Paracelsus and Gorres, then I sit back and listen to their arguments back and forth as if we were sitting at home around a fireplace. I can *attend* their *passionate* debates about the reality of the soul, the paranormal, the question of evil. It is a rich and delightful human company. I see *my* books as *merely an* extension of theirs. They are the standard, the only critics I acknowledge. Our continuing research beyond the centuries is the only activity that matters.

I saw some fascinating items in Leconte's bookstore. He is the expert for De Guaita's library of rare magical books which will be sold at auction on Friday. Among the collection is a seventeenth-century manuscript about Chiromancy. On each page is the contour of two hands, where a researcher has carefully copied the features of the palm he was examining, along with the destiny, name and birthdate of its owner. Lecomte says he has never seen anything like it in forty years. Under one of the pairs of hands I read the note "*Il fut esgorgé en 1616*" (He had his throat cut in 1616). Naturally such treasures are beyond my feeble financial resources. I did buy Wier and Bodin from Dorbon recently, stretching the limits of my purse.

Saint-Germain. Sunday 20 October 1968.

It is hard to believe that in two weeks we will be back in the States. The weather is perfect, sweet, magnificent. It would be nice to just stay here and walk through quaint villages. France is a great and wonderful country, and the most pleasant place in the world if you don't have to bother earning a living.

**Paris. Friday 25 October 1968.**

At the auction house on rue Drouot I watched as several million dollars changed hands. All the books of Stanislas de Guaita were sold *in* a few hours. They represented a collection of occult works which exceeded in rarity even the library of wealthy *attorney* Maitre Maurice Garçon, according to the rare-book expert who ran the sale.

As I left the auction house in mid-afternoon I found myself in the same Metro car as a frail woman whom I had just seen win the bidding for a *hand-painted* fifteenth-century *Book of Hours*. She had paid approximately a million dollars for it. She went out at Miromesnil, very calmly, brushing against the afternoon crowd with the small volume in its red cardboard case under her arm, as casually as if she were carrying the latest romance novel.

**Paris. Rue de la Mesange. Saturday 2 November 1968.**

For the last *few days* I have been waking up with a nauseated feeling. Neither reading good books nor taking leisurely walks can dissipate this anguish. My stomach rebels, my legs are wobbly, my brain is confused: Could it be that my body is telling me it wants to stay here? On the morning when the movers came I felt weak. Yet I dutifully went to the office and I was immediately caught up in a blizzard of paperwork. Janine is in Normandy for a few days. We have no home left. It will be good to listen to the four jet engines screaming behind us when the Boeing takes off.

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**Willingboro. Saturday 16 November 1968.**

Big empty house. Janine has gone to the hairdresser. Little Catherine is slowly falling asleep. Olivier draws quietly in the kitchen, the only room where we have any chairs in this new American house we're renting. This evening we are expected at Bill Olle's home for dinner. On Monday I fly to New York as my new employers representative at a Standards

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meeting on data structures. Suddenly I am back at the cutting edge. I cannot dream of a better environment for my work, although there is something missing of course, after the intensity of Paris, in this quiet *little* development. Willingboro is a modest suburb near Cherry Hill, on the New Jersey side of Philadelphia. The house we are renting faces a small wood, the last undeveloped area in this community. A year from now it may well be gone, but we will probably have moved away by then.

Our friendly neighbors have already introduced themselves. They rake the leaves religiously.

Luxuries: on Tuesday we will have a car once again. I have not driven a car in a year. The phone is already connected, with the apologies of the company for being late by one day. In the good American tradition I have bought all kinds of insurance we don't need. What little furniture we had is on the way from France. As *soon* as we have *someone* here *to take* care of the children Janine is eager to get back to work, to feel alive and productive again.

### **Willingboro. Sunday 17 November 1968.**

Don tells me he is now working as a photographer for an architectural firm. According to him, UFO research is dead in the water. More work is actually being done on old sightings than on current ones, which are being ridiculed.

Sitting in the large room on the second floor that will become our library and office, I dream of things to come. If people mock the UFO subject, this must be a propitious time for good research. People always laugh at important things, to release tension, to regain control over what scares them.

### **Willingboro. Wednesday 20 November 1968.**

The Condon Report is two months away from publication. Tom Ratchford, of the Air Force Office of Scientific Research, is said to be reviewing the final draft. Hynek doesn't have access to a copy.

Yesterday I called Harvey Plotnick at Regnery to get his reaction to *Passport to Magonia*: he told me enthusiastically that he wanted to have it out in hard cover in the Fall of next year. I continue to be intrigued by the fact that I had *to* go back to Europe to write that poetic celebration of the

*Little People*, that acknowledgment of our *Good Neighbors*, a glimpse of an order of consciousness into which every age has projected its own fantasies, and whose existence modern man persists in denying. Could it be that events and thoughts, and even books, are a function of location? Are new ideas and new images to be fished out, like sardines, only at certain predetermined spots?

I have difficulty judging the quality of what I write, even when Janine's opinion is favorable. If she frowns, I always know exactly what to do: the manuscript *goes into* the trash without another look.

**Willingboro. Saturday 7 December 1968.**

Hynek has made another trip to Dayton, where some changes have taken place. He had taken with him his letter of resignation, ready to slap it on the desk. But he found in the new commander a very different man from what he had imagined, a man who listened to him, he said, when he pointed out the Air Force could still save face in this whole business and conduct a good scientific study. But the chief scientist doesn't want to do anything until the Condon Report comes out.

David Saunders' book (entitled *UFOs? Yes!*) is well-written, according to Hynek, who has seen an advance copy. Now that he sees what little progress Saunders' splinter group has accomplished, however, he is glad that he followed my advice and stayed away from them.

"It's just so much philosophical talk, generalities," he told me on the phone about Saunders' theory of UFOs. "Nothing like the kind of in-depth work we really need."

**Willingboro. Wednesday 29 January 1969.**

Winter has brought cold nights and ice storms. The suburbs of Philadelphia offer no beauty, only an absurd steel bridge, and a huge refinery next to the river. The dirt and stink of New Jersey are not much better. And up this road, an hour and a half away, New York City and all its monstrosity. The icy roads are dangerous. Forty cars have just collided together South of the city. Schools are closed. Janine and I are both staying home.

I am using this welcome free time to review my files, compiling the first catalogue of landing reports, which I envision as covering an entire century (1868 to 1968). I have mailed a copy of the manuscript to Don

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to get his advice. I feel alive. Man only moves forward through the changes he creates. What can one learn when one's back is to the wall? That's the way I always felt in France: my back held to the wall by the forces of convention. The letters we get tell us others feel the same way. Even my old conservative uncle writes "we must be forgiving to our country." Perhaps we have always forgiven *too* much.

The new generation growing up in Europe seems unable to correct the mistakes of the older one. If they choose revolution, it will be a long hopeless gray thing where souls will be wasted, as in the Soviet Union. If they turn to nationalism it will be rancid and ugly. If they set their economy in motion again they are in danger of turning Europe into a giant version of New Jersey, with quiet pools of poison on either side of dull expressways that link refineries, nuclear plants, chemical factories. You can already drive from the suburbs of Brussels to those of Paris or Milan and see what I mean.

I feel as one does at the end of a very long road, my love: tired in all my bones, the brain shaken by the vibration of the engines, the howling of the wind.

In my work I have rejoined my peers at last. Our software factory near Riverton, a small town fifteen minutes away from here, gathers the elite systems group in a facility no customer ever visits. They are kept focused on the task of turning out massive software tools. In the basement of the large building we operate six *Spectra 70* computers, including the very first prototype, wired by hand. The new operating systems, the compilers, the application packages are being tested day and night on these machines.

Next week I fly to Chicago, where I expect to see Ben Mittman again. Janine has a programming job with RCA too, in the group which manages the manufacturing schedule for the computer plants. I worry when she drives off on these roads. We will not spend another winter in New Jersey.

### **Willingboro. Sunday 9 February 1969.**

Hellish winter hits, wind-blown snow drifts in tough blows of white draped over *the* dark naked wood. The house shakes and vibrates. The air is taut, twisting around like the skin of a drenched balloon lost in a storm.



I spent three days in Chicago and came back drained: three days of lectures and arguments. My friends offered me a position on the Faculty as they had two years ago. I declined again, in spite of my respect for most of the people there, but mindful of what happened with the Dean when we were faced with a real challenge. Hynek does not understand why academic life doesn't appeal to me. He *cannot* imagine anything better.

"You really should be a professor somewhere, Jacques."

A poisonous kind of beauty rises from Chicago, from the mud of dilapidated Clark Street, from all the ruined buildings downtown, from the empty lots shiny with shards of broken glass, from the drunks in the doorways. At the bus stop an old insane black woman planted in the snow laughs alone like a wicked witch. From the top of the Palmolive building the beam of a searchlight looks like a crazy Menzelian experiment, drawing ghostly disks in the fog. Beyond it the carcass of the Hancock building carries a few lighted offices deep into the cloud itself.

"My answer to society, here it is!" an old cab driver tells me, sweeping theatrically the inside of his car. Following his gesture I see nothing but a grayish photograph of his face on the dashboard, a serial number in big black figures, the meter with its red digits, a pool of mud under my feet where the shoes of his previous clients have dragged in the Chicago winter. Not much of an answer.

And *my own answer to society*, what will it be? It should begin with the expression of human emotions. Last year the turmoil of Paris was softened by sensuality even at the height of the crisis. But America doesn't encourage emotions. Even in love this greedy Freudian continent does seem uniformly cold and mechanical, I thought as I looked at Chicago, searching old memories. I do treasure a few exceptions, but they came by pure chance, when the cold of the night brought lives together by the light of a fireplace for a brief moment of tenderness.

Is paranormal research just another attempt at a "response to society," like the cab driver's little world, or is it a genuine way to transcend ordinary reality?

Last Saturday we had a reunion of the old *Invisible College*. Hynek was there, Fred and Don too. Bill Powers came in, jovial, with a new beard and plans for a huge research proposal that would cost two million dollars.

"It would go over like a lead balloon," grumbled Allen.

A man named Donald Flickinger, who works as an agent for the Treasury Department, has sent a curious confidential report to Hynek: in 1961 it seems that four hunters saw an object near Minot, Minnesota. There were several figures near the landed craft. One of the hunters fired, and at that point the report becomes very confused and dream-like. One of the pilots of the object is said to have fallen.

"Why did you do that?" another supposed ufonaut screamed *in English*.

The witnesses came home disoriented, having lost four hours somewhere. They swore to one another they would say nothing to their families. The *next day*, however, official-looking *men came and* picked up the fellow who had fired the gun. How did they find out who he was? The mystery continues to deepen, with no solution in sight.

### **Willingboro. Sunday 16 February 1969.**

The end of the week turned out to be full of beauty, with the sun in the clear sky and the cold, crisp air in our lungs. At the computer plant the whole software team met to prepare the testing and the release of our data-base system, called User Language-1. I love the energy of this group.

A young French girl from Normandy has joined our household. Maud is nineteen, a redhead, curiously jaded about everything she sees in America. But she cares for the children, they love her and this restores some measure of freedom to us.

On Friday Janine and I spent the evening with a journalist from Philadelphia who knows the witchcraft traditions of Bucks County. In Upper Black Eddy, he told us, a woman named Mary Manners Hammerstein received witch Sybil Leek in 1964. She is said to lead a coven associated with *spiritualist* circles in Philadelphia and with a man who is the inventor of a saucer detector: thus the Delaware Valley is rich in devilry to this day, even if Dow Chemical and RCA Electronics own increasingly large sections of the land. Nearby, in Everittstown, an elf-like ufonaut was seen in November 1957: he came out of a saucer and tried to steal a dog! Was there a symbolic message in this display? The Russians had just orbited a satellite with a dog named Laika on board.

Yesterday we went on an excursion to Princeton with Maud and the children. Catherine is almost standing up by herself now. And Olivier

has started to ask sharp questions for which we have no answers.

"What will there be, *after the days*?" I wish I knew.

Sunday night: classical music. I read *La Mystique* by Gorres, a five-volume treatise on everything in the higher universe, from angels to demons. There is adventure in the passing of time, an inspiration in the air, the spark of love in your eyes.

**Washington. Tuesday 18 February 1969.**

It is because of a retired French officer named General Lochard that I am here. He represents AFNOR, the august French standard "Association for Normalization," at an international meeting about the vocabulary of computing for which he is Chairman. Sending me there as part of the American delegation was a diabolical plot on the part of Pete Ingerman, the man who is in charge of standards for RCA.

A few weeks ago Pete came into my cubicle, playing with his black beard and eyeing me with a conspiratorial air.

"How would you like a job as a double agent?" he asked.

"What's the assignment?"

"This company has a problem with standards. Not just this company, but the whole computer industry. See, we have to have an international standard for data processing vocabulary that can be used in every application."

"That shouldn't be too hard," I said. "There are only three or four companies that run the whole business anyway. If they really want a U.S. standard they can do it in a few months."

"You're right, but the point is, they don't want a U.S. standard. Instead they want an international standard that can be applied in this country but can also be used in selling machines all over the world. That means getting the ISO, the International Standards Organization, to standardize the terms used in computer technology throughout the planet."

"Is there a problem with the ISO?"

"It has two official languages, French and English \_\_\_\_"

I began to see what was coming.

"The French want to protect their own language, which is fine with everybody, but they keep delaying the proceedings until they have a perfect French equivalent for every term. The last international vocabulary the ISO standardized was that of Telephony. Do you know how long it took?"

I had no idea.

"Fourteen years. Now you see the problem? In fourteen years all the terms we use in data processing will have become obsolete. So my idea is this: you work with us drafting the American definitions, and you think through the French equivalents. When we get together for the international sessions you sit with us behind the star-spangled banner. And when the Chairman, who is an old French general, tells us he cannot accept our definitions because they have no French equivalent, you get up and you read them to him in his own language!"

That sounded like a lot of fun, but more importantly it was an exceptional chance to meet some of the pioneers of the software field, so I agreed enthusiastically.

### **Willingboro. Friday 21 February 1969.**

Maud, the young French girl who has recently joined us to take care of the children, is a stunning redhead who is quite ready to have fun. Yet when she goes out with the local boys she comes back scared and utterly disgusted with their games. She tells us stories of drag racing in their parents' cars along the deadly expressway, of mindless groping on the back seat. She finds all that stupid and debasing, yet that constitutes young love in the United States, the rite of passage.

America the Great Teaser. There is plenty of apparent, obvious sensuality at every street corner, of course: the advertising media thrive on suggestive images, naked women and subtle seduction. But these empty promises are just the tantalizing side of a great equation of hypocrisy: there is no depth, no sophistication of feeling, no complexity of the heart around us.

### **Later the same day.**

At the National Bureau of Standards the building seems too big, the rooms a succession of empty caverns in the gigantic Federal style which is uncomfortably reminiscent of Stalin's architecture and of Hider's Berlin. From the gigantic windows one can see the plains of Maryland, white and gray, monotonous.

I am not comfortable on the East Coast. From Boston to Washington I can hear nothing calling me, no detail with which I can identify.

**Willingboro. Sunday 2 March 1969.**

I called Hynek today. The new commanding officer at the Foreign Technology Division, Colonel Winebrenner, has taken him to lunch in great style, but Allen still gets into regular arguments with Quintanilla. There is no reaction yet to the Condon Report, which is pretty much what everyone expected: a despicable snow job, padded with an irrelevant section about radar. Jim McDonald is expected to write a formal critique on behalf of NICAP. Always the same old maneuvers: These people have understood nothing, nothing at all.

**Willingboro. Wednesday 5 March 1969.**

I watch the beauty of the sky when the sun sets in the fields, beyond the thundering expressways I leave behind as I come home from work. Bill Olle has put me in charge of a five-man testing group. I only leave the factory to walk around after lunch with a Swedish friend who is discovering the Delaware Valley with as much astonishment as I do.

When I come home I take little Catherine in my arms. She laughs. She grabs my glasses and plays with them. Together we watch the sunset behind our wild little wood. Soon the greedy developer will come back with his bulldozers and raze everything in sight. Why should he refrain from building more houses, putting in more streets, leveraging his investment in the land, selling, selling and selling? This is a planned community, the kind of town of which contemporary Americans dream. Naturally, it cannot tolerate one little undeveloped wood. Didn't anybody tell the planners that people will eventually go crazy if they are not allowed at least one tiny corner of wilderness?

My own life is serene. We will go away as soon as the first trees are felled. I have written to Renier, the Belgian artist friend of Serge Hu<sup>u</sup>in, to buy some of his latest paintings. I am carried forward by heady anticipation, the same feeling I had in Evanston when I worked closely with Allen. I believe Man has an infinite ability to create.

Our company keeps recruiting good programmers from Europe. I talk to them as they get off the plane. They all say the same thing: it is not possible to be professionally alive and creative over there, in a tiny stifling world crumbling under its moldy crust.

**Willingboro. Sunday 9 March 1969.**

The things I love: a breeze in the foliage, Janine's smile, my children, the sunset, the quaint intricacies of language (old French, Elizabethan English), the efforts of men, especially those who failed, who feared, those who searched relentlessly through the mud.

Who are they, these people who foolishly thought they had found the key to dreams and destiny? The visionaries: Paracelsus, Nostradamus, Joan. The geniuses: Galileo, Copernicus, Bruno. The dreamers: Cyrano, Swift, Renier, Hynek, Villon, Aime Michel. The lovers: Casanova, Baudelaire, Rimbaud. Not a single empire-builder among them; not a single servant to the politics of his time. And none of them was ever helped by those in power with either honors or money. So let us not talk about the State protecting the Arts and the Sciences. Beauty, the creative force, runs deep and invisible under the crust of history, under the pedestals of power and wealth.

Ail those places I love, too: tiny, quiet, modest rue de la Mesange, where my mother lives; Quai de l'Horloge along the Seine; and the streets where no one ever goes any more, in the villages of despair a long way from Paris: rue des Etanets in Pontoise; the carriage doors with their large nailheads where no horses come through. Rivers of memory flow towards oblivion.

We took a Sunday drive into the country today. The road followed the Delaware River, leading to the northern hills. Beyond Frenchtown we found a rugged land, impressive, alien and dark. A steep incline took us to the crest of Everittstown. A few farms showed up in the desolate landscape covered with snow. The Alleghenies rose on the Pennsylvania side. Ten more miles and the atmosphere, the people changed again. Here the little man dressed in green tried to steal John Trasco's dog. Yet we are only one hour away from New York City. Why should our Visitors love such forlorn, isolated spots? When dusk arrives and the crust of freezing snow crackles under the car tires I begin to experience the same fears described by John Keel in his books, I understand the primeval terror he writes about.

**RCA Factory. Riverton. Friday 21 March 1969.**

Curious *Spring*. The developers have started *to* ravage our little wood sooner than I thought. Nonetheless the excitement I feel here doesn't go away. Janine is visiting friends in Florida. How should I spend my time? Should I leave the children with Maud and go explore the bookstores of Philadelphia? Try to make new friends? It hardly seems worthwhile. The remaining months here will be short.

**Willingboro. Thursday 3 April 1969.**

Five pages of *corrections* and additional references for *Passport to Magonia*: I mailed the whole thing to Regnery today. The full text of the landings catalogue, all 923 cases, is ready. The phone lines are active among Don, Allen, Fred and me. Alleged pictures of little men with long noses have been published by the media. They look like obvious fakes. Keel's stories, which have the urgency of terror, are taken too seriously by many believers. Don believes that Jerome Clark, a young ufologist from Chicago, has become so convinced that an extraterrestrial invasion was imminent that he has been driven close to a breakdown.

Once again Fred is angry against Allen, who has just returned from an investigative trip to California with a suitcase full of magnetic tapes so poorly recorded they are inaudible. At one Air Force base where he planned to interview some of the pilots he was sent away with the terse comment:

"Project Blue Book? That doesn't exist any more!"

**Willingboro. Monday 7 April 1969.**

At my suggestion Fred invited Don to the University of Chicago to show him what scientific research was really about: Don is quite gifted but he has grown up in the streets with the teen-age gangs of Chicago and he has only seen the cold, official, boring face of science: the books, the public television shows that never tell the actual truth. Fred says he doubts if Don was very impressed or moved by what he saw in his lab. They did talk about magic, secret societies and the films of Crowley disciple Kenneth Anger.<sup>17</sup> As they came out both of their cars had parking tickets on the windshield. They were signed by an officer named A. Crowley!

**Willingboro. Sunday 4 May 1969.**

Michel and Françoise Gauquelin visited us yesterday. They brought greetings from Allen, with whom they had spent two days in Evanston. We took them to see Princeton. They were surprised to see the campus, with students sprawled on the grass typing their term papers on portable machines, and girls in flowery shorts sitting under the trees, annotating books on quantum mechanics. We were a long way from the gray Sorbonne indeed. The big people in Princeton are the highly paid consultants who regularly take the train to Manhattan or Washington to sell their ideas to Blue Chip companies and Government agencies. Yet even the RCA Labs, which are famous for their numerous past discoveries in radar, radio and color television, suffer from a high level of compartmentalization.

Michel and Françoise were in Los Angeles when the news came that De Gaulle was leaving power. They were as surprised as we were by the political commentaries, which show an amazing level of fascination for *Le General* even among people who hated him profoundly. As if De Gaulle had been anything other than the concentrated reflection of French collective shortcomings. The Providential power that seemed to propel him was little more than a clever magic trick, an illusion utilizing the people's own carelessness, their relinquishing of responsibility for themselves. Some commentators are foolish enough to doubt that the scepter will be passed on to Pompidou.

Olivier, who detests visitors, astonished the Gauquelins by precipitating a family crisis in the course of which he coldly smashed a glass against the wall. This daring terrorist act demanded a certain strength of character, since punishment was inevitable. He ran away into his room. When I followed him there, however, everything was in place and no kid was visible. The window was open: I looked out and decided he could not have run away fast enough *to disappear from the landscape*. I *concentrated* on the room itself and eventually found the culprit hiding under his bed.

**Willingboro. Friday 9 May 1969.**

Computer technology is being swept forward, all around us, by an irresistible current. The latest concept is that of simultaneous, interactive use of the machines by many users in a mode called "time-sharing." It will



make possible a whole range of new applications because it will no longer be necessary to have computers used by a single person at a time. One will not even need to go over to the computing center itself to access the machine. RCA is advancing in that direction, although with great difficulty. *So is IBM*, whose latest machine is capable of being utilized in that mode, but good software is still lacking. The computer field is exploding: opportunities abound for those who can seize them. Programmers are in short supply everywhere.

Aware that I could accomplish far more than what I am doing now, I have sent a letter-proposal to Rowena Swanson at the Air Force. I would develop an automated documentation service that could serve as a test-bed for artificial intelligence research. Perhaps I could run such "experiments" at Princeton. I am scheduled to give a lecture there on Wednesday at the invitation of Saul Amarel, a well-known software pioneer.

Our Riverton group works hard but the RCA computer effort in general is not going well. The grand strategy which called for beating IBM in communications and sophisticated human interaction is not being executed. Our marketing department is convinced that we must remain able at any time to respond to new product introductions by "Big Blue." But the implication is that, like the French, we always follow rather than lead. How can we ever intercept IBM's technical path in the distant future if we are doomed to react to every move they made last week? Those are the things the systems group *discusses* emotionally at the cafeteria.

A case in point is a very advanced implementation language called IL-1 that some of our brightest developers, including my friend Max Smith, have defined. The systems group wants to release it. Initially it was envisioned that all our software would be written in that language, which would provide a tremendous productivity tool to specialized teams like ours. Instead, marketing has now decreed that IL-1 must not differ significantly from a new IBM language called PL-1, which is supposed to be standardized "any day" for general use in the IBM user base, replacing Fortran, Cobol and even Assembler in one big blow. But PL-1 does not contain the advanced programming features we need, so we have been forced to revert to writing all our software in expensive, cumbersome assembly code.<sup>18</sup>

It is the **private** opinion of our little group that IBM has *no* intention at all of ever standardizing PL-1 in spite of all this hype; that our own

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marketing geniuses are wasting their time following that carrot dangled in front of them. Good intelligence from systems programmers within IBM itself tells us that their own shop *is* doing all their development in assembler, just like us, contrary to claims that PL-1 is the universal language of the future. That means we cannot use our best secret weapons. And without secret weapons RCA will probably be forced out of the field.

*Science is no longer* the single major factor of human progress envisioned by Camille Flammarion. Yet digital computers are truly magical tools, deeply changing the world, destroying entire sections of the old mental framework in the process. The power of a few groups of systems programmers, hidden away in the back woods, exceeds everything people had imagined. The growing sophistication of computers combined with media conditioning on a large scale can only mean one thing: the secretive technocrats with high security clearances who shape the major decisions in the advanced nations *will* soon have the ultimate power in the world. Jacques Bergier calls them "cryptocrats."

At the Warwick Hotel in Philadelphia this afternoon I saw the members of the annual conference on automated documentation slowly falling asleep. I did have a chance to talk to Ben Mittman, who gave a talk about Infol. Through the windows of the twentieth floor I could see Locust Street. It was a glimpse of a foreign planet. Many things are wrong in this well-ordered world which keeps the blacks among blacks, the whites among whites, the poor among poor, and cops at all the crossroads. If no deeper current comes to provide new vision we will end up with a society made up of multiple Willingboros surrounded by an infinity of Harlems, and a few Princetons majestically riding on top of the hill. People will be locked and entertained within their own little spheres. They will be allowed to evolve slowly with the ebb and flow of directed fashion. The ruling class will be a refined group of old anonymous minds. *Yet who could say* that this depressing *future* would be any worse than our past, the odious history of mankind?

### **Willingboro. Saturday 17 May 1969.**

What I will remember of this town: the sound of lawnmowers, the smell of weekend barbecues in the backyards, the retired military men and the retired insurance agents who are our neighbors, decent folks with

their red checkered shirts and baseball caps, and the boredom of the curved streets. Psychologists have discovered that straight streets made people unhappy, so everything here curves like the halls of the *Queen Mary*, meeting other curving streets, all the way to the main highway. This is not where I want to spend the rest of my days.

On Wednesday I went to Princeton. I was kindly received and the seminar I gave on information retrieval met with polite applause. Then a man with intense eyes and silver hair took me aside. We sat on the benches in the lab next to the lecture hall.

He said, "There is a fundamental fallacy in artificial intelligence, and you're falling into it like everybody else."

"In what respect?" I asked with the feeling that this discussion was not going to conform to the usual exchange of generalities heard at most professional meetings.

"Artificial intelligence is trying to emulate nature, it wants to approximate what man does."

"What other inspiration is there?"

"Imitation of nature is bad engineering," he answered patiently. "For centuries inventors tried *to* fly by emulating birds, and they killed themselves uselessly. If you want to make something that flies, flapping your wings is not the way to do it. You bolt a 400-horsepower engine to a barn door, that's how you fly. You can look at birds forever *and never* discover this secret. You see, Mother Nature has never developed the Boeing 707. Why not? Because Nature didn't need anything that would fly that fast and that high. How would such an animal *feed* itself?"

"What does that have to do with artificial intelligence?"

"Simply that it tries to approximate man. If you take man's brain as a model and *test of* intelligence, you're making the same mistake as the old inventors flapping their wings. You don't realize that Mother Nature has never needed an intelligent animal and accordingly, she has never bothered *to* develop one!"

I could only greet this stunning thought with silence. He went on:

"When an intelligent entity is finally built, it will have evolved on principles very different from those of man's mind, and its level of intelligence will certainly not be measured by the fact that it can beat a chess champion or appear to carry out a conversation in English."

With his piercing eyes on me, I had a brief vision of what an *intelli-*

gent machine might be. If Nature has never needed an intelligent animal and hasn't evolved one, I kept wondering, then what are we? In our feeble attempts to handle the information we call our life, can we trust the creations of our dreams? Are we perhaps nothing more than the process through which another form of intelligence is evolving?

We spoke of software operators, their role in language understanding and in pattern analysis; he gave me an example of how a recognition operator would do its work by successive matching approximations: "it's a bird, it's a plane, it's Superman!" This made me smile because Olivier is always insisting I should *come* back *horn* work early in the afternoon to watch Superman with him on television. He tells me enthusiastically:

"*Superman*, if you kill him, you can't kill him!" Another nice example of logical operators in conflict.

Chances are slim of my doing serious work at the Princeton Research Labs of RCA. Their computer is already saturated and their overall management is shaky. So I sent my resume to Stanford University, where a position is open for a senior information scientist. It is time for us to *move*. We have agreed that we should find a place where we could buy our own home, a house with an opening on the sky, a house that would live, breathe, commune with the night.

Washington. Monday 19 May 1969.

Another international standards meeting. Dinner and an evening of *conversation* with Saul Gorn, a professor from the Moore School of the University of Pennsylvania where ENIAC, the first fully operational computer, was conceived and built. Saul Gorn is one of the founders of the field of software theory. He was *one* of the first people, among Von Neumann's colleagues, who understood that the instructions given to the new machines were not just a series of electronic quantities represented by zeroes and *ones* but the beginning of an actual language, an "artificial" language, as opposed to English or French which have evolved "naturally" out of human usage.

We continued our discussion as we left the restaurant and walked through Dupont Circle. Blacks were playing drums, hippies sat on the lawn smoking pot, motorcycle gangs rode around making as much *unmuffled* noise as possible, Puerto Ricans strutted back and forth, each

group closely conforming to the accepted rituals of their own tribe while pretending to rebel against society at large.

"No matter what you do, we can't prevent our kids from having the same values as the other kids when they go to school," said Saul Gorn.

"That doesn't bother me," I replied truthfully. "I know that my children will have new data, new knowledge and will be exposed to different values. I just want to make sure they have their own set of standards by which to judge all that."

### **Washington. Thursday 22 May 1969.**

After four days of discussions and decisions presided over by the wild and multi-faceted mind of Pete Ingerman, I have come back to the hotel to rest and read. The evening will be spent with Rowena Swanson, one of the best-informed people in the small world of artificial intelligence. This is a world which functions as a secret society, selecting its members through unspoken rules.

Two interesting episodes took place during our deliberations in Washington. The first one came after we had just defined the term "language" as

A set of signs, represented as symbols, used to convey information.

A problem arose when we tried to separate artificial languages, which computers are using, from natural languages. We agreed that natural languages arose from human usage, while artificial ones were based on machines, but we could not define it in such an "intuitive" way. If the mere fact of prescribing a language made it artificial, then Esperanto would be an artificial language, which confused the issue even more.

I posed the following problem to the group: two computers, each of a different make and each equipped with a communication device, are placed in a room where there is no human. They are programmed to establish contact with their environment. After some preliminaries, we would expect the computers to exchange some signals, acknowledge the contact and establish a joint protocol to send and receive messages. Eventually they would define a communication structure they both understood.

"Would that language be natural or artificial?" I asked.

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The linguist expert in the group thought for a long time, got up, paced up and down, tore up his hair and finally said, "natural."

Later, General Lochard announced triumphantly that the French delegation had finally discovered an appropriate French term to translate "software." We all perked up, because the problem had been a vexing one for years. A prize has even been proposed in Paris for the first person who would solve it. With our group was a very distinguished middle-aged State Department interpreter of French who was on loan to us for the duration of the proceedings. He was dressed in a dark suit and tie and sat humbly in the back, behind the chairs of the delegates, waiting for his services to be required.

"Well, what is that fantastic new term you have found to translate *Software?*" asked Pete Ingerman.

"It is the word *Programmerie*," General Lochard said proudly.

I was too stunned to react. But the State Department interpreter jumped up from his chair like a devil out his box and sang in a clear and enthusiastic voice:

Quand un programmeur rit  
Dans la *Programmerie*,  
Tous les programmeurs rient  
Dans la *Programmerie!*

This was a witty play on an old French children's song mocking the Gendarmes:

When one Gendarme laughs  
In the *Gendarmerie*  
Then all the Gendarmes laugh  
In the *Gendarmerie!*

He had simply replaced the word "Gendarme" with the word "programmer" and the word "Gendarmerie," a barracks for Gendarmes, with the proposed new term *Programmerie*, which thus became a barracks for programmers.

After this unseemly explosion he must have realized what he had done, because he turned as red as a ripe tomato and sat down in confusion among our laughter. Not only was it witty, but for an American it was an amazing demonstration of intimate mastery of French colloquialism,

a lesson General Lochard was not likely to forget. In any case *programmerie* was never mentioned again, and the term *software*, itself an astute play on words in English, remains untranslatable into French.<sup>19</sup>

**Willingboro. Sunday 25 May 1969.**

Yesterday at 3 a.m. I saw a wonderful thing in the glow of my headlights as I arrived home from Washington: all the weeds are growing wildly on the lawn we have not mowed for ages. Our neighbors have been too polite to complain, but it is clear I am expected to do something about this jungle very soon.

Rowena Swanson, with whom I had dinner in Washington, saw little reason to get excited about the current state of artificial intelligence research.

"People are just endlessly re-doing what has been done before," she said as I filled her glass from the Chianti bottle.

"What about Stanford? Everybody is talking about their robot," I said. "Saul Gorn, who is very cynical, thinks its only purpose is to train a few students."

"I'm even more cynical than Saul," she said with a smirk. "The robot's only purpose is to spend a few million dollars of Pentagon money. There is a terrible lack of imagination everywhere."

She warned me about the West Coast:

"It's a tough world out there, nothing is ever stable in California. It's ruthless. Whenever they temporarily run out of money they just lay off all their engineers. They will have a big crisis if Washington ever gets tired of paying for all their high-tech toys."

We walked over to Georgetown, where a young idle rich crowd seemed to be waiting for something no one could define.

Now I am back home with my little family and we listen to the *Moody Blues*:

The trees are drawing me near  
I've got to find out why....

We have celebrated Catherine's first birthday. Olivier was a real pest: he wanted no part of the cake, and locked himself in his room. The challenge for us is to keep re-affirming the structure around him without breaking his independent spirit: he will surely need it later. But this time

it was not so easy to restore the structure. I had to get a screwdriver to take apart the doorknob before firmly putting him to bed.

**Willingboro. Tuesday 27 May 1969.**

A simple business trip has turned into an exhilarating visit to New York. There are flags over Rockefeller Center, strains of music beyond the open-air restaurants. Yet the financial markets are falling. On the marble steps outside the major banks, groups of men my age wearing three-piece suits argue in concern.

I have been invited to New York by the same kindly recruiter for Shell who had spoken to me two years ago. To my surprise he wanted to know if I would consider returning to Europe to work for them again. He had the same subtle Dutch elegance I found among the top executives in The Hague. He took me to lunch at his Club, complete with expensive carpets and leather armchairs, old books, white-haired butler. A middle-aged waitress with rosy cheeks served us food like a mother. The thrust of the discussion was a generous new offer. I said I was honored and pleased they had made this effort to contact me.

"I really have no interest in returning to Europe," I told him. "If I were to work for you again it would be in Emeryville, in the research company," I added, thinking of my friend Paul Rech who belongs to their operations research team in California. He promised to look into it. I was astonished to find how much work he had quietly done on my background. He has even read *Challenge to Science*.

In Manhattan I was struck by the sudden sensual itch in evidence everywhere. Pornography has become a major retail business. Miniskirts are in fashion. A pretty girl sits on a bench in the sun, displaying the flesh of her upper thighs. She unfolds a red cardboard that becomes a tanning mirror. A blind man passes in front of her with his dog, missing the whole spicy show.

**Willingboro. Monday 2 June 1969.**

Don Hanlon has just spent three days at our house, sleeping in my large office on the second floor. Our talks began among the old books, continued in the RCA computer room and concluded on Saturday as we walked under the night sky. We spoke of Hynek and Project Blue Book, of John Keel. But our major topic was esoteric theory and especially



the Rosicrucians. I recently published a humorous piece in the form of a ufological horoscope. It included a short paragraph destined to Allen:

You will learn with amazement that the U.S. Air Force has closed down Project Blue Book. You will be hired as a scientific consultant on a new project called Sitting Duck. Grow a beard.

Don, who was there when Hynek opened the letter, tells me it made him burst into laughter. Indeed, he now expects Blue Book to be closed for good. He believes the higher levels of the Air Force are privately divided over the UFO issue. His relationship with Quintanilla has reportedly deteriorated to the point where they can't stand to be in each other's presence. Allen, who is usually restrained and gentlemanly, refers to the Major in the most unflattering terms.

We debated the various schools of magic, Don being partial to Aleister Crowley's *Ordo Templi Orientis* to which avant-garde director Kenneth Anger also belongs. Any group that demands total belief, as the OTO does, and provides absolute symbols, seems meaningless to me. Magic should be the meta-science, not just a set of cooking recipes. I am not looking for a belief, I am not eager to witness cheap marvels and parlor tricks. What impressed me about the Rosicrucians was their belief that no human organization could be anything but a fragile support in this kind of work, a temporary resource, and that the true realm to which they felt allegiance was "of another level," beyond human life and physical reality itself.

According to Don, the most accomplished scholar of modern magick in America is a man named Israel Regardie who lives in Los Angeles. He once served as Crowley's secretary. Recently vandals have ransacked the old doctor's library, stealing Crowley's manuscripts. Some people suspect that California motorcycle gang members, like those depicted by Anger in his movie *Scorpio Rising*, may be involved in the despicable burglary.

Don also showed us Crowley's admirable Tarot cards, powerful images of genuine beauty.

It is out of the question for me to belong to a faith, a church or a sect, no matter how much intelligence its leaders demonstrate. I am looking for my own slow deliberate progression towards a personal spiritual truth. This means gradually transforming my own life, placing it into the right conditions. I agree with Allen when he says that a man who is

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capable of an active spiritual life can survive anywhere.

Now a terrible wind is blowing, a Summer storm. The trees howl and shake. The rain is about to begin.

I wonder what I will think when I reach the end of my life. I don't think I will miss the earth, mankind, this body.

### **Willingboro. Wednesday 4 June 1969.**

I have just spoken to Fred Beckman in Chicago. He is very upset at Allen who, he says, is kidding himself:

"He seriously believes the Air Force is going to give him five people and a real budget to study the electromagnetic effects of UFOs. He is wasting his time. He just won't face the fact that the same Air Force has just spent half a million bucks to convince the world they didn't exist!"

### **Willingboro. Tuesday 24 June 1969.**

Next Monday I fly to San Francisco for a job interview. I have had several phone conversations with Paul Armer who heads up the Stanford computation center, reputed to be the best in the world, the frontier in the software field.

Fred's voice on the phone registers a careful reserve which is very clear when I ask him to read the galleys of *Passport to Magonia*. I know that he does not agree with me that flying saucers are "folklore in the making," but he will read the text out of friendship. Hynek has left for Corralitos observatory, a small astronomical outpost with two domes operated by Northwestern in the New Mexico desert.

The other day, reading an old Scottish book on folklore, I came across a beautiful proverb: "There comes with Time what comes not with Weather." Curiously it cannot be literally translated into French, where time and weather are the same word, temps. I should mention this to Saul Gorn, who keeps a collection of linguistic anomalies. Indeed, as I get ready for my first trip to the West Coast, I wonder what will come with Time.

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## **Stanford, California. Tuesday 1 July 1969. 00:20**

The little red alarm clock is ticking away, still showing New Jersey time. The white Galaxy I rented is parked out in front of the Tiki Inn motel. I got lost twice before finding the little road through the hills that brought me back after my first visit of the Campus.

I had never flown over Utah and Nevada. I spent my time alternating between the views of the Great Salt Lake and the reading of Baudelaire, with whom I have become re-acquainted at thirty thousand feet. I have seen little of Palo Alto: uninspiring lines of motels and bars, a fancy hotel with plaster statues, the usual kitsch. But the air is soft and tender. I have time to look at the land, to consider where we might live if I got a firm job offer, if we moved here.

## **Stanford. Thursday 3 July 1969.**

House-hunting in Half Moon Bay: the real estate agent whose office I had spotted in this little coastal town has died. I went to the closest coffee shop to think. I discovered with astonishment that the food was very good: real bread that tastes like bread, and butter! Oil and vinegar for the salad. Fresh vegetables.

One cannot understand the coastline just by looking at the map. One has to see the farms spread between the hills and the Pacific, and drive on the rough dirt road to the top of the cliffs that overlook the surf.

Very few people live along the coast. This wonderful landscape is empty; the big crowds are on the Bay side of the hills, where all the motels and the bars lie clustered together. Here there is nothing but weeds, blue wild flowers. Seagulls survey the rolling waves.

San Gregorio, the next little town down the coast, has a few wooden houses lost in the fog and the spray.

La Honda is a beautiful and heretical village in the tall drama of a pine forest high in the hills. What are those palm trees and those euca-

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lyptus trees doing among the redwoods, with an occasional willow to increase the gorgeous confusion of the foliage? The weather was warm, the smells intoxicating. I stopped everywhere to take pictures for Janine. I went to another real estate office in Woodside. They showed me photographs of available houses. I promised to write.

I didn't have the feeling I was seeing the edge of the Western world when I suddenly reached the top of the cliff and faced the Pacific for the first time. Instead I felt a projection into the future, not twenty or thirty years, but a thousand, two thousand years. Everything suddenly seemed possible, everything was allowed. The feeling of total freedom that the rioting students of May '68 had sought in vain was there, in plain view. Everything else, even the details of everyday life, seemed irrelevant. A warm wind blew over, a touch of infinity. The same impression was present last night when I contemplated San Francisco from the top of Telegraph Hill. Only a sin, a mistake, temporary insanity on the part of the gods can explain such magical beauty, vibrant and clear, diaphanous.

### **Willingboro. Monday 7 July 1969.**

The haunting beauty of the California coast stays in my mind now that I am back in New Jersey. It is unique because the landscape there is shaped by constant seismic force. Erosion has no grip on those hillsides.

The people I met at Stanford, however, were not in keeping with the awesome grandeur of their surroundings. Paul Armer turns out to be a Teddy Bear rather than a real manager. A pleasant man in social surroundings, he became somber and withdrawn when we spoke in his office. What problems are hiding there? I was interviewed by Ed Parker, a professor in the Department of Communications, which runs an information retrieval project into which the Federal Government has poured untold millions. There have been no results so far, only wordy research reports.

Another interview the next day, with Bill Miller this time, the vice-provost for computing. He pulled the strings, that much was clear. The description he gave me of his empire was chilling.

When I sat down at one of their terminals and ran a few tests I could see that the database software was a dismal failure, but one thing became obvious: the operating system itself was a real joy, the culmination of every specialist's technical dream. I would give anything to meet the peo-

ple who built it. They are years ahead of everybody, in a field where six months constitutes an eternity.

**Willingboro. Saturday 12 July 1969.**

We are coming back from Princeton. One last attempt to explore alternatives, to compare. It was raining in Princeton. We ate some pancakes with Maud and the children. We visited a house for sale. The windows were nothing but narrow horizontal slits.

Now the rain has intensified, my shirt sticks to my back in the muggy weather, Catherine cries. The world has less meaning than ever.

The other day I was reading Baudelaire's *Le Voyage*, from *Flowers of Evil*, as the plane flew over Nevada:

Yet the real travellers are only those who leave  
For the sake of leaving; light hearted, like balloons,  
They always follow their fate.  
Without knowing why, they always say: go on!

Those whose desires take the shape of clouds  
And who dream, as recruits dreaming of the cannon,  
Of vast, changing, unknown raptures,  
Whose name the human spirit has never known!

I have shown Janine the pictures I brought back from Palo Alto.

**Willingboro. Thursday 17 July 1969.**

Our bedroom is dark, hot and humid in spite of the air conditioner, which makes an annoying sound. Janine is working on a dress. I still hesitate to move to California, fearful of getting caught in the parochial fights that are going on at Stanford. They remind me of the academic feuds I had known at Northwestern, only deeper and more vicious, because more serious money is involved. It is an atmosphere in which creativity may be a liability more than an asset.

A young mathematician who works for me on information structure problems told me he was not surprised at my fears of returning to a university. Academic life is frozen and dull in spite of all the pretentious airs that surround it, he said. If anyone raises a serious problem he is often sent down a blind alley. It is a micro-culture that cannot be threat-

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ened or forced into change. He points out that even the beautiful slogan of the Sorbonne students in May last year, which claimed so proudly that *It is forbidden to forbid!* was only a logical self-negation, an empty statement of Utopia, with no real operational value.

We must move to California. We will have a door leading to the woods, a window over the sea, a trapdoor to the sky, "and thy dress to the evening wind."

Yesterday three well-trained pilots left for the moon. We watched the launch of *Apollo* on an employee's tiny black and white television set installed in the RCA employee cafeteria, a place which smells of dish-water. The rocket went straight up, looking like a child's pencil. But most of the American public, bored with the space race, was watching other channels.

I miss being away from my mother at times like these. I imagine her in Paris, listening to every news bulletin, buying every newspaper. It is one of the great dreams of her life which is taking shape now. I admire her enthusiasm. But I already see the world that will come after we plant our flag in the dust of the moon: I fear it will be a strange dreamless world. I worry that the entire sky may soon become filled in every direction with spy satellites, flying bombs, orbital barracks and the cosmic latrines of the new secret armies.

Will we ever come back to the true questions: who are we? What is that strange process we call thinking? What are the dimensions of our world? Perhaps we should make a machine to think about these questions for us, since we do not dare ask them any more. But there can be no such thing, of course: everybody knows that building a thinking machine is just an unrealistic dream, like going to the moon\_\_\_\_

### **Willingboro. Sunday 27 July 1969.**

Around us, Americans of all ages are increasingly using drugs for recreation because, as they stupidly say, "it makes them happy."

When I was growing up my parents' generation viewed a trip to the moon as a purely theoretical idea. It could only be the culminating achievement of a supremely enlightened scientific culture, something that would not be possible until men became immensely wise. The very fact of going to the lunar surface would be a signal of cultural eminence, an irreversible break with the ancient world of wars and human misery.

Human life would be forever changed into a golden age.

The other night we watched *Apollo XI* land smack in the middle of *Mare Tranquilitatis*. We saw Armstrong, then Aldrin get out and plant the flag in the moon soil. Yet nothing has changed. Our world is as divided, fearful and greedy as ever. People still need to escape into drugs. Why did I have the feeling that an enormous opportunity had just been wasted?

We must leave for California. I need a land of ecstasy, a secret Abbaye. I cannot live much longer on the East Coast. Now I hear Janine's little Opel driving into our garage. I am happy and I do not know how to measure my own happiness.

### **Willingboro. Wednesday 6 August 1969.**

Fred called me from Chicago tonight.

"I was looking out the window over the empty lot across the street. I saw a man who was running back and forth with a huge butterfly net, under the light of the mercury lamps, and I thought: It's been a long time since I've called Jacques!"

### **Willingboro. Thursday 14 August 1969.**

It is hard to believe that we are spending our last few days in New Jersey. We don't open the windows any more for fear of all the mosquitoes.

At the office my mathematician friend and I had a conversation about consciousness. I reported to him a new question posed last night by the fertile mind of five-year-old Olivier, who wanted to know "How did it all begin, the days?"

Olivier has a theory that the world started "when all the grownups were at the hospital," since that is where babies come from. A deduction of impeccable logic. My friend tells me that as a kid he was fascinated with the observation that human thought could think about itself. That aspect also captivated me: the identity of every "I" in its relationship to the whole being. Could it be that there is only one 'T'?

### **Willingboro. Saturday 30 August 1969.**

In the small room downstairs boxes of books are piling up again. Yesterday I took down all the shelves. Janine went out to buy our airline tickets. Her car is sold and I have officially resigned. We have a lot of

practice in mobility. She feels like me, a traveller on the earth. To move is a magical experience, a distillation. One of my colleagues commented, when he learned the news: "I knew you wouldn't last very long here. I've been watching you work. It was like standing before the tiger's cage at the fair."

Carl Jung writes in *Alchemical Studies*:

I had learned that the greatest and most important problems of life are fundamentally insoluble. They must be so, for they express the necessary polarity inherent in every self-regulating system. They can never be solved, but only outgrown.

What is the method making such developments possible?

We must be able to let things happen in the psyche. For us, this is an art of which most people know nothing. Consciousness is forever interfering, helping, correcting and negating, never leaving the psychic processes to grow in peace. It would be simple enough, if only simplicity were not the most difficult of all things.<sup>20</sup>

I believe I have understood that lesson in "difficult simplicity." All the movement in the world, including scientific progress, is rooted in the unconscious. The dogmatic view of science as an Absolute is a pretentious *notion* that has survived from the days of *aristocracy*, an illusion carefully fed and maintained by a privileged elite because it legitimizes scientific power. That the Rationalists should be the first victims of this idealistic illusion is an admirable and touching fact.

Science often denies any new observation or idea that challenges the Absolute. The true progress has to take place underneath the official structure.

When Hynek and I first met in 1963 he wanted to scrupulously preserve academic formalism while bringing UFOs into the forefront of scientific debate. He was anxious to publish a major article on the subject in a recognized journal, a laudable goal indeed. For my part, I wanted to inform a broader public and to quickly reach the younger generation, the scientists of the future who, I hoped, would turn out to be more open-minded than the men I saw in power.

Today, six years later, Hynek still hasn't been invited to submit an article on UFOs for *Scientific American* or *Nature*. A forthcoming meet-



ing of the American Association for the Advancement of Science is supposed to discuss the subject but it is already shaping up as a futile debate about philosophical generalities. I have decided not to attend.

Discoveries are often born out of legend, chance and chaos. The deeper psychic chains remain invisible, even if scholarly books later rationalize the process and demonstrate in hindsight its inescapable logic. The fabulous ocean of human unconscious and conscious dreaming bears the little raft of science over its foamy waves like a mere toy.

If folklore expert Hartmann is right when he says that "human imagination obeys fixed laws," then it ought to be possible to speed up or slow down the development of science in a given culture, not by the play of budgets but by spiritual means, or by exposing the target culture to novel images. Is that one of the grand mythical roles served by UFO phenomena?

### **Willingboro. Sunday 31 August 1969.**

An ultimate image of New Jersey remains fixed in my mind, with the parking lot *of* the software *factory as* a backdrop. Olivier has *come* with me to help carry out some boxes to pack our books. Today I don't have time to let him play with the terminals connected to our machines. I see my son's little face behind me, framed by three big boxes he carries as he stumbles about. The thick, humid East Coast summer hangs around us. We can't see any blue sky: only powdery, heavily polluted air mixed with the ground dust and the sweaty asphalt. Summer has turned into an endless ordeal.

Last Thursday I flew back to California for more job interviews. This time I slept at the Hotel St. Francis in the center of San Francisco, courtesy of Shell Development. I saw Paul Rech again. He introduced me to one of his colleagues, systems manager Mike Kudlick. We were friends immediately. Yet there was a curious atmosphere of insecurity in their shop, because people are often "promoted" out of the area by the company, generally to Houston where Shell has its American headquarters. That isn't my idea of a promotion.

I paid another visit to Stanford and something interesting happened there. I had been asked to give a seminar on data-base management. They even asked me if I would like to join the Faculty. The talk was well received. I had hardly stepped down from the podium when a short,

red-haired fellow with a short beard approached me, introduced himself as Rod Fredrickson, director of the Campus facility for computing, and asked me to come into his office. I suddenly realized that this was the man whose team had built the extraordinary operating system I had admired on my last visit. He closed the door behind us.

"Look," he said as he sat down next to his massive IBM terminal, "I'm a guy with a very big ego. I believe that if you give me a problem and you give me a computer I can solve that problem with that computer, period."

I looked at him, wondering what would come next. There wasn't much I could answer to that introduction.

"There is one exception to what I just said," he said with a chuckle, "and it's information retrieval. I don't understand it and I can't fix it when it goes *wrong*. All the projects that use data-bases around here are screwed up, including the Communications Department project which is eating up more and more time on my machine. You seem to know what you're doing in data-bases. If you want to come here instead of working for those *guys* I'll hire you as manager of *information systems* for the Campus. But if you haven't fixed the problem for me in six months you're fired."

"When do I start?" I asked him.

We struck a deal. Next week we move *to* California.

### **Willingboro. Monday 1 September 1969.**

It makes me nervous to keep this diary, to maintain a regular catalogue of fantasies which are only a rough draft of future life. It would be much easier to write after the fact: then fevers and follies could be carefully built up or smoothed out, the sudden impulses could be explained, transitions could be skillfully managed.

What interests me here is the process by which thought bears upon the real world and eventually results in action. But thought can only be observed when it is caught in the inspired jumps and somersaults of everyday impressions. Too bad for the historian who naively believes he can describe reality by drawing an average line through such follies. It is the peaks and valleys of individual life that count, even if they don't lead anywhere. The doings of famous people are important, like the dates of major battles, but they don't give the essence of an era.

All I can do here is describe and report, capturing specific points of view, perspectives on temporal landscapes that are always contradictory, illogical, linked together by the single irrefutable fact of my own consciousness, my painful awareness.

Unfortunately I won't have much time *to go on* keeping this Journal once I start working at Stanford.

**Stanford. Sunday 7 September 1969.**

Wednesday was our last day in New Jersey. By nightfall we had emptied the house. Warm rain fell in the narrow night. We left the children with Maud in a motel while we drove the car to the movers. In the middle of their warehouse, incongruously, we could see our sofa, lost like a small rowboat among the battleships in the navy yard. Finally our taxi arrived to take us all to the airport. The rain had become intense. Several intersections were flooded. A stormy weather system covered the whole Atlantic coast that night.

We first flew into Chicago where Janine visited her brother while I met with Allen and Don. I spent Friday talking to Fred. Their world has not changed.

For the last three days I have been showing Janine what little I've learned of San Francisco and discovering new parts with her. We had a picnic with the children in a clearing at the foot of giant trees. We have started to visit houses all over the Peninsula.

This evening I saw Half Moon Bay again. A half-naked girl on horseback was riding on the beach, her breasts in the wind, a can of beer in her hand. Behind her the ocean in turmoil was throwing surfboards up high above the waves. We heard the laughter, the music of a very young world where everything is possible, everything could easily be destroyed and rebuilt, and that colorful name: Pacific.

**Stanford. Sunday 14 September 1969.**

I just had a night of strange dreams and sadness. I feel uprooted, caught up in a swirling current. Today we drive up to Mount Hamilton to visit Lick Observatory. In three weeks I fly back to France for the next International Standards meeting and another encounter with General Lochard. I am told that his French friends have counter-attacked: they have hired an American from IBM France to sit behind the *Tricolore* and

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**work** out the tougher definitions with me!

On my return we will settle in our new home at last. I will be able to resume my private research into the paranormal. But it will probably take much longer for my emotional being to find its bearings again.

It is only when I hold Janine in *my arms* that the world *stops*, opening up an immense landscape. She is still the girl in the simple dress with whom I used to listen to Mahalia Jackson songs in my tiny bedroom near Porte Champerret; she is the pretty psychologist from Lille who used to pick me up at the little observatory in her blue Renault all shiny with rain; the smart career woman in Chicago, carrying thick computer listings under her arm, and smiling at me through the snow and the wind; *my* companion for the whole journey.

Olivier is often upset these days. He has left his friends behind again, has no school to go to and runs around in narrow circles. At other moments, however, he is loving and very tender with us, no doubt sensing what we are going through. Yesterday he presented us with his treasure, a little stuffed donkey, for us to sleep with.

Yesterday we went to the beach and Catherine braved the tide, both little feet firmly planted in the cold sand, shiny waves crashing around her in silver mist. When the wind turned cold we climbed up the cliff. Perched on my shoulders she turned around and yelled poems of her own invention to the Pacific far below.

The first reviews of *Passport to Magonia* have appeared. Many UFO believers are upset because I question the "nuts and bolts" model: for them flying saucers can *only* be spacecraft sent here by some other civilization in space. "Vallee has gone *off* the deep end," I hear these people say, denying any parallel between reports of ufonauts and other strange visions in ages past. Many of them had refused to consider these same "ufonauts" until *Anatomy* was published three years ago. But things are not as simple as they would make them.

The success of the book testifies to the fact that some fraction of the public is seeking a more sophisticated statement of the problem: such scenes have always existed, they have always inspired our dreams, perhaps even our science.

The *Library Journal*, however, is strongly recommending *Magonia* as "the one book on the subject even the smallest collections should want to stock."

This brings me to the most recent meeting of our Invisible College at Hynek's house. Fred began by saying again that we must look under the bed. There must be a secret study in progress somewhere, he insisted.

"Do you say this because you have some new data, or are you just posing the question again?" I asked him.

"I don't have any new data," he confessed.

Bill Powers then said what he always says whenever someone raises the issue of a secret study of UFOs. "That can't be true. The story would leak out. We would meet their agents in the field. You shouldn't assume these guys are intelligent just because they work in Intelligence."

Fred answered what he always answers when Bill says this. "They may not need the same data we are seeking. Why should they bother with civilian sightings? They would have access to gun camera footage, radar records. They have the ability to put a smokescreen around the whole business. It wouldn't necessarily leak out."

We have heard both sides of this argument many times. Hynek broke in.

"If I know that another astronomer is using a telescope of eighty-inch aperture to look at the same stars I am investigating with a small Questar, what do I care? My own research satisfies me. Nothing guarantees that his results will be more scientifically significant than mine. Perhaps I am smarter than he is. Perhaps I am a better observer. Perhaps I am looking at some characteristic he hasn't identified yet."

"That's true," I said, "but it isn't a very convincing argument. All it says is that you don't want to look under the bed."

I did not bring up the Pentacle letter. Neither Don nor Bill were aware of its existence, which has remained a tightly guarded secret between Fred, Allen and me.

I have to agree with Fred when he says that the Air Force kept Hynek around only as long as he was silent. I came to Evanston six years ago and put pressure on him, urging him to change his stance. A string of important cases forced the issue. When he started talking,

arguing for a new study, the Air Force simply pushed him aside. First they defused the issue by getting their most vocal opponents to testify before bogus Congressional Hearings; then they selected Ed Condon, a physicist who was about to retire, and he signed his name to a report which was a travesty of science, yet reassured the establishment. They used that report

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to bring about the liquidation of Hynek's position, but they were careful not to fire him.

Allen is now fifty-nine years old. He still goes to Dayton regularly and remains on the payroll as a part-time consultant. He never sees Quintanilla, who still works there with a lieutenant and a couple of secretaries. He is received personally by the commander, who hints he might put him on his own list of consultants some day. Colonel Winebrenner, a former military attache with the U.S. Embassy in Moscow and in Prague, takes him to lunch at the officers' club.

"What do you talk about?" asks Fred.

"Oh, we talk about lots of things, ordinary things like the weather," answers Allen innocently. "We talk about the places we have both travelled to. Foreign foods, European cuisine. He snaps his fingers and old bottles of Chateau-Latour materialize on the table in front of us. He speaks to me in Czech. He even gave me a copy of a UFO novel, *The Fortec Conspiracy*."

Hynek has been charmed and neutralized by the Air Force. That doesn't mean anything, and it especially doesn't mean there is an ongoing secret study. It is highly undesirable for the Air Force of any country to have the citizenry believe in the reality of a phenomenon against which our jet fighters are powerless.

But what kind of science is this, if it is only allowed to discuss those phenomena it can explain? There should be room for other phenomena, for another science. We have the opportunity to help with its birth.

Sadly, our discussion went nowhere. Hynek yawned, got up from his armchair and suggested we listen to a magnetic tape he had received from a UFO witness in the Northwest. I wasn't ready for another light in the sky. I said we should first agree on some sort of practical, tangible joint action.

"We don't get together that often any more; we should use the time as productively as possible," I said. "In any case Don and I are committed to continue working on the landings catalogue," I pointed out somewhat bitterly. "I assume we have at least your moral support. Allen and Fred could consolidate the contacts we have begun with scientists who have volunteered their help, to form a real information and investigation network."

"That's really what we need," Hynek says, "making the Invisible Col-

lege visible. But it should never become a formal UFO group. That would defeat its purpose **completely**. Hell, it would be a disaster if it turned into another NICAP, or another APRO, with dues and memberships and a whole management structure, and people fighting for turf all the time."

We all agreed we wanted no part of such an organization.

The next day Fred insisted on driving me to O'Hare. He wanted a word with me in private. He showed me some very interesting photographs taken from an airplane.

"Do you know who took these? Allen did! But he hasn't recorded the place, the date or the time\_\_\_"

It turns out that Allen was aboard an airliner when he suddenly noticed a white object at his altitude, seemingly flying at the same speed as the plane. He made sure it wasn't a reflection and he convinced himself it must be some faraway cloud with an unusual shape. He pulled out his camera "to see how fast he could snap pictures." In all he took two pairs of stereoscopic photographs and gave it no more thought.

Fred only learned of this a few weeks later. By then Hynek had lost the negatives and one shot from every pair was missing. All that was left consisted of two enlargements, taken separately, showing a well-defined white object, the top rounded like a lens, the bottom cloudy and asymmetrical. Naturally the loss of the negatives makes it impossible to determine whether it was really a cloud or not. Fred is indignant: "Sometimes I have the feeling Allen just doesn't want to know," he *says*.<sup>21</sup>

### **Stanford. Sunday 21 September 1969.**

Yesterday, as we were all horsing around in the motel suite, Olivier fell hard against the angle of the coffee table. Blood gushed out of his forehead. We drove to Stanford Emergency. An intern arranged his head under the light, closed the wound with a few stitches and told us there would be no worrisome consequences. Olivier had not even lost consciousness and did not seem in pain. Yet the incident made us all aware or how vulnerable and insecure we were. All our earthly possessions *will* have to stay in storage until we find a house. We live precariously in two rooms at the Tiki Inn *on* the edge of campus, eating every night with the student crowd at the nearby sandwich and pizza place, where we listen to rock music and put quarters in the Pong machine.

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Today we drove over the San Mateo bridge to visit Berkeley. We had lunch in the foothills of Mount Diablo.

I am reading a book about World War II as seen by the simple folks of Normandy and I wonder: how many Americans my age could understand what that era meant? How many have ever discussed the war with an actual witness of it? How many, for that matter, have ever seen a horseshoe maker at work, a water mill in operation, a woman using a washboard in the river?

### **Stanford. Wednesday 24 September 1969.**

We have found a fine place and we are trying to buy it, with the feeling that we are making an investment much beyond our means. It is a large wood-frame house in the hills of Belmont, so filled with light it seems magically suspended in mid-air over the landscape.

It is hard to pick a location in this land of microclimates and micro-cultures. We almost fell in love with a very similar house above Half Moon Bay, but it would have been engulfed in fog a good part of the year. We decided we wanted the sun.

Today is my thirtieth birthday. In one week I go back to France for another standards meeting, but this time I know I will be able to return to a real home.

### **Paris. Friday 3 October 1969.**

The kind futility of France enfolds me once again. I am reconciled with it. I experience with delight this country that doesn't change, its amiable folks hopelessly dominated by an egotistical elite with an enormous ability *to* exploit those it should serve.

This afternoon I walked all over the Latin Quarter with my mother. The weather was warm and diaphanous, with that delicate expectation of Autumn that the light places in the eyes of the people, the kids playing on the sidewalks, the stained glass windows.

On rue Saint Jacques I paid about five dollars for *The Other Side of Nature* by Mrs. Crowe, but I was disappointed by the little bookshops along the Seine. Little of value is to be found among the *bouquinistes* any more, only silly watercolors and overpriced copies of the Kama-Sutra for sexually repressed tourists. At Vivien's bookstore the owner recognized me and shook my hand. I bought an old parapsychology book



from him. It shows curious photographs of grave experimenters watching a table floating up into the air through the psychic action of medium Eusapia Paladino. One of them, a young man with a beard, is none other than astronomer Camille Flammarion.

At *La Mandragore*, a miracle! The exquisitely rare, complete three-volume set of *Les Farfadets*, the fanciful first-person story of a nineteenth-century man named Berbiguier who was convinced God had sent him to the earth with the specific mission to destroy the plague of these ugly little devils. The most curious thing about the book is that the act of publishing it seems to have cured the folly of its author, who spent his last years buying back and destroying as many copies of his work as he could, thus increasing the value of the few remaining ones.

Maman and I had coffee near the Odeon. We spoke about the era when spiritism was in vogue. She remembered that time well. She told me about the fascination of the Belle Epoque for ghosts and mediums. Her own father, a businessman, was a member of the French Astronomical Society and often attended experimental seances inspired by Camille Flammarion. Unfortunately, in the fashion of the time, a superb banquet usually preceded the scientific work, and the liquid spirits played a more extensive role than the paranormal ones.

After coffee we decided to pay a surprise visit to my uncle. He was putting the final touches on an equatorial mounting of his own design for a new telescope, although he can hardly hope to see the sky from the dark recesses of his apartment. Such details have never bothered the scientific enthusiast. We came back on foot at nightfall. Workers were digging up the square in front of Saint-Sulpice to build a large underground parking lot.

Few other nations have that ability for happiness the French possess, so deep and clear. It can be seen in the lovers' quiet abandon, in the smile of passersby. Kids take each other by the arm familiarly, friends embrace in happy ways. Why are Americans so afraid of touching each other?

### **Paris. Saturday 4 October 1969.**

This city is a hot elixir of gasoline and melting asphalt, its smell a mixture of the burning metal of car engines, perspiration of crowds, effluvium of coffee and chocolate, perfume of elegant women and the

ever-present whiff of dog crap. In this vast cauldron the fragrance is as heady and stupefying as the atmosphere of an opium den.

Something has changed in French social life. Is it because De Gaulle is finally gone? People do seem more free.

What was the probability that I would meet Granville on rue Monge, when I have only seen him once since the Jules Verne Prize? He was walking towards me, lost in thought, wearing a purple shirt and a gray sweater, carrying a grocery net. We laughed at this chance encounter, and we laughed even more when I attempted to explain what had happened to me: that I was spending my sixth year in the United States, but that I had lived in France for most of 1968; that I was here for a computer science meeting, but as a member *of the American* delegation; that I was *still* interested in astronomy but I hadn't worked at the telescope since 1963\_\_\_\_ I took him to a Chinese restaurant nearby to catch up on our lives.

### **Paris. Sunday 5 October 1969.**

The whole city was happy and carefree last night. Near the Tour Saint-Jacques whose stone chimerae have finally been freed from the workers' scaffolding, it seemed that Paris had almost recovered from the loss of Les Halles. On the Beaubourg square a flea market for old metal objects was a paradise of rusty chains, iron lamps, unmatched wheels.

### **Paris. Saturday 11 October 1969.**

Autumn has come, the subtle doorway into the new season has been crossed. I know it in the sudden depth of faraway engine sounds, in the lingering whistle of tires on wet pavement down there on rue Monge. Paris is a fine soft machine, recording and absorbing indifferently the changes of the millenium.

On *Wednesday night* I had dinner with Granville. In our *conversation* could be heard an echo of our old debates.

"We are the men of judgment," he said. And his own judgment on this superficially happy country was pitiless: he described to me the work of a certain ministerial commission on education, the endless studies for the reform of the school system, the despicable way retarded children are treated. His voice was that of someone who was appalled, discouraged, angry. Under the pretense of vague experiments nothing is done. French education, he said, was sinking into chaos.

Nor is there any real move towards greater moral freedom: only the annual national orgy at Saint-Tropez beach, and in Paris a few groups of hippies who are carefully preserved, like the Mona Lisa, to show the world how daring and creative we all are. In reality the true feelings of yesteryear are gone; the old intellectual circles are broken; the enthusiastic social movements are dying, he claims, as unlikely *to* be heard again as the plaintive guitar in our little cafe.

"They have created an Old Town, just as your Americans did in Chicago: the snobs from Neuilly come over every evening to see what the snobs from Passy are wearing. Golden adolescents in Jaguars pretend to reject the conservative world around them and sip their whisky while perfumed minions parade back and forth on the sidewalk. Tourist buses from Dusseldorf gape at all this and are amazed. Is it the same in America?"

No, it is not the same, I said. I described the insane tom-tom of an African priest in Washington, the lawn on Dupont Circle where a ~~drugged-out~~ girl breastfed her baby while the Black Panthers made speeches about setting the entire continent on fire. Something drastically new and unpredictable is happening in America, **I** told him.

### **Paris. Sunday 12 October 1969.**

Aime Michel suddenly appeared at my bedside at ten this morning while I was quietly reading Berbiguier. He is turning fifty, but has lost nothing of his enthusiasm and wit. We called up Guerin and we all went to lunch together. Guerin believed that a vast cover-up explained Blue Book, the Air Force, the Condon Report and everything else. His theory was told in a breathless tone but it left me unconvinced. I think there is indeed a cover-up, but it isn't that simple.

In Guerin's old office at the Astrophysical Institute where everything began, the very same basement office he had ten years ago, we argued about flying saucers. We walked down the same dusty hallways crowded with wooden crates which testified to bureaucratic carelessness. Around us the heavy benches, the antique electrical instruments and the bulletin boards where Union demands were pinned, threatening immediate strike, added a smell of constrained rationalism to the place, with that special lingering odor of crushed ideals and mental sweat without which no French research institution could ever attain excellence.

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We laughed, we argued about every camp in ufology. We debated the case of "Doctor X," the health official from Provence who watched in amazement last October as he saw two large disks merge into one. Aime gave us the unpublished details, which involve levitations and strange healings. And he mentioned the red triangle which appeared on the *mans* abdomen and *on* that of his child, *for* which his colleagues in dermatology have no explanation.

Guerin has not changed much. He is kind and warm, always excitable, open to all arguments presented in good faith, capable of uncommon depth in his analyses, even if he sometimes falls into dogmatic attitudes. Perhaps he lacks knowledge of the larger world, but he is pure of heart, a rare feature in this city of false intellectuals and consummate politicians.

Tonight the shopowners are demonstrating. Once more the Latin Quarter is full of cops in riot gear. The scene reminds me of the days of Charonne. Have they learned nothing?

### **Paris. Wednesday 15 October 1969.**

At thirty I can leave behind the memories, the hope and the rage that took me away from the darkness of Champerret and the drizzle of Lille, from the night dew of *Meudon*. We left France, burning to see other crowds, other lives. Why deny that I am very happy?

Yet our research has not answered the questions which started it. I *still* don't know what I saw in the sky over *Pontoise* in 1955. I *still cannot* tell what game various governments may be playing with UFOs.

People wear so many masks! That much I have discovered. Our words are masks, just as the daily newspaper is a mask journalists of different persuasions put over current events and the facts of life. Gestures, habits, customs are masks. Official science is the mask we put over knowledge so that no one will glimpse the terrible grimace or the tantalizing, seductive beauty of the unknown. Doctrines and philosophies are the masks that fit over changing, unpredictable, iridescent human thought. Academies are the rigid masks we impose on the face of creativity and of discovery.

There are very few times when we see the world without its masks, the real world of man and things rather than the carnival of our own delusions. My childhood visions of the war bore the unmistakable stamp of authenticity; so did my parents; Aime Michel drinking Chinese tea and speaking about Liszt; the evenings at Bryn Mawr with Allen Hynek,

Fred and Don; the naked girl of Half Moon Bay; dawn coming over us; our silences \_\_\_\_ Those are the few exceptions.

Such images condense into a single sphere that shrinks to a tiny point in arbitrary space, that space we call mankind. Then a haze comes over the magic mirror, my consciousness returns to drown the vision into the trivial present again.

**Stanford. Saturday 13 December 1969.**

The plane has taken me back to our house on the hillside, well-sheltered from the cold foggy Western wind. From our redwood deck we can see the Southern part of San Francisco Bay, from Mount Diablo to Mount Hamilton. So this is where we are going to live.

At Stanford the windows of my office in the computing center are always open. There is a flow of fresh air, heavy with the fragrance of a giant eucalyptus tree nearby.

Our weekend excursions around the Bay leave us with postcard visions of horses drinking from ponds in the hills, fiery sunsets beyond the broken fences of old ranches, **campfire** smoke in the steep forests. Why does it all seem so unreal?

Everyone knows such scenes belong in movies. Reality is supposed to be made of rain and blood, mud and grease.

I am aware of a faraway future, with no other country than the whole planet, no other nourishment than Janine's smile, no other vision than this: a tremor in the great skies above, a cloud we catch as we fly, high over the stormy currents. What is this diary, this record of a few passing years? Only the iridescent foam left behind on the beach when the big waves of existence arise, swirl in tumult and go away as we will go away, my love.

# EPILOGUE

The passage of time is highly corrosive. Not only does it erase from our memory many facts, dates and figures, but it erodes even our perception of those human beings who have made an impact on our lives, and it distorts our view of ourselves. A diary, kept religiously enough, is a formidable weapon against this erosion. But it also makes our shortcomings more obvious, our failures more plain. A source of humbling experience, it puts even our proudest achievements in the perspective of far greater accomplishments by others.

This volume would be flawed and incomplete if the story was simply allowed to stop abruptly at the dawn of the seventies. Nearly a quarter century has elapsed since the last entry of this diary was written. It is natural for my readers to ask what became of the protagonists, what findings were made in the intervening period, and how did the events described here determine the present state of the UFO problem and influence its future. These issues can be addressed under five major headings.

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*The **first issue** involves the over-arching question of the reality and possible nature of unidentified flying objects. The sad truth is that they remain as much a mystery today as they were in the sixties.*

The major cases I had recorded in the Journal as they were unfolding—Socorro, the Michigan "Marsh Gas," Monticello, the Hill investigation and many others—were followed by equally sensational events in the seventies and eighties. The new cases, such as the encounter involving two fishermen at Pascagoula, or the Travis Walton abduction, captured the headlines and repeatedly sent Allen Hynek before the bright lights of the media, only to be forgotten a few weeks later. These cases augmented the database but they elicited no new pattern. On the contrary, it seemed that the UFOs took a sadistic pleasure in sending us confusing signals.

A new computer analysis of historical trends, compiled in the mid-seventies, led me to plot a striking graph of "waves" of activity that was anything but periodic. Fred Beckman and Dr. Price-Williams (of UCLA) pointed out that it resembled a schedule of reinforcement typical of a learning or training process: the phenomenon was more akin to a control system than to an exploratory task force of alien travellers.

There are many control systems around us. Some are part of nature: ecology, climate, population development are common examples; others are social, like the process of higher education, or the Justice system, or a concentration camp; still others, such as the attitude control of a rocket or satellite, or the humble thermostat on the wall of an apartment, are built by man.

If the UFO phenomenon represents a control system, can we test it to determine if it is natural or artificial, open or closed? This is one of the interesting questions about the phenomenon, a question that has never been answered.

The publication of such ideas in *The Invisible College*, a book I wrote in 1975, strongly polarized researchers, because the issue of the psychic nature of the phenomenon naturally had to be raised in the same breath. This was anathema to many people for whom flying saucers could only be nuts-and-bolts spacecraft, an idea we had already left behind.

Coming a few years after *Passport to Magonia*, the publication of *Invisible College* deepened the cleavage between my research and the believers' party line. It has continued to widen to this day and has grown into a chasm.

On one side my colleagues in science "know," or believe they know, that the field is utter nonsense and that the witnesses are either hoaxers or poor observers tricked by hallucinations. As for my friends in ufology, on the other side, they "know" with equal force that these objects are extraterrestrial. I cannot join either camp on the basis of the data I have accumulated. Sometimes I get the awful feeling that I am the only human being who doesn't know what UFOs are.

The UFO occupants described by witnesses in close encounter cases are variously designated in the literature as Aliens, Visitors, Humanoids or Operators. They have continued to behave like the absurd denizens of bad Hollywood movies, giving no sign that their purpose on our planet was related to any sort of rational process. Worse, among the thousand or



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so abduction cases that have reportedly been analyzed, no pattern has emerged that could be positively correlated with a visitation by extraterrestrial entities. Their technology is a **simulacrum**—and a very bad one at **that**—of obsolete human biological and engineering notions. The real mechanism of their elusiveness and their absurdity clearly escapes us. Perhaps this should be taken as a sign that our theories are wrong, that our basic assumptions are flawed?

Before we can proceed we must have a more precise definition of what most ufologists mean by "extraterrestrial." Today the dominant interpretation of the term is still understood at the most obvious level: UFOs are thought to be spacecraft from a civilization that has evolved on another planet. Their pilots are supposed to be humanoids, generally "Short Grays" with large dark eyes who first came here about the time of the Kenneth Arnold sighting of 1947. We are told they are surveying the earth in search of mineral or biological material, that they abduct humans to interbreed with them. It almost sounds rational.

Not all ufologists follow precisely this explanation, of course. There are many variants. Yet the above is a fair summary of the "extraterrestrial" hypothesis prevalent in the current American literature of the field. Its imagery has been reinforced by major motion pictures like *Close Encounters of the Third Kind* (1977) for which Dr. Hynek was an adviser, and *E. T. the Extraterrestrial* (1982). It is also found in countless science-fiction movies and television documentaries.

The fact that many witnesses actually describe something entirely different, which does not trace its beginning to 1947 or even to this century, and only occasionally resembles a spaceship, has been neglected. Even when they describe alien creatures, those do not necessarily follow the standard type of a dwarfish humanoid with grayish skin. The shapes and behaviors offer a bewildering variety.

My own speculation is that UFOs operate in a multi-dimensional reality of which spacetime is a subset. In that sense *I do not completely reject the idea of an extraterrestrial origin*: but I believe that the form of intelligence the phenomenon represents could coexist with us on earth just as easily as it could originate on another planet in our universe, or in a parallel universe.

Scientific training is a heavy burden. I was taught by my mentors that science began with the ability to challenge all theories, including

my own. But any questioning of the interplanetary origin of these objects is perceived as a betrayal by those who need extraterrestrial contact as a part of their personal certainty. These people may pretend that they are looking for scientific truth, but in fact they are simply erecting a new dogma.

The stubborn refusal on my part to follow any party line has created some regrettable confusion over the years. Inevitably, various absurd theories have been attributed to me, and statements I never made have been placed in my mouth. For example, my assertion that UFO phenomena were partly psychic in nature was often taken to mean that witnesses were the victims of mere illusions, and that the objects were not physical or material, something I never said, wrote or believed. Later, my observation that a few of the cases were blatantly manipulated by human cults, often inspired by intelligence organizations, was misquoted as a statement that I had renounced my earlier writings and that in my view all "flying saucers" were human secret weapons or instruments of deception. I have made no such statement. To put the matter to rest it is appropriate, then, to restate my position regarding the phenomenon, a position which is consistent with everything I have written before:

***The UFO Phenomenon exists. It has been with us throughout history. It is physical in nature and it remains unexplained in terms of contemporary science. It represents a level of consciousness that we have not yet recognized, and which is able to manipulate dimensions beyond time and space as we understand them. It affects our own consciousness in ways that we do not grasp fully, and it generally behaves as a control system.***

Because it can manipulate our consciousness in unknown ways, the phenomenon also produces effects that we can only interpret as paranormal in nature. I trust, as Allen Hynek did, that the human science of some future century will account for these effects. Aime Michel continues to disagree with this assertion, and I understand his objection: no dog in future centuries will ever understand Einstein's relativity, because a dog's brain lacks the adequate structure to do it. Are we in the position of the dog? This is another important issue we have not resolved.

The UFO phenomenon plays a role in many mythological traditions. It has affected our religions and our modern view of the universe. It may well be deceptive in the images it presents to us, masquerading in various

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guises under different cultures: god-like to the ancient Hebrews or Mesopotamians, elf-like to medieval chroniclers, devilish to Christian inquisitors. It may have manifested in the form of ghosts or rapping spirits for the benefit of our grandfathers at the end of the nineteenth century, or in the form of the Blessed Virgin before devout Catholics. Today we live in the technological civilization of the late twentieth century, and we observe a phenomenon which emulates astronauts in shining space suits.

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*The **second issue** this epilogue must address is that of scientific reaction to the phenomenon. Here again, not much has changed since the Journals were written.*

The only reason the U.S. Air Force was able to get away with its ludicrous treatment of the problem is found in the appalling lack of information, indeed the lack of interest that exists among the academic community in the U.S. and abroad. To most academic thinkers the field of ufology is an aberration. And how could they come to a different conclusion? The genuine data have never been exploited. *The scientific work has never been done*, and the reader of my Diary can plainly see why: a few individual scientists like Hynek and myself and perhaps a dozen others spent their own resources and their spare time documenting tantalizing anecdotes, but the full machinery of science was never brought to bear on the phenomenon. Our greatest failure has been our inability to build a strong enough case before our colleagues, and to get a real investigation started. We simply cannot speculate about what might have been found.

As the Journals show, my early books aroused some measure of private support among a few scientists, of which Fred Beckman at the University of Chicago, Douglas Price-Williams at UCLA and Peter Sturrock at Stanford, who had come to similar conclusions through their own thinking, were courageous examples. A small "Invisible College" continued to develop in later years, but it was not able to undertake long-term research as a group, even when Allen Hynek came out in 1972 with his own perceptive work, *The UFO Experience*. It was a classic effort to launch what he aptly called "the natural history of the phenomenon" but it too failed to create lasting interest among the scientific world.

As I had once predicted to Allen, any effort we made to document the genuine cases and to place them squarely before the public also created a

lucrative market for hucksters who could always grab the bigger headlines simply by manufacturing lurid stories, which were eagerly seized upon by television news and by the tabloids because of their shock value. In the minds of many conservative scientists, a phenomenon that was so disgustingly exploited by the media and by wild-eyed, uncritical zealots was automatically deemed unworthy of their time and attention. Hynek's words and mine were simply lost in the noise.

By the mid-seventies those scientists who had a genuine interest in alien intelligence, like Carl Sagan at Cornell and Frank Drake at the University of California, Barney Oliver at Hewlett-Packard and Ronald Bracewell at Stanford, had a less slippery fish to fry: they had become involved in the Seti project, using radiotelescopes in the search for extraterrestrial intelligence through radio signals.

Most astronomers now agree that life must exist throughout the universe, notably on planetary systems surrounding slowly rotating yellow stars like our Sun. Wouldn't an advanced civilization on such a planet use something better than radio to make its presence known far and wide? Someone said derisively that Seti, like its predecessor Project Ozma, was an effort on the part of the long-time-dead to communicate with the not-yet-born! Yet the scientific press gave it much more attention than it did to UFOs. While fascinating as a technical exercise, Seti ignored the many genuine witnesses of UFO encounters who described a form of non-human intelligence interfering with human destiny right here on earth. It still ignores them, perhaps because the raw material of sighting reports does not look "scientific" enough and does not come to us through the channels of pure science. I regard the continuing lack of attention paid to UFOs by science as one of the great intellectual failures of this century.

If the science establishment was turning away, what of private research? From archaeology to medicine, there are numerous examples of rich mavericks or intelligent patrons spearheading novel areas of research that the Establishment has neglected. The names of wealthy families, from Kettering and Ford to Rockefeller and Carnegie, are associated with research foundations representing some of the finest achievements in the arts and the sciences. Unfortunately these institutions never took an interest in this field, although Hynek and others over the years made serious attempts to raise funds for a small, dedicated effort.

Many wealthy patrons have an axe to grind and tend to finance re-

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searchers who espouse their theories, investigate their ailments or flatter their eccentricities. Tycoons dying of cancer have occasionally endowed chairs in oncology, and rich families whose children have died young have funded parapsychology projects designed to communicate with departed souls. But the millionaires who truly sponsor speculative, frontier research for its own merits are very rare indeed.

Allen Hynek found this out the hard way. He was repeatedly promised vast sums of money which evaporated as soon as he made it clear he would never compromise his scientific standards. In the summer of 1984, bitterly disappointed with CUFOs (the "Center for UFO Studies" he had established in Evanston), he loaded his personal files into a truck and left for Scottsdale with a promise of support, this time from a wealthy Englishman. Unfortunately things did not go much better in Arizona. Allen asked me to join him there but we soon came to the conclusion that the wealthy patron was only interested in keeping a few scientists in his entourage to promote his personal theories about the phenomenon. Another wild hope bit the dust.

The research situation is very much the same today. Affluent individuals have occasionally provided modest amounts of money, but only to fund those projects that matched their own concept of the extraterrestrial origin of UFOs, to the exclusion of alternative theories. Not only is this an unacceptable bias in any field, but it almost guarantees that even those valid results that might derive from the whole exercise will be rejected as flawed by any scientific committee appointed to assess them. This is akin to someone agreeing to finance a new planetary observatory, but only on the condition that its astronomers be committed to the view that the earth is forever fixed at the center of the universe.

What little research is being done on the subject today is unfortunately exposed to all the vagaries of sectarian thinking. The valuable work of a few private groups continues to be disfigured by intense bickering among various factions. Independent scientists venture into the cross-fire of their vituperative arguments at their own risk, like a tourist caught in a western frontier town bar brawl. Good field research by dedicated investigators (fortunately, they are still numerous and active) rarely sees the light of publication. Many important sightings are buried forever.

By 1976 my own career had evolved. I had become a computer entrepreneur in Silicon Valley, much to Hynek's chagrin: "You should

be a professor somewhere," he kept insisting. Well, I had seen first-hand what happened to University professors when they showed too much independence, and I had no inclination to lose the freedom that expertise in high technology afforded me in California.

I saw Hynek often during those years, at my house in Belmont or wherever our travel schedules happened to coincide. Anyone listening in on our discussions would have been surprised to observe that we spent relatively little time on UFOs. To be sure, we did have sharp differences in that regard. He refused steadfastly to question the inconsistencies in the Air Force's policy. As my research led to investigations of ufological sects and cults, I became increasingly aware of the way belief systems were manipulated by outside groups with hidden agendas, although it would be some time before the full horrors of the classified mind-control experiments of that era would be exposed. Hynek and I disagreed on the urgency of pushing for a frank investigation of a cover-up on the part of the agencies I have named in this book. Yet he gradually came to agree with my statement that UFOs were probably not extraterrestrial spacecraft. In October 1976, he courageously told a journalist: "I have come to support less and less the idea that UFOs are nuts-and-bolts' spacecraft from other worlds (...) There are just too many things going against this theory. To me, it seems ridiculous that super intelligence would travel great distances to do relatively stupid things like stop cars, collect soil samples, and frighten people. I think we must begin to re-examine the evidence. We must begin to look closer to home."

Invariably our talks veered towards deeper waters: the latest developments in parapsychology, the psychic nature of man and the failure of science to come to grips with higher levels of consciousness. We debated the phenomena of mysticism and the meaning of initiation.

The man I spoke to on such occasions was the true Allen Hynek, and it is a great pity that his colleagues in science and his followers in ufology never heard or acknowledged what he could have offered them in that regard. He knew far more about parapsychology than he publicly revealed. After his death in 1986 his wife Mimi told me that he had wanted me to keep the books he had accumulated on this subject. Today they represent a very special and treasured section of my own library.

In my present professional work in the financing of technology, I often reflect with some bitterness on the lessons of those formative years—

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the display of blatant bias and obvious dishonesty given by the various academic projects that dealt with the paranormal, the debacle of Project Blue Book and the spectacle of pettiness and fear given by the pundits of science who prejudged every case without serious consideration, or simply destroyed the data, as my superiors had done when I was a young astronomer on the payroll of the French space committee.

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*The intellectual scandal of the Pentacle document constitutes the third issue I must address here. It is hard to excuse the betrayal of science that took place when the Intelligence community decided to bar the Robertson Panel from direct access to the knowledge Pentacle and his group had gained.*

The discovery of the Pentacle document had a major impact on me. It gave me an uncomfortable insight into the practices of government agencies and the high-powered consultants who serve them. If I had remained silent on this issue, as I could have by editing the relevant entries out of the Journal, some of my past actions would have remained incomprehensible. It was the main reason for my return to Europe in 1967. It made obvious some unsavory aspects of scientific policy at the highest level. It provided quite an education for an idealistic young astronomer.

Having said this, I still do not quite know how far one should go in suspecting a sinister design behind this ominous document. A group of CIA officials had convened a panel of the five most eminent physicists in America for what purported to be an objective review of a series of cases of potential importance to science and to national security. The working conclusions reached by a prestigious research institution funded with taxpayers' money and clamoring to be heard were withheld from them, although "Project Stork support at Battelle" was briefly described at the meeting, according to the (now declassified) report on the January 1953 meeting written by F.C. Durant, and addressed to the Assistant Director for Scientific Intelligence of the CIA.

The fact that no member of the Battelle team was asked to elaborate on the findings alluded to in the Pentacle memo is an amazing fact. The reader will recall that the panel was no ordinary group of consultants: Professor Luis Alvarez was awarded the Nobel prize in physics; Lloyd Berkner was a leading space scientist; Sam Goudsmit was one of the

acknowledged leaders of American nuclear physics at Brookhaven National Laboratory; Thornton Page was one of the most respected astronomers in the land. The panel chairman, H.P. Robertson, was a world-renowned physicist at the California Institute of Technology.

There will be those who will say that Hynek should have stormed up the steps of the National Academy of Sciences with this document in his hand as soon as I had dredged it up from his archives. They may be right. But Hynek was a quiet man, who disliked confrontation and scandal, feared authority and was in awe of secrecy. He had once told me plainly that *he would not look under the bed even if he knew for certain that something was hidden there*. The document I had found in his files remained poetically ensconced in the frame where I had inserted it, under a color reproduction of a panel from the *Lady and the Unicorn* tapestry. The frame hung for a long time in his office at Corralitos Observatory in the barren mountains of New Mexico, where it was safe from the prying eyes of curious reporters or nosy ufologists. It is only after much soul-searching that I have decided to reveal its existence.

To those who believe in conspiracies, the Pentacle document may come as further indication of a cover-up dating at least from 1953. In a novel in French entitled *Alintel* (published in 1986) I developed a scenario showing how Pentagon-sponsored UFO research could have been taken underground after the Robertson Panel. In that novel I showed how Project Blue Book could have continued as a deliberate ploy designed to deflect away the attention of the technical community and the public, while a very small group of experts went on examining the data. Yet more conservative observers of the UFO scene can justifiably argue that the ominous memorandum is only proof that some important findings were withheld from Alvarez, Robertson, Page and their colleagues, not that an elaborate plot was being hatched. If so, why were the Battelle conclusions not made available? Could it be that Pentacle's clever, detailed recommendations to set up deliberate artificial UFO flaps and simulated cases in selected areas was actually implemented? Is that the explanation for some of the bizarre sightings we were to observe in later years? When I called attention to the blatant manipulation of belief systems that lurked behind such hoaxes, many researchers rejected the whole idea. I found it hard to defend myself, since neither Hynek nor I were prepared to talk publicly about Pentacle.



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Now it can no longer be denied that as early as the mid-fifties the Intelligence community was seriously considering precisely this kind of deception, and that it conceived its designs on a grand scale. Later public confessions by independent researcher William Moore concerning some of the covert actions engineered by the Air Force's Office of Special Investigations (OSI) support this scenario. I have reviewed these plots in a recent book called *Revelations* (1991) and it would be useless to belabor the point. I must leave it to future historians of the field to decide objectively whether or not there exists a secret project along the lines of *Alintel*. But the Pentacle document represents the kind of negative factor in the inner workings of science that sociologists would do well to investigate instead of spending their time heaping ridicule on the witnesses of the UFO phenomenon who are only trying to offer their testimony as a gift to modern research.

Today it seems likely to me that the executive branch of the U.S. Government and other major governments do know of the existence, physical reality and awesome implications of the UFO problem. It seems obvious that an agreement is in force among them to keep the data quiet and to discourage independent research. The repeated experiences we had in the sixties when our discreet requests at a very high level within the French government hit a brick wall of secrecy and denial are a strong indication in this direction. Allen Hynek's experiences with Washington were similar. Such withholding of data without Congressional authorization is illegal, of course. The military has no right to deliberately lie to the citizenry or to mislead scientific researchers on such a fundamental issue. But when we look for a deeper, more sinister conspiracy we may simply be underestimating the depth of bureaucratic stupidity. More light should be thrown on the whole problem.

Unfortunately my hands are tied: As a private citizen, I have no authority to reveal the name of the person who signed the memorandum, or his associates. Dr. Hynek never did, although he occasionally hinted that the whole story of Battelle's involvement had not been told. So far, my efforts to establish whether or not the memorandum is still secret have gone nowhere: nobody even seems to have a copy. The Air Force claims it has kept no relevant files from that era. Wright-Patterson Air Force Base says it has no record of Pentacle, and the National Archives respond to my attorney's inquiries with standard brochures about Project Blue Book

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that are intended for fourth-graders. My hope is that a patient investigator will eventually go through their collections, recover the actual text and obtain its release so it can be debated in the open.

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*The **fourth issue** that demands to be raised here concerns the sectarian temptation that is undoubtedly present among UFO believers. Anyone who calk himself a serious researcher in this field, and has the courage to confront the skeptics, must also have the courage to expose the dangerous paranoia that is rampant among many zealots of the extraterrestrial Cause.*

When he left the field in April 1991, author Whitley Strieber characterized ufologists as "the cruelest, nastiest and craziest people I have ever encountered." This judgment is too harsh because there are plenty of hard-working, open-minded and generous researchers who have made remarkable contributions to our knowledge of the UFO problem. Unfortunately they are over-shadowed by vocal believers who react to criticism with all the sting and venom of zealots defending a religious dogma.

Any researcher who has not tried to engage the advocates of UFO nuts-and-bolts reality in rational debate can have only a faint idea of what the term "vitriolic" means. These advocates claim that they want to see scientists become involved in the study of the phenomenon, but it does not take long to realize that they only want scientists who agree with their preconceived ideas of its origin and nature.

Some of the more extreme, paranoid views are making an impact upon a wider segment of the public today because the very sensitive, emotionally charged matter of abduction research has become a central obsession for contemporary UFO groups. Various writers who have only limited experience with clinical psychology are allowed to interrogate witnesses under hypnosis, often leading them to fantasize in the direction of their own preconceptions. They spread them to a wider circle through books, films and lectures. Under a warm, comforting and sympathetic, even paternalistic appearance, these writers may be augmenting, rather than healing, the trauma felt by the witnesses; they create a dangerous sense of imminent crisis that heightens the anxiety of their followers.

A summary of the abduction debate may be useful at this point.

It should first be noted that during the period covered by the Journal, abductions were already recognized as one of the most interesting facets

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of the phenomenon. The case of Vilas Boas in Brazil had been researched by Dr. Olavo Fontes and published in English by Gordon Creighton. The reader will recall the many conversations Hynek had with these men and also with witnesses Betty and Barney Hill, with Dr. Simon and with John Fuller, the gifted writer who must be credited with first calling our attention to the curious fact of "missing time."

There were already a dozen abduction cases in our files by 1970. Some veteran researchers, like Coral and Jim Lorenzen, had accumulated many more. It was clear that abductions had been a part of the mystery since the earliest period. It seemed that the problem we were trying to tackle was a much more formidable one than the arrival on earth of space visitors, impressive as that possibility might be. The phenomenon challenged not only our definitions of physical objects but our concepts of consciousness and reality. At the same time it brought into question the entire history of human belief, the very genesis of religion, the age-old myth of interaction between humans and self-styled superior beings who claimed they came from the sky, and the boundaries we place on research, science and religion.

The abduction experience, in my opinion, is real, traumatic and very complex.

It is unfortunate that the small group of researchers who studied such cases did not stop to carefully develop an appropriate methodology. Instead, abduction research tended to drift into disputes between those who thought the Aliens were here to help us and those who saw them as evil. Betty Hill herself summed up her own disappointment when she retired from the field in September 1991, citing "flaky ideas, fantasies and imagination" in the treatment of abduction stories.

When I search for a reason for this state of affairs I find two major factors. First and foremost is the passing from the scene of those elder statesmen who could have cautioned against hasty conclusions. In particular, the late Dr. Hynek and the Lorenzens had accumulated enough experience with abductions and their investigation under hypnosis to recognize both the implications and the limitations inherent in such cases. But the new self-styled "experts" had no such hesitations.

The second factor that made alien abductions so visible in the late eighties was the sudden rise of tabloid television in the United States. Sensational new interview shows replaced the more sedate afternoon pro-

grams of previous years. Television stations concerned with their weekly ratings loved abduction stories because of their high visual and emotional drama.

As a result, when I hear skeptics like Carl Sagan or Paul Kurtz thunder about the growing dangers of the irrational in our society I find it hard to argue against them. Yet the perils created by the hard-core believers are only proportional to the neglect of the problem by those leading scientists who deny the reality of a phenomenon they have never attempted to investigate.

The UFO phenomenon is one of the major scientific and social mysteries the twenty-first century is about to inherit from our own. To investigate it responsibly we need the guidance of open-minded specialists ready to set some standards and to develop new methodology in the treatment of the more traumatic cases. This can only be done in the calm setting of the laboratory, not against a background of televised debates or screaming tabloid headlines.

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*The **fifth point** I must touch on before bringing this book to an end concerns possible future avenues of research, and my own plans within it.*

I believe that there is a larger issue behind the UFO phenomenon. I continue to be optimistic about science's ability to come to grips with unforeseen observations, paranormal phenomena and radical discontinuities in knowledge.

Witnesses are generous in bringing us some remarkable observations that are begging for an explanation. If the data challenge our view of reality, that is not the witnesses' fault. The burden is squarely on the shoulders of the scientists, to patiently sift through the occasional mistakes in perception that are undoubtedly reported, to eliminate the hoaxes and to document the gems of truly unexplained phenomena. We must do this responsibly, with respect and care for those who offer us their testimony, and with a realistic self-awareness of the limitations of our science. At a time when concepts of the physical universe are undergoing a major revolution, the UFO phenomenon is of unique value for theoretical development. It provides no solution, of course. Even if we were to recover bits of hardware and samples of alien flesh we might not be able to make sense out of them for centuries, but this should not surprise us.

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The history of science is filled with anecdotes about phenomena that were clearly recognized, but not applied for a long time. The ancient Egyptians, for example, knew of the magnetic properties of certain metals and their jewelry shows evidence of electroplating, yet they never developed an understanding of simple circuits. The eighteenth-century astronomer Messier observed and named the major nebulae in the northern sky, but it is only in our own century that they were finally recognized as galaxies outside our own. The same even goes for modern technology: the principle of radar had been known for fifty years before it was put into practical application at the end of the Second World War. The list is a very long one. For an anomalous observation to be incorporated into a new theory, many *concepts* have to mature until a match can be realized. We have not yet reached this point in the study of UFOs. This does not authorize us to throw the data away, or to disregard the phenomenon. On the contrary, carefully guided by the physical parameters from the best cases, research on alternative topologies for our own reality can already proceed.

In the seventies, French author Jacques Bergier, a keen observer of technological and cultural trends, once told me that we must revise the old notion of a single "universe." Perhaps the main lesson to be drawn from UFO sightings, he said, was that we are living instead in what he called a "Multiverse" with many more dimensions than we had suspected. He urged me to think about the numerous ways in which a conscious control system could operate in such a manifold.

Gifted science-fiction author Philip Dick explored a similar notion in a series of stunning stories. He called that superior entity VALIS, for "Vast Alive Living Intelligence System." It is at the level of multiple universes and control systems of consciousness that the UFO phenomenon becomes scientifically interesting, not at the simplistic level of a search for the "propulsion system" of unidentified flying objects. The technology we are witnessing may not be based on what we understand today as (propulsion).

Cosmology now recognizes the possibility, indeed the inevitability, of multiple universes with more than four dimensions. Communication and travel within our own universe are no longer thought to be absolutely constrained by the speed of light and a constant arrow of time. Even travel into the past may be considered without necessarily creating insurmountable paradoxes. This is a tremendously exciting develop-

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ment. It opens up vast new realms for theoretical and experimental endeavor.

If we look at the world from an informational point of view, and if we consider the many complex ways in which time and space may be structured, the old idea of space travel and interplanetary craft to which most technologists are still clinging appears not only obsolete but ludicrous. Indeed modern physics has already bypassed it, offering a very different interpretation of what an "extraterrestrial" system might look like.

As I look forward, my goal is to explore some hypotheses about the control system and the forms of communication it may favor. My plan is to quietly reconsider the accumulated data, to take a hard look at my own tentative conclusions and to challenge prevalent theories once again. The time has come to draw the hard lessons from our failure to elucidate the basic nature of the phenomenon. This means seeking advice from a wider circle of experts, reorganizing the work, eliminating a morass of obsolete data.

For some time various knowledgeable friends have urged me to take my research behind the scenes again. I intend to follow their advice. I cannot justify remaining associated with the field of ufology as it presents itself to the public today. Furthermore I suspect that the phenomenon displays a very different structure once you leave behind the parochial disputes that disfigure the debate, confusing the researchable issues that interest me. The truly important scientific questions are elsewhere.

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This extraordinary adventure has carried its share of sadness, because Janine and I have seen valued friends and colleagues pass away, some of them in tragedy.

One of the few prominent scientists who risked their reputation by studying ufology, Dr. James McDonald has been mentioned at length in these pages. Jim attempted suicide, only succeeding in blinding himself. But he eventually obtained another gun, shot himself again and died in 1971. His suicide was prompted by personal reasons. The UFO phenomenon played only an incidental role in his despair, but there is no question that the rejection of his earnest efforts by the scientific community had contributed to depressing him. Reflecting on our relationship from *today's* perspective I find it doubtful that I could have worked more

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*closely with* him, or that anyone could have *influenced his* thinking. While he had the highest integrity, his approach to science left no room for compromise and joint research. It made teamwork almost impossible.

Dr. Hynek himself died *on 27 April 1986* at his home in Scottsdale, of a brain tumor for which he had been operated on a few months before in San Francisco. We had been very close until the end, and I still miss him every day.

Coral Lorenzen died at sixty-three of respiratory failure, on 12 April 1988. Her husband Jim had fallen victim to cancer two years before, in August 1986. They had perhaps *come closest of all of us, in the sixties and seventies*, to assembling a complete documentation about the mystery. Their influence on the field is still felt. The organization they founded, the Aerial Phenomena Research Organization (APRO), had a sterling international reputation and worked successfully with foreign specialists.

Donald Keyhoe, the founder of NICAP, died in November 1988, the fourth major pioneer of the field to disappear in two years. He was over ninety at the time. His death brings up another painful point for me, because one of the mistakes I undoubtedly made in the sixties was my failure *to seek a meeting with him*. When I *read* Keyhoe's books today I find a ring of truth in them which I had missed in the sixties. I had allowed myself to be repelled too easily by the NICAP officials around him, who fancied complicated titles and seemed to specialize in creating bottlenecks. But Keyhoe himself appears to have known much about the phenomenon, and his insider's understanding of the military was a real asset.

In retrospect there were many other things I could have done during those years that never occurred to me. Primary among them would have been a thorough documentation of the Ruppelt years. I had relied on Allen Hynek's recollection of the early phase of Blue Book, but he himself had told me that Ruppelt had kept many things from him, that he "played his cards close to the vest." The Air Force officer had not trusted his consulting astronomer with all the data. This was an important gap I should have tried to fill, since the Ruppelt archives were accessible. The fact that no one else seems to have thought of it does not justify my omission.

Another important figure, author John Fuller, who popularized the concept of "missing time," died of lung cancer in November 1990, at

age seventy-six.

Gerard de Vaucouleurs, who gave me the great opportunity to come to the United States, now serves on the National Academy of Sciences. He has kept his open mind on the subject but his skepticism was still in evidence when he wrote to me after the publication of *Dimensions*, pointing out that the observations I reported merely showed that the human mind was susceptible to extraordinary distortions. In saying this he was summing up the current scientific consensus on the paranormal. Indeed men like Philip Klass and Carl Sagan continue to state publicly that studying UFOs is a waste of time, and that no resources should be channeled to that endeavor.

Fortunately many of those who had a decisive influence on my research in its formative years continue to favor me with their advice and friendship. Aime Michel and Fred Beckman have remained valuable friends. I still meet with Pierre Guerin whenever I travel to Paris. We sit in a cafe near the Sorbonne and we argue bitterly about everything from the Big Bang to government cover-ups. After a distinguished career in planetary astronomy, Guerin recently retired from the *Institut d'Astrophysique*, but he has lost none of his argumentative passion. Gordon Creighton, another towering figure with a lovably cantankerous character, has replaced Charles Bowen as editor of the *Flying Saucer Review*, surely one of the most colorful and informative magazines ever published. We correspond regularly, and our meetings are always challenging and fruitful.

Other close friends of that era have simply left the field. Don Hanlon, to whom I am indebted for many important facts and ideas, vanished without a trace in the mid-seventies. Others have gone on with their lives and careers. Bill Powers successfully published his seminal book *Behavior: the Control of Perception* and has been recognized for his work in psychology and in the theory of systems. Sam Randlett teaches music with great talent. Other members of our old Invisible College occasionally come to one another's notice through a published technical paper, a book review, or a lecture.

As for the flying saucers of old, they are still with us in their various forms and guises. Not a day goes by without some notice of a sighting somewhere. The details are seldom catalogued or documented. One of the most profound and puzzling phenomena in the history of man is allowed



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to exist around us without interference, without even a flicker of acknowledgement or an attempt at intelligent response.

Of that silence of mankind, of that refusal to recognize the unknown, I am still ashamed today. I can only hope that my testimony here may be a challenge to others and that eventually, collectively, we will find the strength to respond.

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It is the destiny of man to stand always between the certainty of his scientific achievements and the annoying evidence that they do not account for all there is. Other forces manifest. We are quick to give them convenient names and familiar roles. We call them ghosts, spirits, extraterrestrials. When all else fails we abjectly turn them into gods, the better to worship what we fail to grasp, the better to idolize what we are too lazy to analyze.

I am in search of a different truth.

I returned to Pontoise last year to look again at the hills of my childhood, to stand at my father's grave, to examine the steps I had taken when I began this research and to assess what I had learned. I came away with the certainty that, given the chance, I would take the same actions again today, because the only thing that counts in this life is to question the mystery of it, with all the means at our disposal, with every moment of awareness, with every breath.

# NOTES AND REFERENCES

## Part One: Sub-Space

1. This communication was published in *L'Astronomie*, the Bulletin of the French Astronomical Society, January 1958, pp. 8-9.

2. Michel, Aime, *Mysterieux Objets Celestes* (Paris: Arthaud, 1958). The text was translated into English by Alex Mebane as *Flying Saucers and the Straight-Line Mystery* (New York: Criterion, 1958). This book introduced the term *orthoteny* (Greek for "drawn in a straight line") to designate the fact that sightings appeared to fall along rectilinear patterns. It was a pioneering work because for the first time it proposed an analytical approach to UFO sightings rather than a value judgment about the witnesses.

3. The French title was *Le Sub Espace* (Paris: Hachette, 1961). It appeared in the book series entitled "*Le Rayon Fantastique*" under my pen name of Jerome Seriel. It received the Jules Verne Prize for that year. A mass paperback edition was later issued by Editions des Champs-Élysées. *Le Réseau Praxitèle* was never published.

4. The *Bourbaki* school of mathematics was born in the thirties when a group of young French graduate students decided to challenge the traditional approach to that discipline. They ridiculed their elders by hiring a destitute actor, dressing him as the bearded, solemn Russian visiting "expert" Professor Nicolas Bourbaki, and having him give a long, absurd lecture on advanced mathematics which was favorably received by the assembled Faculty at Ecole Normale Supérieure. When the students revealed their hoax the ensuing scandal was so great that the perpetrators had to leave France in order to pursue their careers elsewhere. From then on their articles, which revolutionized modern mathematics, were always published collectively in international journals under the single name Nicolas Bourbaki.

5. 1939, which saw the start of World War II, was a watershed year in many respects: it marked the founding of Silicon Valley with the first

Hewlett-Packard lab, the beginning of television broadcasting, of modern science fiction and the first color movie, *Gone with the Wind*. It was in 1939 that the first digital computer was built by Professor John V. Atanasoff of Ames, Iowa. The *Batman* comic script and the film *The Wizard of Oz* were also born that year. More significantly, it was on October 11, 1939, that a letter from Albert Einstein and Leo Szilard was delivered to President Roosevelt, announcing that atomic energy could be used to make bombs. At the time of my birth Sigmund Freud was dying in London.

6. There are several organizations calling themselves "Rosicrucian." The one mentioned here is the French Branch of AMORC, the *Antiquus Mysticusque Ordo Rosae Crucis* (Ancient and Mystical Order of the Rosy Cross) founded by Spencer Lewis, with international headquarters in San Jose, California.

7. *Dark Satellite (Le Satellite Sombre)* was published in November 1962 by Editions Denoel in their collection *Presence du Futur*, also under the pen name of Jerome Seriel. A mass paperback edition in Portuguese was later issued in Brazil.

8. Zazzo, Rene: *Le Devenir de l'Intelligence* (Paris: Presses Universitaires de France, 1945).

9. The *Ecole Polytechnique* was created in Paris by Emperor Napoleon to supply technical graduates in support of his military ambitions. It quickly became a power base for the elite of the French administrative, military and industrial world, nurturing a parochial view of most national problems. The existence of this antiquated mode of education was never challenged, even in the depth of the most "revolutionary" movements in France.

10. Camille Flammarion (1842-1925), founder of the French Astronomical Society, was an astronomer and an author eagerly read by the French public of the "Belle Epoque" for his clear descriptions of planetary and atmospheric phenomena. A keen observer of Mars, he discussed the possibility of life on other planets. He investigated paranormal research, publishing several books about near-death phenomena and ghosts.

11. We are talking in terms of "old" francs here, naturally. A sum of 100,000 francs in 1961 represented about \$200, or about one month of an engineer's salary. It later turned out that this "Prize" was in reality an advance on the royalties from the book!

12. Eleven million francs represented approximately \$20,000 in 1988 currency.

13. The National Investigations Committee on Aerial Phenomena (NICAP) was formed in Washington in October 1956 by former Navy scientist T. Townsend Brown. Major Donald Keyhoe, a retired Marines officer who wrote several very popular books about flying saucers in the fifties, became the Director in January 1957. Major Keyhoe died on 29 November 1988.

14. Project Blue Book was an official study of unidentified flying objects started in 1953 by the U.S. Air Force. It was based at Wright-Patterson Air Force Base, Dayton, Ohio. It functioned more as a public relations office than as a research project. It was to give American citizens a place to report sightings. It answered periodic Congressional inquiries about Air Force vigilance on the subject. It was generally headed up by a Major and had very limited resources. The project was disbanded in December 1969 following the Condon Report. Its files were transferred to Maxwell Air Force Base in Alabama.

15. To my knowledge, the policemen who had committed the atrocities at the Charonne subway station on Thursday 8 February 1962 were never brought before a Court to answer for their crimes. The dead included a 35-year-old mother of three children, Suzanne Matorell, killed by blows to the head given with a rifle butt. Many of the victims had been crushed to death, according to forensic pathologists. To squelch the media uproar about the killings, Interior Minister Roger Frey seized leftist newspapers the next day and issued a statement whitewashing the police.

## Part Two: Blue Book

1. The five short science-fiction stories I wrote in French during this period were all published in the monthly magazine *Fiction*. They were: *Les Calmars d'Andromède* (#94, Sep.1961), *L'Oeil du Sgal* (#107, Oct. 1962), *Les Planètes d'Aval* (#110, Jan. 1963), *Le Satellite Artificiel* (Special Anthology of French Science Fiction #4, 1963) and *Le Fabricant d'Evènements Inéluctables* (#145, Dec.1965).

2. About the Mars Map see the article "Charting the Martian Surface" by G. de Vaucouleurs in *Sky and Telescope*, Vol.XXX, No.4 (October 1965), notably p. 197.

3. Josef Allen Hynek was a first-generation American, the son of Czechoslovakian parents. Born in Chicago in 1910, he became fascinated with astronomy when his mother, a schoolteacher, gave him a book on the subject. He graduated in physics and astronomy at the University of Chicago in 1931, and completed his Ph.D. at Yerkes Observatory. When *Nova Herculis* flared up, Perkins Observatory in Ohio borrowed a spectrograph from Yerkes to study it, and Hynek went along to assist in the project. His work impressed the director of Perkins and he was offered a faculty position at Ohio State University in 1936. He specialized in the study of stellar spectra and in the identification of spectroscopic binaries. He met and married Mimi, his second wife, in Ohio. During their honeymoon they travelled to Washington and he visited a friend who was recruiting scientists for the war effort. He found himself signed to work on a classified project for the development of the radio proximity fuse.

After the war Hynek returned to McMillin Observatory in Ohio. He was contacted by the U.S. Air Force to act as scientific consultant on their investigation of unidentified flying objects. He retained his consulting position with them when he moved to Massachusetts to join Donald Menzel and Fred Whipple at Harvard's Smithsonian Observatory. In 1956 Hynek was in charge of the project to track the future American artificial satellite planned for the International Geophysical Year. When the Russian *Sputnik* was launched on October 4, 1957, he became one of the nations most "visible" scientists. He moved to Northwestern as chairman of the Astronomy Department in 1960.

4. *Special Report #14* was one of the most important documents in the series issued by the Air Force on the topic of unidentified flying objects. Based on work done in 1953 by one of its contractors, a prestigious Columbus, Ohio, "Think Tank" called the Battelle Memorial Institute, it contained the first serious statistical analysis of UFO sightings. The document was released by Secretary of the Air Force Donald Quarles on 25 October 1955.

5. The Air Technical Intelligence Center (ATIC), formerly the Intelligence Division of the Air Materiel Command, was the Air Force branch in charge of assessing threats to the United States deriving from new, potentially hostile technologies. It focused on Soviet rocketry and aircraft development and later artificial satellite recovery and analysis. It

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was also charged with screening UFO reports, beginning in 1947. Based at Wright Field in Dayton, Ohio, it supervised the work of the various projects, like Sign (formed in January 1948), Grudge (1948) and Blue Book (1953).

6. Hutin, Serge, *Voyages vers Ailleurs* (Paris: Arthème Fayard, 1962).

7. The "Robertson Panel" convened by the CIA from the 14th to the 17th of January 1953 in Washington under Air Force sponsorship was supposed to assess the relevance of the UFO problem to national security. Chaired by Dr. H. P. Robertson of the California Institute of Technology, a relativity expert and CIA consultant, its members were Dr. Samuel A. Goudsmit of Brookhaven, the discoverer of the electron spin; Dr. Luis Alvarez of the University of California at Berkeley, who was to receive the Nobel Prize in 1968; Dr. Thornton Page of Johns Hopkins, an operations research expert; and Dr. Lloyd Berkner, a director of the Brookhaven National Institute. In addition Dr. Hynek and Frederic C. Durant of Arthur D. Little attended selected portions of the meeting. Also present were Captain Ruppelt, Dewey Fournet of the Ethyl Corporation, General W M. Garland who was chief of ATIC, Navy photo analysts R.S. Neasham and Harry Woo, and CIA personnel Dr. H. Marshall Chadwell (Assistant Director for Scientific Intelligence), his deputy Ralph L. Clark, and Philip G. Strong, Chief of Operations staff. Also from CIA were Lt. Colonel Frederick Oder (Physics and Electronics Division) and David Stevenson, Weapons and Equipment Division.

8. *Levelland*: On 2 November 1957 near Levelland, Texas, five groups of witnesses independently saw luminous cigar-shaped objects hovering near the highways, interfering with car engines and headlights. *Loch Raven*: On 26 October 1958 two men saw a glowing oval object over a bridge at Loch Raven Dam, Maryland. The Air Force concluded they were not lying, found four groups of independent witnesses and carried the report as unidentified.

9. Several articles appeared under my name in the *Flying Saucer Review* in this period: "Towards a generalization of orthoteny" in March 1962; "Mars and the Flying Saucers" (with Janine) in September 1962; "How to codify and classify saucer sightings" in September 1963; "Recent developments in Orthotenic Research" in November 1963; "A descriptive study of the entities associated with Type-1 sightings" in January and May 1964; "The Menzel-Michel Controversy" in July 1964; "How to

Select Significant UFO Reports" in September 1965; and "UFO Research in the USA" in November 1965 and January 1966.

10. Among General De Gaulle's schemes to enhance Frances independence was the *Force de Frappe*, an ambitious plan to develop an independent nuclear capability using the South Pacific as a testing base. He also encouraged the development of nuclear power plants and the Concorde aircraft.

11. *Project Sign* was the first Air Force project designed to deal with the reports of UFOs by the American public following the wave of sightings recorded in 1947. Formed in January 1948, it carried a 2A restricted classification and issued a report in February 1949. By then the name of the project had been changed to *Grudge*. The files remained closed to the public and to scientists.

12. The Aerial Phenomena Research Organization (APRO) was the oldest continuing UFO research group in the United States. It was formed in Wisconsin by Coral Lorenzen in 1952 and continued through 1986. Coral Lorenzen died on 12 April 1988 in Tucson, Arizona, where the organization had moved in the sixties.

13. *Le Parisien Libéré*, 22 May 1952.

14. Dr. Carl G. Jung, the well-known Swiss psychiatrist, wrote a book on the UFO question entitled *Flying Saucers—A Modern Myth of Things Seen In The Skies*. Wilbert Smith was a Canadian government engineer. Captain Edward J. Ruppelt was the head of UFO investigations for the Air Force from 1951 to 1953. He died in 1960.

15. The participants were: Major Robert J. Friend, ATIC; Arthur C. Lundahl, CIA, Chairman; Commander Julius M. Larsen, ONI; Lt-Commander D. W. Luiber, CIA; Lt-Commander R. S. Neosham, CIA; Mr. C. F. Camp, CIA; Mr. H. F. Schemfele, CIA; Mr. J. W. Cain, CIA; Mr. W. S. Stahlings, CIA. Colonel Friend retired as head of Blue Book in 1963.

16. On 17 May 1965 Lt-Col. Spaulding wrote to Lt-Col. Carl C. Arnold, director of information, 3AF, to inquire about British UFO activity and to ask "if they had a program comparable to Blue Book' and if so, "do they have a scientific consultant," adding that Dr. Hynek "would like to correspond with him on a personal basis."

17. These observations were cut from the NBC documentary but both cases were described in detail in *Challenge to Science*. The sighting at

Vins was the basis for a celebrated scene in the movie *Close Encounters of the Third Kind*, where metallic objects vibrate wildly while a saucer hovers nearby.

18. Lt. Colonel John F. Spaulding had his office in the Pentagon and was responsible for overseeing Project Blue Book. His title was Chief, Civil Branch, Community Relations Division, Office of Information.

19. Ivan T. Sanderson was a gifted biologist and naturalist, widely travelled, who wrote several books on the fauna of Africa and the Caribbean (notably *Caribbean Treasure* [New York: Viking, 1939]). He later became fascinated with UFOs.

20. Other members were Jesse Orlansky, Launor Carter, Richard Porter and Willis Ware.

21. The paper with Hynek appeared in the August 1966 issue of the *Publications of the Astronomical Society of the Pacific* (PASP) under the title "An Automatic Question-Answering System for Stellar Astronomy," while the paper with Krulee appeared later in the *Information Storage and Retrieval Journal* (No.4, p. 13, 1968) under the title "Retrieval Formulae for Inquiry Systems."

22. The article with John Welch was entitled "Respiratory Mechanics following Major Surgery." It was printed in the *Proceedings of the Sixth International Conference on Medical Electronics*, p. 497 (1965).

### Part Three: Pentacle

1. Realizing at last that computers were now vital to a country's national security and status in the world, the General placed a high priority on internal French computer development when he drafted the Plan Calcul. His ambitious plan floundered under the greed of the industrialists at semi-public companies Bull, CGE and Thomson, who rushed to obtain subsidies under the plan and squandered millions of dollars on worthless projects, destroying in the process most of the small independent computer firms that were beginning to appear in France. Bull emerged with the biggest piece of the pie, and proceeded to impose its awkward "French computers" based on manufacturing licenses from the U.S. Honeywell corporation. No genuine French computer approximating the performance of the CDC 6600 was ever developed.

2. Bill Powers eventually published his theory as a book entitled *Behavior—the Control of Perception* (Chicago: Aldine, 1973).



3. The budget for the project was eventually expanded to \$513,000.

4. Max Heindel, Manly Hall and Rudolf Steiner represent three major currents in twentieth-century esoteric scholarship. Heindel created a Rosicrucian movement that was heavily oriented towards spiritism and astrology. His influence throughout Latin America has remained considerable. Hynek owned nearly all of his books. Manly Hall is the founder and director of the Philosophical Research Society in Los Angeles. Among his many works is an amazing compilation entitled *Secret Teachings of All Ages*. Rudolf Steiner, whose influence on Hynek was the deepest, thought and spoke as a scientist. Born near Vienna, Austria, in 1861, he first joined the theosophical movement of Helena Blavatsky, then went on to start his own movement called *Anthroposophy*. The Nazis set fire to his headquarters building, the beautiful Goetheanum, on 31 December 1922. Steiner died on 30 March 1925, leaving many books and an active network of disciples all over the world.

5. *Le Comte de Gabalis*, by the Marquis Montfaucon de Vilars, subtitled *Entretiens sur les Sciences Secretes* (Paris: Claude Barbin, 1652), has long been a classic among occult books because it describes various orders of paranormal beings and man's relationship to them.

6. Jean Baudot is also the author of *La Machine a Ecrire* (Montreal: Editions du Jour, 1964), the first volume of poems written by a computer. The machine composed some pearls of wisdom that would have enchanted Jean Cocteau, such as "The plaintive coffins no longer furnish interesting tears," or "Wealth and a pleasure filled with joy waste away sadness together with its roots."

7. This article, entitled "Theorie des Systemes Autocodeurs," appeared in *Revue d'Informatique et de Recherche Operationnelle* (RIRO) No. 3, pp. 63-70, 1967.

8. This was written at a time when practically all computer work took place in batch mode, meaning that programs punched in the form of decks of cards were read into the computer and run by an operator, who later separated the output listings for each user to pick up. The programmer had nothing to do until his output came back. Typical turn-around time was 24 hours, although we were often able to get two runs a day at Northwestern. There was no such thing as an interactive program, and time-sharing only existed as an experiment in a few computer laboratories.

## NOTES AND REFERENCES

9. Allen Newell and Herbert Simon were among the founders of Artificial Intelligence in the United States. Their work, which was mainly conducted at Carnegie-Mellon University in Pittsburgh, is summarized (among other places) in *The Sciences of the Artificial*, by H. Simon (Cambridge: MIT Press, 1969).

10. The Tunguska explosion took place in Central Siberia shortly after 7 a.m. on 30 June 1908. This thirty-megaton blast, which devastated an area of hundreds of square miles of pine forest, is still unexplained. Various theories have been proposed, including the idea that the exploding object might have been a giant meteor or the nucleus of a comet, but they fail to explain the data since no fragments were recovered and no one had observed an approaching comet, which presumably would have been a spectacular sight for weeks before the collision. Alexander Kazantsev speculated that the object may have been a spaceship propelled by anti-matter.

11. M.K. Jessup, trained in astronomy, later served as a photographer with a Department of Agriculture expedition to the rubber plantations of the Amazon. Deeply fascinated by the UFO mystery, he wrote four books on the subject before committing suicide in April 1959.

12. The prospector who was the witness in the Falcon Lake case was named Steven Michalak. A 51-year-old mechanic, he was looking for minerals when he suddenly observed two glowing red objects, one of which landed. When he approached and touched it he received severe burns and a scorching pain on his chest while the disk took off.

13. Wilbert Brockhouse Smith was a Canadian radio engineer, born in Lethbridge, Alberta, in 1910. He held a Masters degree from the University of British Columbia. In 1939 he joined the Department of Transport. He engineered Canada's wartime monitoring service. At the end of the war he established a network of ionospheric measuring stations. In 1952 he became a member of a special government group to investigate UFO Phenomena, known as Project Second Storey. He lost credibility with his colleagues when he began claiming that he was in communication with UFO occupants. Smith died in 1962.

14. Journalist John Fuller published many non-fiction books including *Gentlemen Conspirators* and *The Money Changers*. From 1957 to 1966 he wrote the well-known *Trade Winds* column for the *Saturday Review*. Before researching the Barney and Betty Hill story he had published

*Incident at Exeter*, a study of UFO sightings in New Hampshire.

15. In spite of repeated requests on the part of my attorneys under the Freedom of Information Act during 1989, the Air Force has proven unable to locate this document or to tell me whether or not it has been automatically declassified. The chief of the Records Section at Wright-Patterson Air Force Base wrote back on 19 July 1989 that "As of the current date most records with a date of 1953 and earlier would probably be destroyed or retired to the appropriate Federal Records Center." We were not able to find out what the appropriate Center was for Project Stork.

On 22 August George Conner of the Records Management group at Wright-Patterson forwarded our request to the Foreign Technology Division (FTD/IMD) for processing. There the chief of the Freedom of Information Branch, Sergeant Tammy K. McDonough, indicated to us that their files included no information on Project Grudge or Sign, and that the codename used for the research on unidentified flying objects was Blue Book: "In 1970 all Project Blue Book material was forwarded to the Albert F. Simpson Research Center, Maxwell AFB, Alabama, for storage. In 1976, because of the historical value, all UFO material was assessioned (sic) by the National Archives. We possess no further information on Project Blue Book or Unidentified Flying Objects." Sergeant McDonough provided an address at the National Archives.

On 31 August 1989 we redirected our request to the National Archives, which has not located the relevant papers at this point. Accordingly I have decided to err on the side of safety by refraining from publishing the names of the authors and by deleting some passages. Perhaps other researchers will be able to obtain the release of the information in full.

16. Frank Edwards (1908-1967) was a popular radio commentator and author who often attacked on the air what he saw as the official debunking of the UFO question.

17. Hynek conducted a covert study of astronomical opinion while attending the joint meeting of the American Astronomical Society and the Astronomical Society of the Pacific in Victoria, British Columbia, approaching 44 astronomers between June 23 and July 7, 1952. The attitude ranged from "I would not say anything about it if I did see one" to a definite, sympathetic interest in the problem. The astronomers interviewed were not aware that anything more than a personal private talk

between scientists was involved. Five out of forty-four had seen a UFO. Among these were Otto Struve and Adel, who had taken five bearings on a passing object they saw in 1950 in Flagstaff, Arizona. La Paz described seeing his famous green fireballs, Clyde Tombaugh said that he and his wife had once seen an unusual light they could not identify, and Desbins said he had seen two lights that were "too slow for a meteor and too fast for an aircraft."

18. American journalist John Keel published *Jadoo*, an interesting book about the Near East and his experiences among the Yezidis before becoming interested in UFOs. He wrote several books (*The Eighth Tower*, *Operation Trojan Horse*) and many articles about UFOs.

19. Hynek's letter was finally published in *Science* magazine, October 21, 1966, p. 329.

20. See Eisenbud, *Jule: The World of Ted Serios* (New York: William Morrow, 1967).

21. Dr. Bill Olle was one of the first computer scientists to propose the notion of "non-procedural languages." This concept was especially important in the retrieval of files and the manipulation of large data-bases, since it saved the long and tedious process of writing and debugging a special program for every interrogation of the files. Infol was the first of many systems which are now called DBMS, or "database management systems."

22. "What is Flying in our Skies?" appeared first in *Tekhnika Molodezhi* (*Young Technology magazine*) No. 8, Moscow, August 1967. It was reprinted in *Trud* on 24 August 1967.

#### **Part Four: Magonia**

1. This article by William Markowitz appeared in *Science*, Vol. 157, pp. 1274-1279, 15 September 1967. It was entitled *The Physics and Metaphysics of Unidentified Flying Objects*, with this absurd banner, "Reported UFOs cannot be under extraterrestrial control if the laws of physics are valid."

2. Institut de Recherche en Informatique et Automatique (IRIA) has since grown into a "National" Institute and is now known as INRIA. One of its notable members is Professor Ichbiah, who developed the Ada language.

3. In fact I published a number of articles in *Flying Saucer Review*

during this period. Following "A ten-point research proposal" which appeared in September 1966, "The Pattern behind UFO Landings" in October 1966, "Airships over Texas" (with Don Hanlon) in January 1967, and "A Survey of French UFO Groups" in September 1967, I wrote several pieces while in France, notably: "Analysis of 8,260 sightings" which appeared in May 1968 and "A Catalogue of 923 landing reports" in July 1969. In addition I wrote an article entitled "The UFO Phenomenon: A Scientific Problem" for publication in *UFOs Around the World* in September 1966.

4. Hynek's article in *Playboy* appeared in the December 1967 issue under the title: *The UFO Gap* (p. 143).

5. Andrew Tomas was working at the time on the manuscript of *The Time Barrier*, which was published as *La barrière du Temps* (Paris: **Juliard**, 1969). He also published *Sur le Rivage des Mondes Infinis* (Paris: **Albin Michel** and London: Souvenir Press, 1974, under the title *On the Shores of Endless Worlds*).

6. *Planète* #2. Paris: Editions Retz, Dec. 1961. "La Sociologie," p. 133.

7. The "Service de Documentation Exterieur et de Contre-Espionnage" (SDECE) was the major French secret service. It has been reorganized several times since 1968 and is now called "Direction Générale de la Securite Exterieur" (DGSE).

8. General Ailleret's four-engine DC-6 military aircraft operated by the GLAM (the French Cabinet's air transportation group) crashed as it was leaving the island of La Reunion, in the Indian Ocean. Also killed in the crash were his wife, daughter and fifteen staff members. There was one survivor, Michèle Renard, a nurse. The airplane turned inland and hit a mountain five km away from Saint-Denis airfield instead of banking towards the sea after taking off. Rumors variously attributed the accident to pilot error, to bad weather or to sabotage. General Ailleret had served as commander of "Special Weapons" from 1956 to 1960. In that capacity he supervised the work leading to the first two French A-bomb tests, working with Professor Rocard.

9. Michel and Francoise Gauquelin were French psychologists trained in statistics. As an experiment they once "disproved" the alleged correlation between the Zodiac and human destiny. However, Michel Gauquelin also noticed an unexpected effect linking the position of certain planets above the horizon at the time of birth with the careers pursued by ~~remark-~~

## NOTES AND REFERENCES

able individuals. For instance, there is a significant tendency for champion athletes to have been born at the time of either the rise or the upper culmination of Mars. Among the Gauquelins' books published in the U.S. are *The Cosmic Clocks* (Chicago: Henry Regnery, 1967) and *Cosmic Influences on Human Behavior* (NY: Stein & Day, 1973) with a Foreword by Dr. Hynek. More recently Michel Gauquelin (who divorced Françoise) published an article entitled "Is there a Mars effect?" in the *Journal of Scientific Exploration*, Vol.2, No.1, pp. 29-51, 1988. He died on 20 May 1991.

10. A member of the French Committee for National Defense, Professor Yves Rocard was director of the physics laboratory at Ecole Normale Supérieure from 1945 to 1973. He wrote several textbooks on physics and was regarded as an authority on mechanical vibrations and instability, working notably on the Tancarville bridge near Le Havre and on the suspension system for Citroen cars. From 1944 to 1951 he was head of the French Navy research services, and he remained a scientific consultant for them throughout his career.

11. The sighting by Father Gill in Papua New Guinea on 26 June 1959 has remained a classic of UFO literature. Father Gill was the head of the Anglican mission at Boianai. Together with dozens of witnesses, he saw an orange object hovering over the sea at 6:45 pm. Four occupants were visible on its "deck" and a beam of blue light was emitted upward from it. When the witnesses waved, one of the occupants was seen to wave back.

12. This think tank swore that the CDC6600 computer would not be used to compute an H-bomb, a pledge that the Americans presumably took with a grain of salt.

13. Marcel Granger, *Adieu a Machonville*, published in *Minute*, 26 September 1968.

14. The *Barbouzes*, in French slang, are "the bearded ones," the agents of the multiple, shadowy intelligence organizations that operate under the umbrella of the French government, often in competition with each other.

15. Jacques Bergier, French nuclear chemist and prolific writer, is best-known as co-author of *The Morning of the Magicians* with Louis Pauwels. He also wrote several books on modern espionage and was a co-founder of *Planète*, a monthly magazine similar in contents and ori-

entation to the later *Omni*. A man of seemingly universal interest, he avidly read scientific and science-fiction magazines in several languages, notably Russian which he spoke fluently. He had access to De Gaulle in matters of French national security, especially when they involved high technology.

16. Stanislas de Guaita assumed the leadership of the French Rosicrucian movement in 1887, at the age of twenty-four, when he founded the *Ordre Kabbalistique de la Rose+Croix*. The descendant of a line of Florentine aristocrats, the Marquis de Guaita had gained a reputation as a minor poet when he first arrived in Paris. In 1884, when he read Peladan's *Vice Suprême*, he began his own esoteric research. In his luxurious apartment of 20 Avenue Trudaine he accumulated a magnificent library with the help of high-ranking Mason Oswald Wirth. His major work is his three-volume *Essais de Sciences Maudites*. Maurice Barres, who was a childhood friend of Guaita, wrote that he occasionally fought against "larvae" by firing his revolver at them, and spent weeks in his library making heavy use of morphine and hashish while performing his magical experiments. Legend has it that he was strangled by a "flying spirit" in 1897.

17. American film-maker Kenneth Anger directed *Scorpio Rising*, *Invocation of My Demon Brother*, *Lucifer Rising* and *The Inauguration of the Pleasure Dome*, among other works inspired by the magical theories of Aleister Crowley. He also authored a two-volume expose of the steamy underside of the movie business, *Hollywood Babylon*.

18. The "assembly code" of a particular computer is a language in which one statement generally corresponds to one machine instruction. Writing in that language gives systems programmers greatest power because it makes available every feature of the computer. At the same time, however, it is long, cumbersome and error-prone, hence very expensive. In contrast the so-called "higher-level languages" like Fortran and Cobol are more general languages, closer to English and easier to use. They mask the true power of the machine and cannot be used to develop the basic tools like compilers and the operating systems that manage the flow of "jobs" through the computer. The "Implementation Language-1" of RCA was a brilliant attempt to give programmers a language that was both elegant and powerful, but it was never released for marketing reasons. A year or so after I left the company RCA decided to get out of the computer business altogether.

## NOTES AND REFERENCES

19. A few years later the term "software" was translated into French as "*logiciel*," a word that has become standard.

20. Jung, Carl Gustav: *Alchemical Studies*. Princeton University Press, Bollingen Series XX, 1967, pp. 15-16.

21. The two photographs in question were published by Allen Hynek in his book *The UFO Experience*, Chicago: Henry Regnery, 1972, facing page 53.



Unless you make yourself equal to God, you cannot understand God; like is understood by like. Make yourself grow to immeasurable immensity, outstrip all body, outstrip all time, become eternity and you will understand God. Having conceived that nothing is impossible to you, consider yourself immortal and able to understand everything, all art, all learning, the temper of every living thing. Go higher than every height and lower than every depth. Collect in yourself all the sensations of what has been made, of fire and water, dry and wet; be everywhere at once, in land, in the sea, in heaven; be not yet born, be in the womb, be young, old, dead, beyond death. And when you have understood all these at once – times, places, things, qualities,

quantities – then you can understand God.

‘Mind to Hermes’ (*Corpus Hermeticum Treatise XI*)

Translated by Brian P. Copenhaver

# **-SATANS CREED**

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# INTRODUCTION

In September 2010 the London *Times* carried the banner headline ‘Hawking: God did not create Universe’, conveying a sense of finality, as if one man – no matter how distinguished – had finally answered arguably the greatest question of all time. In fact, to us the most remarkable thing about this was that Britain’s leading broadsheet thought this topic worthy of their front page. Although it was publishing extracts from his latest book, *The Grand Design*, the readiness with which *The Times* accorded Hawking not only the headline, but also a lengthy article and most of the accompanying magazine, shows just how big the debate between religion and science has become.

An even more strident anti-God voice is, of course, that of Richard Dawkins, the British evolutionist and crusading atheist, whose *The God Delusion* (2006) polarized the controversy and gave rise to a flurry of books either attacking him or turning him into a demi-god in his own right. This even led to the bizarre sight of London's big red buses carrying posters that declared, 'There's probably no God. Now stop worrying and enjoy your life', followed swiftly by the other side's call to arms, 'There definitely is a God. So join the Christian Party and enjoy your life'. Seeing these buses sail past in the capital of arguably the most secular country in the West was indeed a curious sight. The controversy has become so cool that it has even found its way into the routines of the edgier comics such as Eddie Izzard and Ricky Gervais, both of whom are vociferously and colourfully atheist.

The debate is by no means simply confined to personal belief or philosophical interest. Religion is now also a hot topic for politicians and social workers, as the gulf widens between the secular and religious mindsets. It seems that virtually every day the media carries some manifestation of this tension, from the French ban on the wearing of the Muslim *burqa* to the fundamentalism that fuels the War on Terror.

When the argument about the existence of God is framed, as it usually is, in terms of dogmatic organized religion, the Dawkins' school seems to be well ahead. When he is arguing with a Christian fundamentalist or a fervent Catholic it is hard not to agree with him. But when he extends his reasoning to anything that touches on the mystical, magical or transcendental, that is where we part company.

There are several major problems with the position advocated by Dawkins and his even



more vociferous fellow atheist Christopher Hitchens, author of *God is Not Great* (2007). The first is that, taking advocacy of rationalism and science to its logical conclusion runs the risk of scientism – science as an ideology instead of an objective method for evaluating and improving the natural world. This would create a society in which every aspect of life – not just technology, medicine and so on – is assessed and governed by science. However, as very few people have either the time or the inclination to keep up to date with cutting-edge science, they would have to take the pronouncements of scientists on trust – or faith. Which is exactly how priests rose to power, by claiming an exclusive insight into God's laws beyond the reach of ordinary folk. We would be back where we started; scientists would be the new priesthood, and scientism would have become the new religion.

More importantly, it seems to us that a sweeping dismissal of anything remotely spiritual or mystical actually ignores a major part of what it is to be human. The Dawkins/Hitchens school fails to distinguish between, on the one hand, the religious *impulse* that is innate to human beings and, on the other, the systems of authority and control that the organized religions have become.

The debate is almost always portrayed with just two alternatives, scientific atheism and organized, dogmatic religion. But something is missing: the profound sense of the 'Other', or the transcendental – what may be termed the mystical, or even magical – that underpins, but is not the same as, religious sensibilities. And, as this book hopes to demonstrate, this is by no means incompatible with a truly scientific worldview.

There has never been a culture – from rainforest tribes to the greatest civilizations such as Rome, ancient Egypt or even the

modern West – which did not begin with an understanding of the world based on a belief that it is both purposeful and meaningful, arising from a supernatural ordering of things. It, and everything in it, are here for a reason. This way of looking at the world around us is not learned, but instinctive; it comes naturally to the individual. And this yearning for the transcendental is not rooted in organized religions; they and their priest-hoods might exploit this innate impulse, but they did not create it.

Ours is the first civilization where a significant number of people have attempted to break away from such a worldview. But, as Richard Dawkins laments, it is a slow and difficult struggle, precisely because such thinking is second nature to our species. It is so universal, so taken for granted, that it seems to be hardwired into us.

Indeed, while we were writing this book, new evidence emerged, in the work of

developmental psychologist Professor Bruce Hood of Bristol University, who concluded at the 2009 meeting of the British Science Association that ‘superstition is hardwired’, being there from the beginning:

Our research shows children have a natural, intuitive way of reasoning that leads them to all kinds of supernatural beliefs about how the world works. As they grow up they overlay these beliefs with more rational approaches but the tendency to illogical supernatural beliefs remains as religion.<sup>1</sup>

Hood demonstrated just how hard that wiring is. For example, his study of a group of staunch atheists revealed that even they found the idea of receiving an organ transplant from a murderer utterly abhorrent – a completely irrational reaction. Another researcher, American anthropologist Pascal Boyer, concludes:

Religious thinking seems to be the path of least resistance for our cognitive systems. By contrast, disbelief is generally the work of deliberate, effortful work against our natural cognitive dispositions.<sup>2</sup>

Hood and Boyer are not talking about deeply mystical and religious feelings but something much more common. Yet, while recognizing how fundamental magical thinking is to human beings, they fail to explain the big question of *why* this should be so.

Similarly, the Dawkins school pays little attention to this mysterious human propensity for a belief in the supernatural and the magical. As it is the very antithesis of scientific, rational thought they don't even give it a nod. But this is dodging a major question. Even if it is all just superstition, surely investigating such a basic instinct with an open – a truly scientific – mind would reveal something important about humanity? If, as Dawkins insists, God is a delusion, why should we be *programmed* to be quite so delusional?

As a specialist in the genetic basis of human and animal behaviour, Dawkins has attempted to explain the ubiquity of religion as

a by-product of a useful evolutionary trait, suggesting that human beings have evolved an instinct to obey the commands of elders because, as children, we need to do so to remain safe in a dangerous world. We are programmed to believe what we are told by those we look up to in authority. However, as this instinct remains into adulthood we stay susceptible to the pronouncements of authority, and so priests effectively become our surrogate parents, our holy fathers.<sup>3</sup>

Although this makes some sense, it disingenuously addresses only one aspect of religion: why human societies almost always develop religious institutions and priesthoods – the *exploitation* of magical thinking, not the reason it exists in the first place. Dawkins' scenario would work equally well without religion – if people are programmed to accept authority, then kings and dictators would do just as well, without an appeal to a higher but invisible being.

Science has yet to provide an answer to the basic question of why humans are hardwired to believe. And it is an exquisite irony that one of the products of this magical mindset was science itself. It is, as we will see, what motivated all of the great pioneers of the scientific revolution.

As readers of our previous books will realize, anything that is forbidden has an instant appeal to us. So the discovery that there is a forbidden science was just too tantalizing to ignore. Its focus is an ancient mystical and cosmological system that has always clamoured for our attention, from our first research into Leonardo da Vinci and the Turin Shroud, and our discoveries about the heresy that upholds John the Baptist as the true Christ, which we explore in *The Templar Revelation* (1997) and *The Masks of Christ* (2008). Lynn's *Secret History of Lucifer* (2006), which explores forbidden paths to

both mystical and scientific enlightenment, also lit the way to this book.

As we hope to demonstrate, the greatest inspiration of luminaries such as Copernicus and Isaac Newton was almost lost over the centuries. Although the usual explanation for this decline is that scientists simply became too mechanistic – Dawkins would say too sophisticated and intelligent – to think in transcendental terms, we argue that this is not the case, and that there was another reason entirely ... In fact, this venerable philosophy has much to reveal not only about the origins of science but, we contend, is also increasingly relevant for today's scientists.

This extraordinary tradition is set out in a collection of texts that have had the greatest impact on western culture of any book apart from the Bible, and the greatest impact on the modern world than any book *including* the Bible. Surely that in itself is a major



reason for rediscovering these ancient secrets. And the best part is that they are not merely ancient, not just some historical curiosity – they even have something important to teach science of the twenty-first century.

Lynn Picknett

Clive Prince

London, 2010

## Introduction

[1](#) Quoted in Leake and Sniderman.

[2](#) Quoted in *ibid.*

[3](#) Dawkins, *The God Delusion*, pp. 200–8.

# PART ONE

# The Occult Roots of Science

## CHAPTER ONE

# COPERNICUS AND THE SECOND GOD

There are three key events that science historians cite as landmarks in the long journey from superstition to intellectual enlightenment: Copernicus' proposal of the heliocentric theory (1543), the prosecution of Galileo by the Church for promoting that theory as fact (1633) and the publication of Isaac Newton's *Principia Mathematica* (1687), which established key physical laws, primarily those of motion and gravity. As a leading historian of science put it: 'The series of developments starting with Copernicus in 1543 and ending with Newton in 1687 maybe be labelled the Scientific Revolution.'<sup>1</sup> However,

these great leaps forward were not made because Copernicus, Galileo and Newton elevated pure reason above religious irrationality, but because they were all inspired by the same unashamedly metaphysical and magic-oriented philosophy – one that also excited and motivated other great minds of the time, including our own special hero Leonardo da Vinci.

To today's materialist-rationalists, the unpalatable fact is that a magical mindset not only bubbled along through the Renaissance, but it was magic that inspired and drove the whole of that era's explosion of thought and achievement. In a very real way, magic made the modern world.

The event that is considered *the* watershed moment, the beginning of the parting of the ways of science and religion, is the proposal of the heliocentric, or 'sun-centred' theory of the cosmos, which posited that the Earth circles around the sun and not, as had been

thought, the other way around. The radical new notion was proposed by Nicolaus Copernicus (1473–1543) as the Polish canon Mikolaj Kopernik styled himself in the manner of contemporary scholars.

Until then astronomy and its esoteric twin astrology had traditionally been based on the belief that the Earth was at the centre of the universe. It was a natural assumption, since the sun, moon and stars appear to move around us in regular cycles, while the world we stand on seems to be static. The only complication with this system was presented by the movement of the five planets visible to the naked eye, which despite demonstrating a pattern, did not appear simply to circle the Earth. In the second century CE the Greek-Egyptian astronomer and mathematician Claudius Ptolemaeus, who is known as Ptolemy, devised an Earth-centred model with a complex system of cycles and epicycles to account for the movements of the

planets. He was the single great astronomical authority until Copernicus took centre stage.

Strangely, for such a monumentally influential figure, very little is known about Copernicus the man, although the outline of his life is well documented. He was born in Torun in Poland in 1473 to a copper merchant, hence the name. His father died when Copernicus was young, leaving an uncle, who was a canon, to raise him. After studying church law, he extended his stay in the stimulating environment of Renaissance Italy by studying law and medicine at Padua in the Republic of Venice. A gifted artist and draughtsman, his real passion was astronomy, to which he devoted much of his free time.

When his uncle became a bishop, he secured Copernicus a job as a church administrator, or canon, in the town of Frombork. He lived out the rest of his life, based in a tower – now known as Copernicus' Tower –



in the courtyard of the cathedral. His remains were only discovered under the cathedral as recently as 2000. As an ordained clergyman Copernicus was forbidden to marry, but it seems he may not have been totally celibate, according to rumours linking him to his housekeeper. This did not go down well with the Church authorities.

His duties gave him enough leisure time for his passion for astronomy, which he indulged in his tower. Like many astronomers at the time, Copernicus was dissatisfied with the fixes and fudges that were needed to make Ptolemy's system work, and so set out to address the problem. But unlike the vast majority, the results Copernicus achieved would change astronomy for ever.

Copernicus developed his radical new theory in the first decade of the sixteenth century, but refrained from going public for many years, contenting himself instead with scholarly discussions and penning an

account for private circulation in the early 1510s. He only published what he termed his 'new and marvellous hypothesis', *On the Revolutions of the Celestial Spheres* (*De revolutionibus orbium coelestium*) at the end of his life – the last page proofs were delivered to him on his deathbed in 1543. The popular science writer Paul Davies calls the book 'perhaps the very birth of science itself'.<sup>2</sup>

Contrary to common belief, Copernicus did not delay publication until death made him safe from the Vatican's wrath. He was only reticent about going public because of the academic controversy his theory would generate, and only agreed to write his book under pressure from colleagues who were excited by his theory. Even Pope Paul III had listened enthusiastically to a lecture on the subject given by his secretary, the German scholar Johann Widmannstetter, ten years before *On the Revolutions* was published. A

cardinal who attended the lecture, the Archbishop of Capua, was one of those who urged Copernicus to write and publish his theory. So much for today's perception of the Church's hostility.

*On the Revolutions* put forward three new controversial ideas: That the Earth moves in space, revolves on its own axis and that it and the other planets circle the sun. Copernicus pointed out flaws in the old Ptolemaic system and set out the observations that led him to propose a new model of the universe. On the thirty-first page he reveals his groundbreaking, even shocking, proposition in the form of a diagram that shows the planets, in their correct order, circling the sun. And just four lines beneath the all-important diagram he makes an extraordinary statement:

Accordingly [considering the sun's central position], it is not foolish that it has been called the lamp of the universe, or its mind, or its ruler. [It is] Trismegistus' visible God ...

Copernicus was linking the sun's physical place in the solar system to resolutely transcendental concepts: that the sun is the universe's 'mind' or the seat of the power that rules all creation, or 'Trismegistus' visible God'. And it is in those three words that the greatest clue to understanding Copernicus' theory lies, for they reveal a hint of the *real* heresy that was to rock the Vatican to its foundations.

# MAN THE MIRACLE

To discover why Copernicus' reference was – and in certain respects still is – so earth-shaking we have to look back at another seminal document, published over half a century earlier, which cited the same mysterious authority.

Here was a tract that many have called the manifesto of the Renaissance,<sup>4</sup> as it crystallizes and embodies the spirit and purpose of that new era. Published in Rome in 1487, it has become known as the *Oration on the dignity of man* (*De hominis dignitate*). Intended to be given as a public lecture, but never delivered, it was written by the twenty-four-year-old Giovanni Pico della Mirandola (1463–94). As the youngest son of the ruler of the city-state of Mirandola in northern Italy, and Prince of Concord, Pico's name

was already known. Although his family may have been only B-list nobility it was related by marriage to illustrious dynasties such as the Sforzas of Milan and the Estes of Ferrara. Pico had inherited influence, which he was happy to exploit.

When he arrived in Rome from Florence, after attending various universities, including Paris, Pico had with him a set of nine hundred theses – statements from various philosophical, mystical and esoteric traditions – which, he claimed, were mutually consistent and reconcilable. He said he would demonstrate this in a public debate before Rome's intelligentsia. But as the majority of his sources were not Christian, his request for a public debate was refused and his work condemned. This was Rome, after all.

Pico was, however, not to be dismissed so easily. With astonishing courage and foolhardiness (a combination that distinguishes

many Renaissance heroes), he published an *Apology* – in fact, a defence – which included his nine hundred theses and what would have been his opening speech in the debate, the *Oration on the Dignity of Man*.

As his chosen title suggests, Pico's fundamental point concerned the brilliance of humankind and its privileged place in creation. To him, a human being's defining faculty is his intellect, the hunger for knowledge and the ability to satisfy it.

According to Pico's parable, after God made the universe and populated it with the angelic beings of heaven and the beasts of the Earth, each with its specific nature and function, he still needed a creature 'to think on the plan of his great work'.<sup>5</sup> As every niche in the cosmological ecosystem had already been filled, God decreed that Man should 'have joint possession of whatever nature had been given to any other

creature'.<sup>6</sup> Furthermore, being of an 'indeterminate nature' that was 'neither of heavenly nor earthly stuff, neither mortal nor immortal',<sup>7</sup> Man could choose with his own free will the attributes of any other created being, earthly or celestial. Only Man has the flexibility to choose his own path:

... with the sharpness of his senses, the acuity of his reason, and the brilliance of his intelligence [he is] the interpreter of nature, the nodal point between eternity and time.<sup>8</sup>

Aligning humanity with angels was fundamentally anathema to the Church of Rome, for whom the doctrine of original sin means that humans are born physically and spiritually soiled, only reaching Heaven if they submit to the Church's dogma and the pronouncements of its priests. And perhaps not even then.

Pico's landmark *Oration* opens with an appeal to two authorities. The first is 'Abdala the Saracen', the ninth-century Muslim



scholar Abd-Allah ibn Qutaybah, who declared there was nothing more wonderful in the world than Man. Pico follows with a quotation from the same mysterious sage whom Copernicus would also come to cite: ‘the celebrated exclamation of Hermes Trismegistus, “What a great miracle is man, Asclepius” confirms this [Abdala’s] opinion’.<sup>9</sup>

It is easy to see why Pico found himself in such hot water. It was not the best idea to start a debate with Holy City scholars by appealing to the authorities of both a Muslim and a resolutely non-Christian sage, Hermes Trismegistus. Notably, his theses also gave pride of place to the Cabala, the Jewish mystical system (which is very different from the modern cult popularized by Madonna).

Pico’s *Apology* only made matters worse. Under pressure from Roman scholars, Pope Innocent VIII swiftly banned it. In the interests of self-preservation Pico retracted his claims, before prudently fleeing to Paris. But

the Pope's arm was long, and even there he was imprisoned. Yet, as we will see, just when all seemed lost, Pico's fortunes were to turn around.

Pico's *Oration* is illuminating about the Renaissance for several reasons. It reveals the era's defining characteristic, a dramatic shift in attitudes about humanity: Man suddenly became a being of wonder with limitless abilities and possibilities rather than a miserable creature innately blighted and damned by original sin. It also highlights the clash between two mindsets: the new, open, questioning, eclectic spirit of the Renaissance – in particular its willingness to take seriously sources of wisdom outside the Christian domain – and the old, blinkered, Bible-bound attitude of the Middle Ages. The Church had always been wary of learning for learning's sake, frowning on novelty and intellectual challenge. The frenzy of interest in new ways to explore the universe and

humankind's place within it was the direct result of being freed from the old shackles. Effectively, the Renaissance represented a great surge of collective self-confidence.

To 'think for oneself' today often implies a rejection of established religion and all forms of 'superstition', however this was emphatically not the case among the intellectuals of Renaissance Europe. Most of the traditions from which Pico drew his theses were not established works of physics or mathematics, but metaphysical, mystical and what we have come to know as occult sources. Above all it was the works of Hermes Trismegistus that drove Pico with a passion.

There were many reasons why the Renaissance happened when it did. One was the renewed interest in the scholars and philosophers of ancient Greece and Rome, especially Plato. Many works from antiquity had been lost to Europe but preserved in the Middle East, from where they began trickling

back during the late Middle Ages. This became a flood in 1453 when Constantinople, the last bastion of the Byzantine Empire (itself the last bastion of the Roman Empire), fell to the Muslim Ottomans. Another factor was the expulsion of the Jews from Spain in 1492, their scholars dispersing into Europe's intellectual centres. Jewish traditions of learning had until then been ignored in Christian Europe.

Apart from the intellectual sphere, cultural, economic and political factors all played a part in giving birth to the Renaissance. The fact that its first flowering took place in Florence, for example, was intimately linked to the city's wealth as well as its republican government.

One of the most important defining factors of the Renaissance, however, was a renewal of interest in the esoteric, specifically the theory and practice of magic. Given the scale of its impact on the Renaissance and the fact

that it was hardly hidden away (as Pico's *Oration* clearly demonstrates), it is astonishing that historians completely ignored the influence of this renewed interest on the period until the 1940s, when studies began to reveal its influence over many of its great figures. It is only really over the last half-century or so that the crucial importance of esoteric, magical philosophies has been properly appreciated, as for example in the work of academics such as British historian Frances A. Yates (1899–1981). In a series of books published in the 1960s and 70s, Yates demonstrated that the Renaissance was largely motivated and driven by the 'occult philosophy', a blend of fifteenth- and sixteenth-century magical and esoteric systems.

The term 'occult philosophy' comes from one of the period's most important expositions of the principles of magic, *Three Books on the Occult Philosophy* (*De occulta*

*philosophia libri tres*) by Heinrich Cornelius Agrippa, published 1531–33. The Latin *occultus* simply meant hidden, obscured or, by extension, secret, but not necessarily supernatural. Agrippa's book would have been understood at the time it was published to be about 'hidden philosophy'.

Magic's reputation enjoyed a major boost in the Renaissance. From being the exclusive province of reclusive, more than usually malodorous and scary individuals, it very nearly became mainstream, and was widely discussed as a respectable aspect of philosophy and even theology. In his *Oration*, for example, Pico della Mirandola argues that magic is a valid path to knowledge, but is careful to differentiate between the more odious and hellish magic that utilises demons, and the natural magic that comprises the highest realisation of philosophy.<sup>10</sup> In the intellectual explosion that was the Renaissance, magic came to be considered an

integral part of all aspects of human knowledge.

As Frances Yates demonstrated, the Renaissance occult philosophy was based on three streams of esoteric thought. Of the three, modern academics favour what is now known as Neoplatonism, a philosophy and cosmology developed in the intellectual melting-pot of the Egyptian seaport Alexandria in the second and third centuries CE. Neoplatonism blended the original ideas – then already eight hundred years old – of the great Greek philosopher Plato with other Greek and Egyptian mystical concepts. A second strand was a Christianized version of the Jewish Cabala, which Pico aligned with the occult philosophy in what was to be considered his greatest innovation. But the third, and by far the most important strand, was Hermeticism,<sup>11</sup> the philosophy attributed to the legendary wise man honoured by Pico and Copernicus: Hermes Trismegistus,

or the ‘Thrice-Great Hermes’. And it is this strand that shifted the world from a morass of ignorance and self-hate to the sunlit uplands of intellectual genius.

The sheer power of Hermeticism cannot be overestimated. It effectively created the Renaissance, whose essence could be summed up by Hermes’ adage, ‘*Magnum miraculum est homo*’ (literally, ‘Man is a great miracle’). Hermeticism embraced that fanatical determination to discover, invent and understand, and the overwhelming sense of excitement at the prospect of endless possibilities. It seized the imaginations not only of Copernicus but also later luminaries. It drove them, hearts and minds, to dare to challenge the old thinking and encompass the most radical, even subversive, ideas – which changed the world forever. Their contributions to science would simply have been impossible without Hermeticism. Without



Hermes Trismegistus, these great thinkers would never have fully realized their genius.

# KEEPER OF ALL KNOWLEDGE

Hermes Trismegistus was a legendary Egyptian sage and teacher, whose wisdom was embodied in a collection of books known as the Hermetica. Although during the Renaissance Hermes Trismegistus was taken to be his full name – hence Copernicus simply calling him ‘Trismegistus’ – ‘Thrice-Great’ is an honorific, so his proper name is just ‘Hermes’. He was said to be a descendant of the god Hermes, or his Roman equivalent, Mercury.

During the Middle Ages, Hermes Trismegistus was a truly legendary figure, known only from odd fragments of his own supposed writings and references to him and his work in ancient texts. One such reference

came from Clement, Bishop of Alexandria, who around 200 CE witnessed Egyptian priests and priestesses parading their sacred books and noted that there were forty-two works of Hermes. (Which, if nothing else, according to cult comedy science-fiction writer Douglas Adams, is a number that is sacred to galactic hitch-hikers.)

Although scattered references to the Hermetica survived, all but one of the actual books had disappeared, at least in Europe. However, hand-written copies of many of the books did still circulate in Byzantium and, significantly, in Islamic centres of learning. At some point eighteen treatises were grouped together and became known as the *Corpus Hermeticum*. When, by whom and why they were selected, is unknown, but the *Corpus* was finalized by the eleventh century, and Byzantium seems to be the logical location for its compilation.

Another important source on Hermeticism was an anthology of around forty fragments, some from the *Corpus Hermeticum* but others otherwise unknown, compiled by the pagan Macedonian scholar Stobaeus around 500 CE, and including a complete treatise, *The Virgin of the World* (*Korè Kosmou*). Another Hermetic text may only be a mere half page long; the *Emerald Tablet*, but it is difficult to overstate its importance. Allegedly containing the words of Hermes Trismegistus himself, the thirteen alchemical maxims of the *Emerald Tablet* were believed to have originally been engraved on a tablet fashioned from the bright green jewel itself. Nobody knows for sure if this work has any connection with the Greek Hermetica, since it comes from an Arabic source that entered Europe via Spain in the twelfth century, but it was immensely influential among alchemists, helping cement Hermes' status as more than merely a wise man. To those whose

admiration bordered on worship, he was at the very least a semi-divine teacher.

The one complete Hermetic book known in Europe in the Middle Ages was the *Asclepius*, or *The Perfect Word*, a fourth-century Latin translation of a lost Greek original, a question-and-answer session between Hermes and his eponymous pupil. Asclepius was the Greek god of healing; the pupil in the treatise is his descendant, although he himself is not divine. The names of the characters, including Ammon and Tat (Thoth) who also appear as witnesses to the debate, reveal the Hermetic attitude to both divinity and humankind in general. This has it that while there is a God, human beings who attain a certain level of wisdom can themselves become divine. An example of this is presented in the form of Asclepius' ancestor, originally a mortal who discovered medicine, and who despite being dead and buried – his mummified body lay in a

specially constructed temple – was still able to intercede for the sick. Similarly, Hermes Trismegistus describes himself as a descendant of the god Hermes, who continues to help mankind.

The Hermetic texts are a mixture of on the one hand philosophical and cosmological teaching, and on the other astrology, alchemy and magic. Over the centuries, and even today, attempts have been made to separate the two, on the grounds that the philosophy itself is sophisticated and coherent, while the astrology and magic is considered primitive and incoherent. (One 1920s edition simply deleted this material.) Some even consider the compilation of the *Corpus Hermeticum* as an attempt to purge the canon of the most magically inclined texts. Of all the known Hermetica, those in the *Corpus* are conspicuously the least magical, but even they include some arcane elements – which is hardly surprising given that the

philosophy and cosmology are indivisible  
from an occult worldview.

# THE MIND OF GOD

The Hermetic books explore an intimately related cosmology, philosophy and theology that is fairly accessible in principle, even if some of the details are as abstruse as an ancient alchemical text, and for similar reasons. While any student might read the books, they are designed to speak only to the heart and mind of those who are worthy of learning their secrets. An ability to navigate the extraordinary allusions and metaphors, and an understanding of the connections between them, is in itself a sort of initiation into a world of spiritual and intellectual wonders.

Despite the medieval and Renaissance tendency to regard the books as the work of the great Hermes Trismegistus, they are obviously authored by various individuals who



‘present different interpretations of their common doctrine,’<sup>12</sup> and with scrupulous honesty often point out that some of the treatises are contradictory.<sup>13</sup> The reason for the attribution to Hermes is that all of the authors have chosen to remain anonymous, which – as we will see – is very telling. The writers believe that the common doctrine stems from Hermes, God’s chosen teacher of humankind, ‘the all-knowing revealer’.<sup>14</sup>

The Hermetica’s philosophy and cosmology is not only mystical but emphatically magical, embracing different realms of being, from gross matter to the divine spheres, and that of supernatural beings, divine, angelic and demonic. But ultimately it is monotheistic, ascribing all creation to a single God, while also encompassing lesser gods and goddesses, a category to which even mortal humans can aspire if they become sufficiently advanced. ‘Advanced’ is not merely the sort of ‘spiritual evolution’ that is

today assumed as a badge of superiority by New Agers; great intellectual progress that benefits humanity also qualifies. Asclepius won his godhood for his medical advances. (It certainly beats a Nobel Prize.)

Unlike the creator-God of Judeo-Christian tradition, however, the Hermetic God is intimately part of his creation. In the Hermetic vision, the universe is God and God is the universe. The cosmos is a living entity, and everything in it is imbued with life. Hermeticism also incorporates the once-common idea of the *anima mundi*, the world-soul. The Hermetic universe is really more of a great thought, an emanation of the mind of God, than something zapped into being on his orders. But God needs the universe in order to realize himself, as American historian of science and philosophy Ernest Lee Tuveson writes (his emphasis):

The essential elements of the Hermetist conception of reality is that the world emanates from the divine intelligence, and, as a *whole* in which *each* part is an essential component mem-

ber, expresses that great Mind. <sup>15</sup>

As the American philosopher Glenn Alexander Magee – whose speciality is the influence of esoteric thinking, and particularly Hermeticism, on western culture – points out, this explanation of God's need to create the universe overcomes some of the nonsensical aspects of the biblical creation tale. Magee points out that the traditional Judeo-Christian account provides no good reason why God should have wanted or needed to create either the universe or humankind: what does he get out of it? This was one of the main reasons the Hermetic explanation appealed to the increasingly sophisticated Renaissance thinkers: 'The great advantage of the Hermetic conception is that it tells us *why* the cosmos and the human desire to know God exist in the first place.'<sup>16</sup>

Hermeticists see human beings as enjoying a special place in creation. As essentially divine beings stuck in animal bodies,

according to Hermeticists, human beings possess not only the divine spark (which is present in everything) but effectively share in God's mind. Humans are the only beings in God's creation with the potential to become divine. Salvation, in the Hermetic scheme, comes from the use of our advanced mystical and intellectual faculties. As Treatise X of the *Corpus Hermeticum* states:

For the human is a godlike living thing, not comparable to the other living things of the earth but to those in heaven above, who are called gods. Or better – if one dare tell the truth – the one who is really human is above these gods as well, or at least they are wholly equal in

power to one another.<sup>17</sup>

One therefore ascends through knowledge, which comes through both greater intellectual and philosophical understanding of the cosmos and the more spiritual form of enlightenment called *gnosis*. But the relationship between creator and humanity is an endless cycle, as Magee notes:

Hermeticists not only hold that God requires creation, they make a specific creature, man, play a crucial role in God's self-actualization. Hermeticism holds that man can know God,

and that man's knowledge of God is necessary for God's own completion.<sup>18</sup>

So, not only did the Hermetic vision provide a more satisfactory explanation of why the universe exists, it also gave human beings *potentially* the most exalted role – though one that has to be earned. As *Asclepius* declares, ‘a human being is a great wonder, a living thing to be worshipped and honoured’.<sup>19</sup> The *Hermetica* encourages people to use all their faculties, powers and talents in the pursuit of both self knowledge and knowledge of the universe. A major part of the kinship with creation involves observing the world around us and delving deeply to discover its hidden workings. In Hermeticism, this is not mere lofty sentiment, but one of the major paths to salvation. The Hermetic motto ‘Follow nature’<sup>20</sup> – which would come to have a profound effect on the beginnings of science – bears witness to this cornerstone of the philosophy.

# MAGIC AND MYSTERY AT HARRAN

Wherever and whenever Hermeticism originated, it was being discussed by both Christian and non-Christian writers in the Roman Empire from the second century onwards. But it disappeared soon after Christianity became the dominant Roman religion and persecutor of pagans in the fourth century. Apart from a fragmentary presence, the Hermetica basically vanished from Europe until the Renaissance. But its wisdom survived outside the Christian world, focusing on the city of Harran, some fifty miles south of Edessa in south-eastern Turkey. How it came to be established there is unknown, but presumably Hermeticists fleeing from Christian persecution would provide an answer.

By the time Harran fell into Arab hands in the mid-seventh century it was a renowned centre of learning. Two centuries later, according to tradition – which may or may not be apocryphal – the inhabitants were given a stark choice by the caliph al-Mamun: convert to Islam, be massacred, or identify themselves as one of the ‘peoples of the book’. The Qur’an requires tolerance and protection for the latter – such as Jews and Christians – provided they venerate a prophet recognized by Islam.

Unsurprisingly rejecting the option to be massacred, the residents of Harran identified themselves as Sabians, one of the ‘peoples of the book’ mentioned in the Qur’an.<sup>21</sup> But the Sabian prophet was found in neither the Old nor the New Testament. Instead they proudly declared him to be Hermes and their holy book the *Corpus Hermeticum*. Fortunately the Qur’an identifies Hermes with the prophet Idris, the Muslim

rendering of the Old Testament Enoch. The Sabians of Harran also venerated Asclepius as a prophet and Agathodaimon ('Good Spirit'), a character in the Hermetic dialogues, as a great teacher and an intermediary with God.<sup>22</sup> They went on pilgrimages to the two great pyramids at Giza, revering them as the tombs of Hermes and Agathodaimon.<sup>23</sup>

Soon after the al-Mamun episode was supposed to have happened, the great library of Baghdad, the House of Wisdom (*Bayt al-Hikma*) – which was also a centre for research, translations of foreign works and an observatory – was re-established. Many Sabians moved there, the most eminent of which was the renowned polymath Thabit ibn Qurrah (835–901). It was here, in Baghdad, that the Hermetic books were translated into Arabic. The foundation of Arab science in the Middle Ages was therefore laid by the Sabians, and inspired by the Hermetica.<sup>24</sup>



The Sabians disappeared from Baghdad and Harran during a clampdown on non-Muslims in the middle of the eleventh century. It is possible they became devotees of Sufism, the mystical form of Islam, which aims at individual communion with the divine. Although Sufism had been around for centuries, it underwent a formalization during the eleventh century that was, some think, due to a Sabian influx.<sup>[25](#)</sup>

Many specialists have noted that the revival of interest in the Hermetica in Byzantium coincided with the end of Sabian Hermeticism.<sup>[26](#)</sup> But was this purely coincidence? Psellus, the Byzantine Platonic philosopher, became the first westerner to write about the Hermetica in half a millennium and many have speculated that Sabians, fleeing persecution, had carried their precious literature with them to Constantinople.

# THE REDISCOVERY

One of the great patrons of the early Renaissance was Cosimo de' Medici, scion of the banking dynasty that pretty much owned the republic of Florence. Cosimo was also hugely ambitious in his vision of what he and his court could accomplish, sending agents out in search of key books, and employing one of the great scholars of the age, Marsilio Ficino (1433–99), on massive learned projects and as tutor to his grandson Lorenzo. Cosimo's aim was nothing less than to re-establish Plato's Academy, this time in Florence, with Ficino as its head. The lynchpin of this somewhat ambitious task was the first ever translation of Plato's complete works from Greek into Latin, then the *lingua franca* of scholarly Europe.

Just as Ficino was about to dip his quill into the ink and get started on Plato, an even more exciting prospect presented itself. One of Cosimo's agents, a monk named Leonardo de Pistoia, returned from Macedonia with a Greek manuscript of the first fourteen treatises of the *Corpus Hermeticum*. Ficino records that in 1463 Cosimo ordered him to drop his translation of Plato forthwith in order to concentrate exclusively on the *Corpus Hermeticum* – an urgency that was probably the result of Cosimo becoming gravely ill, and desperately wanting to read the Hermetic books before he died. He got his wish, with a year to spare.

Because of the mysterious aura surrounding Trismegistus and his lost books, this was by far Ficino's most popular work, as evidenced by the many copies of the manuscript and several editions of the first printing of 1471. The discoveries that Ficino's translation made possible sent seismic shockwaves

throughout the academic community in Florence and beyond, being widely and feverishly discussed and debated. The books enticed Pico della Mirandola to Florence, where he studied under Ficino between 1484 and 1486, when he departed for Rome with his nine hundred theses. As Tuveson writes in *The Avatars of Thrice Great Hermes* (1982), ‘with the translation by Ficino of the *Hermetica* in the fifteenth century, a kind of “new force” had entered the Western world.’<sup>27</sup>

One reason for the excitement generated by the rediscovery of the *Hermetica* was precisely because it was so radically different from Christianity’s stifling view of creation and humanity’s place within it. Another was the idea that an ancient original religion, now lost, lay behind all other religions. This was variously known as the *prisca theologia* (‘ancient theology’), *prisca philosophia* (‘ancient philosophy’) or *philosophia perennis*

(‘perennial philosophy’). Many believed that this ancient, lost religion could be found in Egypt, as even the Bible acknowledged that its civilisation and religion pre-dated that of the Israelites. Indeed, there was even a suggestion that Moses himself learned great secrets from the Egyptians. Given that Hermes Trismegistus was thought to be the renowned sage of ancient Egypt, it was logical that the Hermetica could contain the ancient theology.

Ficino was hugely influential in his own right. His close relationship with these books lured him ever deeper into the Hermetic world, and he began to discern strangely recurring themes. A modern writer on Italian history, Tim Parks, describes Ficino’s momentous declaration:

The whole world, it seemed, had always followed a single faith whose ancient priests included Zoroaster, Hermes Trismegistus, Orpheus, Pythagoras, Plato, St Paul, St

Augustine. [28](#)

Thus, according to Ficino, a secret line of priests linked the ancient pagan and Christian beliefs. Ficino threw himself into trying to recover and reconstruct this 'single faith', concluding that it was a magical current flowing under and linking many otherwise apparently irreconcilable belief systems. From this he developed the idea of 'natural magic', one that worked with the forces of nature rather than by the conjuration of demons or spirits.

The robust joy in life that marked the Hermetic path extended well beyond that of academic study. As American researcher Peter Tompkins writes:

Ficino regarded sexual desire as a current of energy responsible for the cohesion of the entire universe ... Ficino even went so far as to recommend the pagan revels of Bacchus (or Pan) as a way of escaping from normal human limitations into an ecstasy in which the soul was mira-

culously transformed into the beloved god himself. <sup>29</sup>

Ficino's masterwork was *Three Books on Life* (*De vita libri tres*), published in 1489, which was extremely influential on arcane

philosophers such as Agrippa. But once again, despite being a synthesis of several magical and philosophical systems, Hermeticism stood firmly as the heart and soul of Ficino's work.

The next step would be from Florence to Rome. Astounding though it may seem to us today, many in the highest echelons of the Catholic Church were sympathetic to the message of Hermeticism, and considered it to be compatible with Christianity.

The *Hermetica* proclaimed that the material universe was created by a lesser god, or Demiurge, who had been assigned the task by the God of all. In *Asclepius*, God is said to love this second god as 'His own Son',<sup>[30](#)</sup> which has obvious parallels with Jesus. In *Pimander*, the first treatise of the *Corpus Hermeticum*, God's creative Word is also described as the 'Son of God'<sup>[31](#)</sup> – to some a clear echo of the majestic opening of the

Gospel of John: 'In the beginning was the Word'.

Such references led some early Christian proselytes, such as the late-third/early fourth-century author Lactantius, to accept Hermes Trismegistus as a pagan prophet who foresaw the coming of Christ. This view was by no means unanimous: others such as St Augustine ascribed Hermes' foreknowledge to warnings from worried demons. But when the *Hermetica* was rediscovered in the fifteenth century, at least enthusiasts could argue their case by invoking early Church authorities.

Some thinkers tried hard to find a compromise, accepting the philosophy and cosmology but rejecting the magic, while others, such as Pico della Mirandola, pointed out that the two sides of the *Hermetica* were inseparable and argued this demonstrated that magic – provided there was no occult nastiness such as conjuring spirits – was a



legitimate Christian activity. After all, Moses had engaged in magical contests with the pharaoh's magi and had probably learned magic in Egypt. Some even suggested that Jesus had performed his miracles by means of natural magic.

Others went further, seeing Egypt as the origin of the wisdom inherited first by the Jews and then by the Christians. This, they argued, elevated Hermes to at least an equal footing with Moses, who despite not being a Christian, was still deserving of respect for his contribution to the religious tradition into which God had chosen to send his son.

The extent to which men in high places accepted this reasoning – even, astonishingly, including the Pope himself – can be demonstrated by resuming the story of Giovanni Pico della Mirandola, who we left earlier languishing in a Parisian prison after his arrest on the orders of Pope Innocent VIII. He didn't languish for long. As Pico was from a

well-connected family, his powerful supporters interceded with the Pope on his behalf. One such supporter was Charles VIII of France, and another Lorenzo de' Medici – 'the Magnificent' – who was now one of the wealthiest and most powerful men in Florence. Eventually the Pope allowed Pico to return to Florence, under Lorenzo's guarantee that he would behave himself, although his works remained on the banned list.

In 1492 Innocent VIII died and was succeeded by the Spaniard Rodrigo Borgia, who wore the papal crown as Alexander VI. His reign certainly began with a bang. He not only absolved Pico and his works from all taint of heresy but wrote him a fan letter, and the fact he did so early in his reign demonstrates how strongly he felt about it. Tantamount to a papal endorsement, the letter was included in subsequent editions of Pico's books. In the event, Pico's repatriation

was short lived, as he died in 1494, at the age of just thirty-one.

But why did Alexander support this heretical upstart? As his fan letter suggests, Pico and the Pope shared a passion for all things Hermetic. The Borgia Pope even commissioned tell-tale decorations for his personal rooms in the Vatican – the Appartamento Borgia – which survive to this day. In the series of frescoes on mythological themes by Pinturicchio, Hermes Trismegistus is depicted twice, possibly three times if an image of Mercury slaying the giant Argus is intended as a veiled reference to him.

The first Hermetic reference in Alexander's apartment is in a series of pictures showing the pagan and Jewish prophets who allegedly foresaw Christ's coming. So far this is conventional: images or statues of Hermes Trismegistus appear in several cathedrals for the same reason. More unexpected is a painting in which Hermes and Moses are

shown sitting before Isis, implying that Alexander accepted Hermes' equal status to Moses and that both drew their wisdom from Egypt. Judaism is seen as having emerged from the Egyptian Hermetic religion just as Christianity was to emerge from Judaism. Not only does Isis therefore appear in the Vatican, but she is depicted in all her power and glory – not as some pagan deity wretchedly grovelling to a triumphant Christianity.

Other peculiar pro-Egyptian imagery in the Borgia apartments relates to bulls. As that animal was the Borgia family's symbol, this may not be so surprising, at least at first glance. However, the bas-reliefs in Alexander's apartments clearly associate the Borgia bull with the sacred Apis bull of Egypt, which is shown being worshipped and, in turn, worshipping the cross. Once again an association between Christianity and the religion of Egypt is implied, linked thematically with

a Borgia pope worshipping Christ, suggesting that the relationship between Hermeticism and Christianity was important to Alexander.

However, extraordinary though it may seem, this is not to imply that Alexander wasn't a Christian, or that a closet occultist had infiltrated the highest office of the Church. It was quite permissible to see Christianity as the heir of a tradition that stretched back to ancient Egypt, and one to be celebrated. Such associations belonged to the new spirit of the time. Indeed, the most surprising thing about the Appartamento Borgia frescoes is that they indicate that even a Borgia pope was capable of caring more deeply about his religion's origins than most Catholics at the time.

# THE TRIUMPH OF HERMES

Eighty years after the rediscovery of the lost books of Hermes, Copernicus gave pride of place to the legendary Egyptian sage in his own seminal work on the movements of the planets. But why?

It is hardly surprising that Copernicus was familiar with the *Hermetica*, having studied in Rome and Padua in the 1480s and 90s, where it was on everyone's lips. But evidence suggests that the works meant considerably more to him than mere intellectual fashion. The debt Copernicus owed to the *Hermetica* is demonstrated by the fact that the three revolutionary ideas he was to famously propose – the Earth's motion in space, its rotation on its own axis and the orbiting of the

Earth and other planets around the sun – *all appeared in the Hermetica*.

*Asclepius*, for example, provides the following statement in the middle of a discourse on ‘classes’, or archetypes:

The class persists, begetting copies of itself as often, as many and as diverse as the rotation of the world has moments. As it rotates the world changes, but the class neither changes nor ro-

tates.<sup>32</sup>

Hermeticism lays great emphasis on the sun, which is regarded as a kind of relay station for God’s creative and sustaining power and described in turn as the ‘visible god’ and a ‘second god’.<sup>33</sup> But although it isn’t so surprising to find the sun given such prominence in the *Hermetica*, some passages about its importance are intriguingly specific. Treatise XVI, in which *Asclepius* expounds various points of teaching to King Ammon, contains two particularly tantalizing statements: ‘For the sun is situated at the centre of the cosmos, wearing it like a crown’<sup>34</sup>; and

‘Around the sun are the eight spheres that depend from it: the sphere of the fixed stars, the six of the planets, and the one that surrounds the earth.’<sup>35</sup>

These ‘spheres’ correspond to the modern concept of orbits, as it was thought that the celestial bodies were fixed to transparent spheres. Under the old Ptolemaic system the spheres surround (‘depend from’) the Earth, with the sun occupying its own sphere. But this is not what is described in Treatise XVI, with the spheres surrounding the sun, which is situated at the centre. And the Earth has its own sphere which, like the other planets, ‘depends from’ the sun in a way that only makes sense in Copernican terms.

Perhaps most interesting of all is the fact the heliocentric aspects are only mentioned in passing, when some other principle is being elucidated. It appears that the writers of at least these particular Hermetic treatises took the Earth’s journey around the sun for



granted. Clearly, by referring to Hermes Trismegistus in his own exposition of the heliocentric system – besides quoting from Ficino on the sun as the embodiment of God – Copernicus shows that he was at least familiar with the prototype for his own ideas. As Frances Yates concluded:

One can say, either that the intense emphasis on the sun in this new worldview was the emotional driving force which induced Copernicus to undertake his mathematical calculations on the hypothesis that the sun is indeed at the centre of the planetary system; or that he wished to make his discovery acceptable by presenting it within the framework of this new attitude. Perhaps both explanations would be true, or some of each.

At any rate, Copernicus' discovery came out with the blessing of Hermes Trismegistus upon its head, with a quotation from that famous work in which Hermes describes the sun-

worship of the Egyptians in their magical religion. <sup>36</sup>

While Tobias Churton, the British authority on Hermeticism and Gnosticism, states that (his emphasis):

One gets the impression that Copernicus is saying: *the truth of the matter was already there, but went unseen because we judged things from an earthly perspective. But Hermes, at the*

*beginning of science, he saw it.* <sup>37</sup>

The fact that Copernicus was inspired by the Hermetica also, of course, made the debate

over heliocentricity of keen interest to Hermeticists, especially as it seemed to vindicate their semi-sacred texts. If the theory could be proven beyond doubt, it would engender confidence in the entirety of the Hermetic philosophy. And as we shall see, there were some who took it considerably further than that. Certainly, and unsurprisingly, in the ensuing furore about Copernicus' new theory, the Hermeticists were among his most ardent supporters.

## ‘TOO MUCH IN THE SUN’

As already mentioned, it is a misconception that the heliocentric theory in itself sparked off a notorious religious furore. Although Copernicus dedicated his book to Pope Paul III, he was not, as many assume, simply boot-licking in an attempt to head off papal disapproval. After all, Paul was quite happy with Copernicus' theories ten years before *On the Revolutions* was published. In the dedication, somewhat airily, Copernicus explained his reluctance to go public by saying he wanted to avoid harsh words from lesser scholars: he was not concerned it might stir up theological controversy, let alone accusations of heresy.

Even the notorious preface, apologetically explaining that the ideas contained therein were just theories, no more valid than any

other about the workings of the heavens, was designed to placate *scholars*. The preface was actually written by a Lutheran theologian, Andreas Osiander, who oversaw the printing of *On the Revolutions* after Copernicus' death. But because Osiander didn't make his authorship clear, many readers assumed the preface expressed Copernicus' own position. Georg Rheticus, the mathematician who persuaded Copernicus to go public with his theory, later threatened to beat Osiander up for his audacity.

The heliocentric theory raised no major theological difficulties anyway. True, there are a handful of implications in the Old Testament concerning the immobility of the world. The First Book of Chronicles, for example, states that, 'The world is firmly established; it cannot be moved',<sup>38</sup> and Joshua is said to have convinced God to stop the sun in the sky, which implies that it was the sun, not the Earth, which moves.<sup>39</sup> But in the end

few churchmen thought Copernicus' theory was worthy of oiling the rack and heating the pincers.

Ironically, any religious objections came not from the Vatican but from Protestants, although even the most hellfire-and-damnation regarded the theory as mere folly as opposed to blasphemy. Martin Luther himself ridiculed it, but mainly because he was aghast at the suggestion that astronomy could have got it so fundamentally wrong for so long.

This was also largely the position of scholars, who too were disturbed for another reason, which is less obvious today. Proposing that traditional astronomy was profoundly flawed seemed intimidating, since it implied that human understanding of the order of the universe, and the way one part influenced another, was seriously lacking. If Copernicus was right, then *everything* changed.

This was not yet the era of science as we know it in the modern sense. Even learned men such as Copernicus and Johannes Kepler believed that a greater understanding of the movements of the heavenly bodies would improve the accuracy not only of astronomy but also its esoteric twin, astrology. No astronomer at that time believed the workings of the universe were due to impersonal physical forces. To them, God had decreed that the universe should operate in the way it did. As such, discovering how it worked offered an insight into the divine mind, and might also throw light on God's plan for all creation. This mindset drove the likes of Kepler who, building on Copernicus' work, established the laws of planetary motion.

Kepler (1571–1630) was another great name of the scientific revolution who was steeped in the Renaissance occult tradition. He believed that the planets, including the

Earth, are living entities with their own world souls and that the seat of the *anima mundi* is in the sun. As an astrologer he wrote that a new star that appeared in 1604 portended major changes on Earth. Unsurprisingly, his writings also reveal a detailed knowledge of the *Corpus Hermeticum*.

A suggestion that Kepler drew direct inspiration from the works of Hermes Trismegistus appears in the following enigmatic statement from the *Harmony of the World* (*Harmonices mundi*), in which he outlined the laws of planetary motion:

... after the pure Sun of that most wonderful study began to shine, nothing restrains me; it is my pleasure to yield to the inspired frenzy, it is my pleasure to taunt mortal men with the candid acknowledgement that I am stealing the golden vessels of the Egyptians to build a tabernacle to my God from them, far, far away from the boundaries of Egypt ... See, I cast the

die, and I write the book. [40](#)

Some embraced Copernicus' new ordering of the solar system as a leap forward in understanding the workings of creation, but it absolutely terrified many others. If the

traditional understanding of cosmological behaviour was wrong, then how could men begin to understand their own place in the universe? And the uncertainty – some accepted Copernicus' new order, others stuck to the old system of Ptolemy – meant that chaos reigned, and not merely in the academic discipline of astronomy, but in the world at large. This aspect of the heliocentric debate was so significant at the time it even surfaces as a major theme in William Shakespeare's *Hamlet*. Shakespeare was obviously familiar with Hermeticism, as allusions appear in his works, for example in Hamlet's homage to humankind which echoes Pico's vision: 'What a piece of work is a man! How noble in reason! How infinite in faculty! ... In action how like an angel! In apprehension how like a god!'<sup>41</sup>

Astronomers, rather than literary historians, have often seen clear and specific allusions to the debate over the heliocentric



theory in the play, which dates from around 1600. Peter D. Usher, Professor Emeritus in Astronomy and Astrophysics at Penn State University, has recently argued that the whole work is an allegory for the struggle between the two models of the universe, suggesting that *the* major theme is that Hamlet, prince of the new learning and repeatedly associated with the sun, is involved in a bid to establish his rightful place as the king – at the centre of his universe – by overthrowing his uncle Claudius. It just so happens that Ptolemy's first name was Claudius.

References to the heliocentricity controversy are undeniably scattered throughout the play. For example, Hamlet writes to his love interest Ophelia:

Doubt that the stars are fire;

Doubt that the sun doth move;

Doubt truth to be a liar;

But never doubt I love.<sup>42</sup>

Other references are less obvious today. For example, many generations of readers and actors have studied Hamlet's apparently peculiar declaration, 'I could be bounded in a nutshell and count myself a king of infinite space',<sup>43</sup> without realizing its potentially subversive undercurrent.

The leading supporter of Copernicus' theories in Shakespeare's England was the mathematician (and Member of Parliament) Thomas Digges, who went one step further than his hero. Although Copernicus maintained the traditional belief that the stars all exist on the same sphere, equally distant from the centre of the solar system, Digges suggested that they are positioned at different distances in an infinite universe. His actual words were that the world was not enclosed in the stellar sphere '*as in a nutshell*'.

And as Shakespeare knew Digges personally – they lived in the same building in Bishopsgate, east London, and Digges’ son worked at the Globe Theatre<sup>44</sup> – there seems little doubt the ‘nutshell’ line was an allusion to Digges’ theory.<sup>45</sup>

But the most specific of Shakespeare’s references to the heliocentric debate relate to Tycho Brahe (1546–1601), the flamboyant Danish alchemist and astronomer (whose eccentric household included a clairvoyant dwarf who lived under his table and a pet elk that met its end in a drunken plunge down stairs). Tycho’s great ambition was to reconcile ‘the mathematical absurdity of Ptolemy and the physical absurdity of Copernicus’<sup>46</sup> through a hybrid model in which the sun and moon orbit the Earth but the other planets and stars orbit the sun. Tycho therefore literally embodied the struggle between the two great systems.

Tycho was employed by his patron, Frederick II of Denmark, to purchase artworks and scientific equipment for his new castle at Elsinore (built just twenty-five years before *Hamlet* was written), where the play is set. Frederick gave Tycho the island of Hven, in sight of the castle, to build an observatory, Uraniborg. The character of Hamlet, like Tycho, was a graduate of the University of Wittenberg. Most tellingly, two of Tycho's relatives were envoys to London in Shakespeare's day. Their names – Frederick Rosenkrantz and Knud Gyldenstierne – are the same as Hamlet's ill-fated peers, Rosencrantz and Guildenstern.

Obvious though the links may be, what was Shakespeare trying to convey about the big heliocentric debate? After all, the play sees the demise of all of its leading characters, including Hamlet himself, in the famously bloody finale. So although Shakespeare seems to be championing the new

Copernican system, his major emphasis is really the uncertainty that was overturning the world and throwing everything into chaos.

During Shakespeare's time, none of this was an issue for the Church, which had long frowned on astrology. But by Galileo's day heliocentricity had become a burning issue and its spokesmen were condemned as heretics. He was first warned off in 1616, and it was only in that year – seventy-seven years after it was published – that the Catholic Church placed *On the Revolutions* on its Index of Forbidden Books. From that point on books advocating heliocentricity were automatically relegated to the Index, a practice that only ended in 1758.

What had changed? Why, by the 1600s, had heliocentricity become a matter of life and death? What made it so dangerous that even the Church of Rome was running scared?

The answer to these questions lies almost entirely in the threat posed by one man ...

## Chapter One

- 1 Morris A. Finocchiaro, from his introduction to Galileo, *Galileo on the World Systems*, p. 2.
- 2 Davies, *The Goldilocks Enigma*, p. 147.
- 3 Our translation of the Latin: '*Siquidem non inepte quidam lucernam mundi, aln mentem, aln rectorem vocant. Trismegistus visibilem Deum ...*'
- 4 For example, Washington State University's World Civilizations website: [www.wsu.edu:8001/~dee/REN/PICO.HTM](http://www.wsu.edu:8001/~dee/REN/PICO.HTM)
- 5 Pico della Mirandola.
- 6 *Ibid.*
- 7 *Ibid.*
- 8 *Ibid.*
- 9 *Ibid.*

- [10](#) See Yates, *Giordano Bruno and the Hermetic Tradition*, pp. 87–91.
- [11](#) Some academics prefer ‘Hermetism’, while others use that term for the original philosophy of the early centuries CE and ‘Hermeticism’ for its Renaissance reincarnation.
- [12](#) Tuveson, p. 9.
- [13](#) E.g. the opening of Treatise XVI (Copenhaver, p. 58).
- [14](#) Lindsay, p. 166.
- [15](#) Tuveson, p. xi.
- [16](#) Magee, p. 10.
- [17](#) Copenhaver, p. 36.
- [18](#) Magee, p. 9.
- [19](#) Copenhaver, p. 69.
- [20](#) Tuveson, p. xii.
- [21](#) The relationship between the Sabians of Harran and the Sabians mentioned in the Qur’an – known to us today as the Mandaeans, a baptismal sect whose homeland is in

southern Iraq and Iran and who venerate John the Baptist as their great teacher – is a matter of controversy. The line taken by the Arab chroniclers who first set down the al-Mamun story – the earliest account was written about a hundred years after it was supposed to have happened – is that the Harranians took the name simply because although it appears in the Qur'an by then everyone had forgotten who the Sabians were. This is also the position of most historians. However, there is an intriguing complication, as the Mandaean also have an ancient link with Harran, which seems to be stretching coincidence rather far, especially for us personally since they were central to our research on the true status of John the Baptist, as discussed in our books, *The Templar Revelation* (Chapter 15) and *The Masks of Christ* (Chapter 7).

[22](#) Gündüz, pp. 157–8 and 209.

[23](#) *Ibid.*, p. 208.

[24](#) Churton, *The Golden Builders*, p. 27.

[25](#) See *ibid.*, p. 38.



- [26](#) E.g. Copenhaver, p. xlv.
- [27](#) Tuveson, p. ix.
- [28](#) Parks, p. 207.
- [29](#) Tompkins, p. 52.
- [30](#) Yates, *Giordano Bruno and the Hermetic Tradition*, p. 7, quoting an 1871 translation by William Fletcher. Copenhaver (p. 71) renders the phrase as ‘progeny of his own divinity’.
- [31](#) Copenhaver, p. 2.
- [32](#) *Ibid.*, p. 89.
- [33](#) E.g. in *Asclepius* (*ibid.*, p. 85).
- [34](#) *Ibid.*, p. 59.
- [35](#) *Ibid.*, p. 61.
- [36](#) Yates, *Giordano Bruno and the Hermetic Tradition*, pp. 154–5.
- [37](#) Churton, *The Golden Builders*, p. 59.
- [38](#) 1 Chronicles 16:30 (TNIV).

[39](#) Joshua 10:12–13 (TNIV).

[40](#) Kepler, p. 391.

[41](#) *Hamlet*, Act II, scene 2.

[42](#) *Ibid.*

[43](#) *Ibid.*

[44](#) Gingerich, p. 23.

[45](#) See Couper and Henbest, pp. 111–3.

[46](#) Quoted in *ibid.*, p. 116.

## CHAPTER TWO

# THE HERMETIC MESSIAH

Although largely forgotten today, the Dominican monk-turned-heretic Giordano Bruno was regarded as one of the greatest intellects and philosophers of his time. The champion *par excellence* of the Hermetic tradition, he travelled Europe preaching its virtues and arguing for a root-and-branch reform of society based on its principles. He aimed to be Hermeticism's greatest prophet – even its messiah – but instead became its greatest martyr, ending his days in the searing embrace of the Inquisition.

Bruno was messianic, bombastic and stubborn, with a huge ego and belief in his own

brilliance and importance. But then a man whose whole philosophy and mission in life centres on the Hermetic adage of *magnum miraculum est homo* is hardly destined to be a shrinking violet. He saw himself as living proof of just how miraculous a man could get. Where he parted company with most typical egocentrics, however, was that he considered all men and, less usual for the time, all women, as being either actually or potentially as brilliant as himself. The targets of his greatest fury were those who held people back, who told them they were insignificant and worthless. Surely it is difficult for a philosophy to be more diametrically opposed to the Christian doctrine of original sin, the idea that babies are born in a fallen state due to the famous transgression of Adam and Eve.

Bruno was first, foremost and totally besotted with Hermeticism, the great golden thread that connected his philosophy,

religion and magic. He wrote a huge number of treatises and poems that contained coded and symbolic teachings, being heavily influenced by the works of Ficino and Agrippa, although characteristically he was never afraid to depart from them.

Bruno was born in 1548 – five years after the publication of *On the Revolutions of the Celestial Spheres* – in the town of Nola in the Kingdom of Naples, which comprised the whole of the southern half of Italy and, due to the complex geopolitics of the day, actually belonged to the Spanish king of Aragon. As we will see later, this area witnessed particularly odd activities during the sixteenth and seventeenth centuries, mostly concerning Dominican monks. Although he was baptised Filippo, when he became a monk in the Dominican monastery in Naples at the age of sixteen, he took the name Giordano (or ‘Jordan’, from the baptismal river). Like many bright kids from a humble background

– his father was a soldier – his decision to become a monk was probably the only career move that allowed him to get an education. And he was indeed very bright, being particularly distinguished for his mastery of mnemonics and memory systems, even being summoned to Rome by Pope Pius V to explain how they worked.

The ‘Nolan’, as Bruno was often known, refused to let anybody tell him what to think or even what he could and couldn’t study, which was something of a shortcoming in a sixteenth-century monk. In 1576, at the age of twenty-eight, he came under suspicion for heresy, or rather suspicion of suspicion of heresy. ‘Suspect of heresy’ was the formal term for a transgression against Church law, committed by those who read heresy and listened to heretics, even if they disagreed with them. At that time it was in fact best for one’s health and safety to have no dealings with the work of heretics at all. (The official

transgression had the somewhat Monty Pythonesque subdivisions of 'Vehemently Suspect' and 'Slightly Suspect', although there was nothing funny about the Vehemently Painful punishment.)

Though the details are a bit sketchy, it appears that all Bruno did was read and discuss ideas that had been condemned as heretical. He certainly debated the Arian heresy<sup>1</sup> in tones that weren't unequivocally negative and questioned the doctrine of the Trinity, largely because he thought it made no sense. (He later maintained to the Inquisition he had never denied the doctrine, only doubted it.) And he hid a copy of a book by the Dutch proto-Protestant Erasmus in the monastery toilet – although he could easily have explained away its presence as toilet paper, which would no doubt have appealed to his superiors. Perhaps that's what he did do. It would have been in keeping with his character.



Despite being mild compared to what he would preach later in his life, this string of actions coupled with his general freethinking was enough to attract suspicion, and so he abandoned the monastery and fled from Naples. For five years he wandered around northern Italy, southern France and Switzerland and appeared in Venice, Padua, Milan, Geneva, Lyons and Toulouse, among other places. Given the extent of his travels, it is impossible to pinpoint how and when Bruno became devoted to Hermeticism and magic. He may have started to study it in the monastery (perhaps in the toilet?), or perhaps encountered it during his wanderings, but the catalyst for his entrance into the world of the arcane is most likely to have been his fascination with memory systems.

The art of memory, which Bruno did much to help revive, developed in classical Greece as a system for storing and recalling information using specific mental images. So

powerful is the system that it is still widely used today, even by celebrities such as the gifted British illusionist Derren Brown. However, an esoteric version of this technique that combined the mental images with magical principles could, it was believed, be used not just to remember what had already been learned but to acquire completely new information. Briefly, this version employed the principles of talismanic magic, in which different symbols, shapes, colours and materials are deemed to have specific properties and energies based on magical associations. The trick was to use those principles when forming the mental images. It was as if a portal opened and hidden knowledge flowed in. It was writing books on the magical art of memory that made Bruno's reputation when he settled in Paris in 1581, but by this time he had also developed some extraordinary ideas about the importance of magic in general and Hermeticism in particular – ideas which

challenged its previously conceived limitations.

As we have seen, since the rediscovery of the *Hermetica* a century before Bruno's birth, many had believed Hermeticism was compatible with Christianity, as its sacred books could be seen to foreshadow the coming of Christ. However, as far as Bruno was concerned this line of reasoning didn't go far enough. As Frances Yates explains:

Giordano Bruno was to take the bolder course of maintaining that the magical Egyptian religion of the world was not only the most ancient but also the only true religion, which both

Judaism and Christianity had obscured and corrupted.<sup>2</sup>

Bruno burned with a sense of destiny, believing passionately that it was his mission to restore the old Egyptian religion, and that this would bring an end to Europe's political and social ills. He also saw Hermeticism as a way of transcending the religious schisms that were causing such horrors.

One of the keys to understanding his Egyptian passion is found in the famous section of *Asclepius* known as the 'Lament', in which Hermes warns of a time when the gods will abandon Egypt to the rule of foreigners, who will then establish their own false religions and ban the country's traditional faith on pain of death. This will, Hermes continues, be a tragedy not just for Egypt but for the world, since Egypt is the home of the gods on Earth, and once they leave the land they will be lost to all mankind. But, he goes on, in time the one God will intervene and the lesser gods will be restored, and 'stationed in a city founded at Egypt's farthest border toward the setting sun, where the whole race of mortals will hasten by land and sea.'<sup>3</sup>

Because the Hermetic books were believed to hail from the zenith of the Egyptian civilization, the Lament was seen as an authentically ancient prophecy. And since the time of

their writing, it had come to pass that Egypt's native religion had indeed been eclipsed: since Alexander the Great's invasion in the fourth century BCE, the country had been under foreign domination – first Greeks, then Romans, then Christians and now Arabs. It stood to reason that if the first part of the prophecy was true, the second part might be also. The ancient gods might return, and a golden Hermetic city might be built that would draw the whole world to its magic.

While most considered the Greeks and Romans as the interlopers responsible for crushing Egypt's religion, Bruno singled out the Christians as the real villains. He may even have been right. Although the Greek and Roman overlords did import their own gods and cults, they also permitted the continued practice of religions native to the area. As we mentioned earlier, Clement of Alexandria witnessed processions of the Egyptian

priests and priestesses, bearing the forty-two sacred books of Hermes, around the year 200. It was only when Christianity came to dominate in the fourth century that native Egyptian cults were ruthlessly persecuted and ultimately banned on pain of death. Bruno's interpretation of the Lament required no uncanny knowledge on his part, since Christian writings of the time recorded the suppression of Egypt's 'demonic' pagan cults with characteristic glee.

What excited and motivated Bruno most, however, was his conviction that the second part of Hermes' prediction – the restoration of the Egyptian religion and the return of the gods – would take place during his own lifetime. He interpreted the religious wars that were ripping Europe apart as the death throes of the faith that had suppressed the Hermetic religion. He also believed that Christianity was an offshoot of something much bigger and more ancient, despite it

mistaking itself as the main event. Bruno did, however, admire the way of life Jesus taught, particularly the simplicity of the injunction to treat others as you yourself wish to be treated. (He seems to have regarded Jesus's mission as an attempt to take the Jewish religion back to its Egyptian roots, which our own research indicates to be at least partly correct.)<sup>4</sup> In a statement made to the Inquisition at the time of his arrest, Bruno is reported to have said, 'the Catholic religion pleases him more than any other, but that this too has need of great reform'.<sup>5</sup> He particularly deplored the way the Catholic Church sought to impose itself through 'punishment and pain'; using force rather than love to keep its worshippers was a sure sign that something was terribly wrong.

Yet even at its best, Bruno viewed Christianity as only an also-ran in the great race towards enlightenment and salvation. The ancient Hermetic religion of Egypt would

soon assert its superior position when it returned to the Earth through the mediation of its greatest prophet, Bruno himself.

Bruno believed that the great religious revolution on Earth would be preceded by upheavals in the heavens, reflecting the Hermetic principle (from the *Emerald Tablet*) of 'as below, so above/as above, so below'. Bruno moreover suggested an intriguing variation on this theme, namely that any changes would be echoed in a shift in mankind's *perception* of the heavens. And this, he believed, unlocked the true significance of the heliocentric theory.

For centuries the most learned of men had simply got cosmology wrong; Copernicus had shown that. But the Hermetic books, which Bruno believed preserved the most ancient wisdom of all, also stated that the sun was at the centre of all that mattered and that the Earth moved around it. Copernicus – who also invoked Hermes Trismegistus –



had restored the correct perception of the order of the cosmos. Bruno thought Copernicus had proved mathematically what Hermeticists already knew but had never been able to prove. At the very least, he reasoned, establishing that the Hermetic philosophy contained demonstrable truths about the cosmos would surely win it more converts.

But Bruno also believed that Copernicus' work went way beyond vindicating the Hermetic treatises; he considered it as the key to the prophesied new Hermetic age. The fact that Copernicus had presented his proofs when he did was a portent of the coming changes. But not everybody had yet accepted the new system; it was still being hotly debated. If it could be established beyond doubt and enter into the canon of accepted fact, Bruno thought, then this would literally trigger the new age of Hermetic enlightenment. In turn, this would reveal a new way of comprehending the mysteries of creation,

that is by using the intellect to obtain otherwise elusive proof of certain Hermetic magical and philosophical concepts, as summarized by Frances Yates:

The marvellous magical religion of the Egyptians will return, their moral laws will replace the chaos of the present age, the prophecy of the Lament will be fulfilled, and the sign in heaven proclaiming the return of the Egyptian light to dispel the present darkness was ... the Coper-

nican sun.  
6

Ironically events showed that Bruno was at least half right. Establishing heliocentricity did indeed lead to a revolution that would change academic attitudes to religion, but it was the scientific revolution. The crucial Hermetic philosophy was simply lost along the way. Another cause to Lament.

# THE MISSION

It was no accident that Bruno decided to begin his mission in Paris. The city was the perfect place given that the centre of the Renaissance had shifted to France as the sixteenth century unfolded (neatly symbolized by Leonardo da Vinci's own move to France at the invitation of the king in 1510).

This shift was a consequence of the Catholic Church's attempts to reverse the damage of the Protestant Reformation, through their Counter-Reformation. This was kicked off by the Council of Trent, initiated by the Pope in 1545 – and which continued for eighteen years – to tighten up and rigidly define Catholic doctrine and practices. One result of the Council was that the Church came to assert greater control over the arts, which included, for example, the banning of non-Christian,

and especially pagan, imagery in paintings and sculpture. (No more depictions of Isis and Hermes by popes. Amazingly those in the Appartamento Borgia were allowed to remain.) These prohibitions bit more deeply in Italy than in France, where the Church's real power over French daily life might best be summed up by the timeless Gallic shrug. As the cultural centre of the Renaissance had relocated to Paris, it also became a great centre of Hermeticism, even among Catholic scholars and intellectuals. Both developments owed much to the sophisticated thirst for knowledge of the French court.

Although of course outwardly Catholic, King Henri III of France was a devotee of the occult philosophy. The celebrated poet and chronicler Agrippa d'Aubigné recorded how after swearing him to silence, Henri had revealed a collection of magical treatises he had had brought in from Spain. In this he was only maintaining the family tradition,

since his mother was Catherine de' Medici, the great-granddaughter of Lorenzo the Magnificent. Then in her sixties, she still exerted a powerful influence of her own in Paris. Very much a de' Medici, like her ancestor the great Cosimo, Catherine was a renowned patron not just of the arts but also of astrologers and magicians. So it was hardly surprising that Henri III, the third of her sons to reign in France, shared her arcane interests.

But Henri was, from Bruno's perspective at least, also ideally positioned in Europe's power politics, in which a major conflict between the Catholic and Protestant nations was looming. Henri had a relaxed attitude to Protestantism, both at home and abroad, and as a result of anxiety about the strength of the major Catholic power of Spain he favoured closer ties with Protestant England, Spain's great enemy. Many, not just Bruno, saw Henri as Europe's best hope for a peaceful and tolerant future. As a powerful

Catholic monarch with a zealous interest in magic and the Hermetica and no animosity towards Protestants, Bruno considered Henri the ideal leader of his Hermetic revolution. There are indications in other books published in Paris at that time, and in plays being performed in the king's honour such as the *Ballet comique de la Reine* (the first ballet, staged for the court of Catherine de' Medici in 1581) that Bruno was not alone in this view of Henri.

Meanwhile a well-established circle of expatriate Italians who had settled in Paris because of their heterodox ideas (probably because of the Medici influence) welcomed Bruno with open arms. More significantly, these Italians had some influence over the king. But lurking behind the Franco-Italian circle was, inevitably, an *eminence grise*, a secret adviser and friend of the greatest movers and shakers of the time. This shadowy force-to-be-reckoned with was one

Gian Vincenzo Pinelli of Padua (1535–1601), a scholar and collector (primarily a botanist but his interests were truly Renaissance in scope and depth) best remembered today as Galileo's mentor. Pinelli had built up a pan-European network of correspondents and informants who reported to him on not just scientific and cultural issues but also political events. Unsurprisingly, he therefore showed great interest in Bruno's arrival in Paris and they are likely to have met when Bruno visited Padua during his wanderings.

After the larger-than-life Hermeticist arrived in the French capital in 1581, he gave public lectures and published two books on the magical art of memory. Bruno soon attracted the attention of the King, and having cannily dedicated the first of his books, *On the Shadows of Ideas* (*De umbris idearum*) to Henri, was duly summoned for a royal audience. As a reward he was given a paid lectureship at one of Paris' colleges. His next

move was more surprising: in the spring of 1583 he left Paris for London, where he was to spend more than two years and produce his most important work. The English ambassador in Paris sent a report to Queen Elizabeth's spymaster, Francis Walsingham, advising him of the impending arrival of Bruno, 'whose religion I cannot commend'.<sup>7</sup> With a nice ironic edge Bruno described himself to the Oxford scholars as a 'doctor of a more abstruse theology'.<sup>8</sup> Well, yes. That's one description of it.

Although he had no official diplomatic standing, Bruno was clearly on some kind of unofficial, or semi-official, mission to England. Travelling with letters of introduction from Henri, he lived in the house of the French ambassador, Michel de Castelnau, Sieur de Mauvissière. Because he kept such close company with Castelnau – even accompanying him regularly to Queen Elizabeth's court – and Castelnau was in turn happy to



be known as an associate of Bruno, it fostered the impression that the latter had the French king's backing. And it seems Henri had no problem with that.

As to the purpose of Bruno's mission, it fitted perfectly his agenda of uniting Christianity and averting a catastrophic war in Europe. The idea was to get the Catholic nations to band together under a single monarch and the Protestant nations to unite under another, both of whom would be advised and influenced by Hermeticists who would ensure peace between them. Henri III and Elizabeth I were prime candidates.

English esoteric circles, too, had great influence at the royal court, most obviously in the shape of John Dee (1527– 1608), Elizabeth's astrologer and adviser in many areas, including diplomacy, espionage and the expansion of English influence across the globe. Although there is no record of Bruno and Dee meeting, because they had mutual

friends and frequented the same court and intellectual circles they almost certainly did. This was especially likely as Dee was not only a champion of the Copernican theory but also a passionate devotee of the Hermetic tradition.

Bruno met the Queen herself on the many occasions he accompanied Castelnau to court, declaring himself a fervent admirer of the 'diva Elizabetta' and proclaiming her superior to any man in her heroism, learning and wisdom.<sup>9</sup> That fulsome compliment 'diva' was to count against him with the Inquisition, since they took against calling a declared heretic 'divine'. Worse by far, Elizabeth was an illegitimate heretic in Catholic eyes at least. In any case she was female. And she had a sure sign of the witch, being red-haired. But Bruno enthusiastically joined the cult of the Virgin Queen, which lauded her as the potential spearhead of a new age, the jewelled goddess who would unify Protestant

Europe. He seems to have admired the relatively peaceful nature of Elizabethan England when compared to the internal divisions that were then tearing apart the other nations of Europe.

The uncompromising Neapolitan took part in a famous debate with the scholars at Oxford, in front of the Polish prince Albert Laski and the eminent courtier and poet Sir Philip Sidney, in which he endorsed Copernicus' ideas and linked them to magical concepts about the sun derived from Marsilio Ficino's work.

It was in England that Bruno wrote some of his most important books. Of these, all apart from the first were penned in Italian rather than the customary Latin. But why go to London to publish books in Italian? Of the few Londoners who could read in the first place, how many could read Italian? Presumably Bruno's books targeted Italians in London and Paris, a readership who would then

take his ideas back to their homeland. Or perhaps Bruno had intended that the books be shipped over to Italy? Either way, they were circulating there within a few years, as we will see.

The first – and only Latin – work he published in his first year in London was *Explanation of the Thirty Seals* (*Explicatio triginta sigillorum*), a book about the magical memory system that culminates in an essay about the Hermetic vision. In this, Bruno lists Moses and Jesus as among those who had achieved enlightenment through this means. The latter is portrayed not as the Son of God, or even as a divinely appointed prophet, but as a gifted and advanced magus, a practitioner of the same art so beloved by Bruno. This is an interesting concept – the founder of the religion that saw Bruno's work as heretical practising the same heresies himself – but one that is not without

some foundation, as we have discussed elsewhere.<sup>[10](#)</sup>

In 1584 Bruno published two key works, both of which relate to Copernicus and heliocentricity. The first was *The Ash Wednesday Supper* (*Cena de le ceneri*), a dialogue between a group of scholars as they journey around London. In this book Bruno praises Copernicus, although he also claims that even Copernicus never came to understand the full importance of his discoveries. With his usual bravura, Bruno also declares himself to be Copernicus' heir and states his intention to use his revelations to free the human spirit.

The second book was *Expulsion of the Triumphant Beast* (*Spaccio della bestia trionfante*) a 'glorification of the magical religion of the Egyptians',<sup>[11](#)</sup> an unequivocal declaration of the need for its return in order to restore balance to the world. He links this to

the Lament in *Asclepius*, which he reproduces in full.

The drama of the *Triumphant Beast* takes the form of a gathering of Greek and Egyptian deities to reform the heavens, changing the constellations in order to produce a similar shift on Earth. This is modelled on the Hermetic treatise *The Virgin of the World* (*Korè Kosmou*) in which Isis describes a similar council of the gods to her son Horus. She also features, alongside Sophia, in Bruno's work. The 'triumphant beast' is, according to Bruno's dedication to Sir Philip Sidney, the sum of all the vices that prevent human beings from activating their divine potential. However, some – including, fatally, the Inquisition – interpreted it as a veiled reference to the Pope. A political subtext runs through the *Triumphant Beast*, as it ends with the council of the gods praising the great virtues, pureness of heart and

magnanimity of Henri III, and his fitness to preside over a spiritually unified Europe.

Another significant work Bruno wrote and published in London in 1585, also dedicated to Sidney, was *On the Heroic Frenzies* (*De gli eroici furori*). Ostensibly a collection of love poems, it soon becomes clear that the ‘frenzy’ of passionate love is a way of attaining the Hermetic gnosis. This concept is taken from Agrippa (in turn a development from Ficino), who wrote of four types of *furor* that enable the soul to reconnect with the divine: poetic inspiration, religion, prophecy and love, the *furor* of Venus. Of the last, Agrippa writes that it ‘transmutes the spirit of a man into a god by the ardour of love, and renders him entirely like God, as the true image of God’<sup>12</sup> before proceeding to cite Hermes Trismegistus, from *Ascepius*, as an authority for this idea. This is obviously why the idea was so attractive to Bruno.

The concept of erotic love as a portal to Hermetic illumination links Bruno with other well-established traditions of sacred sexuality, including sex magic and tantrism. For someone who elevated what we would now call the sacred feminine, and who admired intelligent and able women, it is curious that nothing in the historical records specifically links him with any women. Or man for that matter: if Bruno had even been remotely rumoured to be gay this would have featured in the Inquisitions list of his calumnies. As it is, the Inquisition records only suggest that he was a womanizer, without any actual proof.

Bruno wrote in his dedication to Sir Philip Sidney that, although he hadn't had as many lovers as Solomon, it wasn't for the lack of effort on his part:

I have never had a desire to become a eunuch. On the contrary I should be ashamed if I agree to yield on that score were it only a hair to any man worth his salt in order to serve nature

and God. <sup>13</sup>



Only one source links Bruno, if only obliquely, with affairs of the heart. Several historians have suggested that the character of Berowne, the leader of the poets at the court of the King of Navarre in Shakespeare's romantic comedy *Love's Labour's Lost* is based on Bruno. The identification is highlighted, as Yates has shown, by the fact that some of Berowne's speeches, particularly his great paean in praise of Love in Act IV ('For valour, is not Love a Hercules ...'), contain specific parallels to *Expulsion of the Triumphant Beast*, the greatest of the works Bruno wrote in England, about ten years before Shakespeare penned the play.

*Love's Labours Lost* is not one of Shakespeare's most popular works because of its abstruse and often tedious wordplay. The plot describes the oath taken by the King of Navarre and three of his scholars, led by Berowne, in order to concentrate on their

pursuit of knowledge, which entails living an abstemious life for three years, including forswearing the company of women. But the arrival of the Princess of France and a bevy of young ladies-in-waiting throws several cats among the pigeons, with predictably hilarious(-ish) consequences. Other than the lesson that locking oneself away in the pursuit of knowledge is a bad idea – wisdom comes from participating in the real world – there seems little message in this typically mannered Elizabethan romantic comedy. Most of the jokes have never been found funny since doublet and hose went out of fashion.

But there is a bit of a mystery surrounding *Love's Labours Lost*. The play has no proper ending – all of the characters simply disperse with a promise to meet up again in a year's time. There are also a couple of contemporary references to an otherwise unknown sequel by Shakespeare called *Love's*

*Labour's Won*, but for some reason this has been omitted from the Shakespeare canon that passed into history. One clue, however, lies in the fact that at the time the play was written the King of Navarre and the King of France were one and the same, and he was being supported by Bruno and other Hermeticists – as we will see.

(However, at least one good thing came out of this little literary mystery. It inspired the 2007 *Dr Who* story ‘The Shakespeare Code’, in which David Tennant’s Time Lord discovered that the now-lost *Love’s Labour’s Won* contained coded magical utterances that were set to open a portal to another dimension.)

# THE INFINITE UNIVERSE

In addition to his zeal for Hermetic reformation, Bruno was unquestionably one of the greatest intellects of his time, and was especially admired for his scientific and mathematical ideas and theories. Several studies have been devoted to this side of him, including Paul-Henri Michel's *The Cosmology of Giordano Bruno* (1962), Dorothea Waley Singer's *Giordano Bruno: His Life and Thought* (1950) and Hungarian academic Ksenija Atanasijevic's *The Metaphysical and Geometrical Doctrine of Bruno* (1923). Atanasijevic describes him as 'certainly the greatest philosopher of the XVIth century',<sup>14</sup> and writes:

If the Inquisition had not managed to put its jackal's claws upon him when he was forty-four and if he had not been burnt alive at the age of fifty-two, Bruno would have left to humanity

some more of his inspired and farsighted conceptions.<sup>15</sup>

Many of his pronouncements – all derived from the essential principles in the *Hermetica* – were staggeringly ahead of their time.

Clearly in a fever of composition, while still in London in 1584, Bruno published another remarkable work: *On the Infinite Universe and Worlds* (*De l'infinito universo e mondi*), in which he proposed two ideas that went way beyond even those of Copernicus. The first was that all creation was not contained within the space bounded by the sphere of the fixed stars, but was infinite. The second was that the stars are not small bodies of light fixed on that sphere but are actually suns like our own, only immensely far away, at different distances in the infinite universe. Bruno made a further extrapolation: if the stars are suns, then they too are circled by planets. He wrote:

For there is a single general space, a single vast immensity which we may freely call *Void*; in it are innumerable and infinite globes like this on which we live and grow. This space we declare to be infinite, since neither reason, convenience, possibility, sense-perception or nature

assign to it a limit. In it are an infinity of worlds of the same kind as our own ... Beyond the  
imaginary convex circumference of the universe is Time. <sup>16</sup>

The last sentence is strangely prescient of the curvature of space-time that is regarded as one of Einstein's greatest insights.

Not only did Bruno think there were other planets, but also that some were inhabited. *On the Infinite Universe and Worlds* takes the form of a dialogue between two characters, Fracastoro and Burchio. At one point, the latter asks whether the other worlds are inhabited like ours, to which Fracastoro replies:

If not exactly as our own, and if not more nobly, at least no less inhabited and no less nobly. For it is impossible that a rational being fairly vigilant, can imagine that these innumerable worlds, manifest as like to our own or even more magnificent, should be destitute of similar  
or even superior inhabitants. <sup>17</sup>

Ideas such as the one expressed by Fracastoro are so extraordinarily modern that it is difficult to appreciate just how big a

conceptual leap they were at the time – and just how shocking they could seem.

Even Copernicus had maintained the conventional idea of a fixed sphere of stars. As such, shifting the centre from the Earth to the sun made relatively little difference to established views of mankind's special place in creation. Even though the Earth was no longer the centre of everything, the sun is, making mankind *almost* the focus of creation. And according to Copernicus there was still only one relatively small, finite cosmos, in which existed a singular world in which God had created living things: a cosmos made just for us.

But if there are other suns, with their own inhabited planets, then the unique specialness of this world and of humanity is called into question. Since an infinite universe can have no centre, neither the world, nor even the sun, could claim to fill this role. In this theory of the world, humankind is shifted

further from the centre of things – and from being the focus of God's creation.

Modern science, which emphasizes the insignificance of both humanity and the Earth in cosmic terms, credits Copernicus with beginning the shift in perception from humanity being the centre of everything to our inhabiting a tiny part of an infinite universe. However, the credit should really belong to Bruno, since it was his notion of an infinite universe that provided the truly radical leap.

There was one major and insurmountable difference between the modern view and Bruno's. He would never have accepted the twenty-first century reasoning that, because the universe is infinite and we are not alone in it, human beings are therefore unimportant. He believed that the universe teems with life, including us, because it was *made* for life.



Another major difference between Copernicus' and Bruno's cosmologies was that Bruno's unequivocally clashed head-on with Christian teaching, flatly contradicting the biblical story that God created the sun, moon and stars after making the Earth, with no mention of other earths. One of the heretical ideas for which Bruno was executed was that of an infinite, inhabited universe. So what was the source of his radical ideas?

In fact, Bruno derived the notion of an infinite universe from a passage in *Asclepius*, in which Hermes refers to a region 'beyond heaven', which implies that the heavens are not bound by the sphere of the fixed stars.<sup>[18](#)</sup> Although this suggests an infinite universe, it does not state that it is full of suns. The idea therefore seems to have been Bruno's own extrapolation.

As we noted in the last chapter, at least one thinker had challenged the 'celestial sphere' concept and argued for infinite

space. This individual was Englishman Thomas Digges, ‘the first Copernican in England’,<sup>19</sup> whose ideas Shakespeare alluded to in the ‘nutshell’ line in *Hamlet*. Digges made the proposal in 1576 in his outline of Copernican theory – the first published in England – *A Perfit Description of the Caelestiall Orbes*. Given that Bruno wrote his work in England, it could be that he was influenced or inspired by Digges.

But Digges, too, was part of the Elizabethan esoteric scene, being a protégé of John Dee, himself a great supporter of heliocentricity. Although Dee left no reference to the theory in his own works, he encouraged its first champions in England, urging the astronomer John Field to use Copernicus’ system to draw up a table of the positions of the planets in 1557. Dee was also, notably, Digges’ mathematics tutor (Digges called Dee his ‘second mathematical father’).<sup>20</sup> In

fact, Digges' version comes straight from *Asclepius*.<sup>21</sup>

These were not the only anticipations of modern scientific thinking and discoveries in Bruno's work. In fact, some of his ahead-of-their-time pronouncements become positively eerie. In *On the Infinity of the Universe and Worlds* he writes:

Thus soul and intelligence persist while the body is ever changing and renewed part by part ... for we suffer a perpetual transmutation, whereby we receive a perpetual flow of fresh atoms and those that we have received are ever leaving us.<sup>22</sup>

As we now know, every cell in our bodies is constantly being replaced throughout successive cycles of seven to ten years. But how did Bruno know? And that is by no means the limit of Bruno's prescience. Peter Tompkins writes:

The doctrine of evolution, the progressive development of nature, an idea unknown to classical philosophy, was first pronounced by Bruno, not vaguely or partially; he extended its laws to the inorganic as well as the organic world, maintaining that unbroken line of evolution from matter to man which only modern science later began to recognize.<sup>23</sup>

Bruno heavily influenced the English natural philosopher and physician William Gilbert (1544–1603) who British science writer John Gribbin describes as ‘the first person to set out clearly in print the essence of the scientific method – the testing of hypotheses by rigorous experiments – and to put that method into action.’<sup>24</sup>

Gilbert’s major work, *On the Magnet, Magnetic Bodies, and the Great Magnet of the Earth* (*De magnete, magneticisque corporibus, et de magno magnete tellure*), published in 1600, was one of the landmarks of the scientific revolution, presenting his theory that the reason magnets, or loadstones, work is because the Earth itself is a magnet. Historian Hilary Gatti, author of a study of Bruno’s legacy to England following his visit, demonstrates that in his ideas about the Earth’s magnetism, Gilbert built on Bruno’s cosmology.<sup>25</sup>

A collection of Gilbert's papers published half a century after his death, *A New Philosophy of Our Sublunar World* (*De mundo nostro sublunari philosophia nova*), makes his debt to Bruno very clear.<sup>26</sup> The two men almost certainly met, as Gilbert was physician to Elizabeth I at the time that the Neopolitan was a frequent visitor to her court.

Another royal physician who made an indelible mark on the history of science was William Harvey, who as Charles I's physician in 1628 famously demonstrated the circulation of the blood – 'one of the greatest achievements of the Scientific Revolution'.<sup>27</sup> However, as Harvey acknowledged, his inspiration came from the work of one of his colleagues, the Hermeticist Robert Fludd (who we will meet in a later chapter), who had proposed the idea based on Hermetic principles. Fludd's own inspiration was almost certainly his esoteric hero Bruno, who had put forward the same thing for the same

reasons nearly half a century earlier.<sup>28</sup> Once again, he deduced this from the *Hermetica*, specifically its association of the spirit that moves through the body with the blood; Treatise X of the *Corpus Hermeticum* explicitly states ‘the spirit, passing through veins and arteries and blood, moves the living thing’.<sup>29</sup> And so another major scientific discovery can be attributed to Hermes Trismegistus – and to Bruno.

His influence was, indeed, vast. As Ksenija Atanasijevic writes:

But Bruno’s contribution to the development of subsequent philosophy and modern astronomy is beyond proper evaluation not only in terms of his conception of the infinity of the universe; with his comprehensively conceived and elaborately argued doctrine of the triple minimum he is also one of the leading forerunners of later monadology, atomism and the

teachings about the discontinuity of space, time, motion and geometrical bodies.<sup>30</sup>

Atanasijevic concludes that ‘it was Bruno who laid the firm foundations upon which was to rise, in the course of time, the ... edifice of new atomic science’.<sup>31</sup> But although Bruno’s ideas were in many respects far

closer to the modern scientific mindset than the works of Copernicus and Galileo, they sprung from his immersion in the ancient philosophy of Hermeticism.

# THE GIORDANISTI

Bruno returned to Paris with Castelnau in the autumn of 1585, being attacked by pirates as they crossed the Channel – much like Rosencrantz and Guildenstern in *Hamlet*. Things were fraught in Paris: a group of ultra-Catholic French nobles had formed the Catholic League, which aimed to oust Henri III and wipe out the French Protestants – the Huguenots – and form an alliance between France and Spain. Henri had been forced to make a number of concessions such as rescinding liberties he had granted to the Huguenots, in order to avoid civil war. Henri had no heir and France was simmering with tension as sides were being taken over who would succeed him.

Somewhat surprisingly, in Paris Bruno made overtures to the papal nuncio about



returning to the Catholic Church and receiving absolution, although he was spurned. This seems incongruous, but Yates explains that Bruno had become convinced that the great Hermetic reformation would happen *within* the Catholic Church, so that was the place to be. As she wrote, "The new dispensation was to be an Egyptianized and tolerant Catholic and universal religion, reformed in its magic and reformed in its ethics."<sup>32</sup>

However, it soon became apparent that this rather unrealistic hope was doomed, with political events in France taking a turn for the worse for Bruno's programme of reform. He left Paris in the late summer of 1586, shortly before the Catholic League took control of the city. Adapting himself to the new situation, Bruno shifted his focus to the Protestant lands, and toured Germany for the next few years. Initially he obtained a post as lecturer at the University of Wittenberg in Saxony (which had produced Martin

Luther, not to mention the fictitious Hamlet). Bruno owed his job to the influence of another important Oxford contact, Professor of Law Alberico Gentili, an Italian refugee whose family had fled abroad because of their Protestant beliefs. Gentili is remembered today as the founder of international law.

After a couple of years at Wittenberg, Bruno moved on briefly to the Prague court of the Holy Roman Emperor Rudolph II. Despite his leading role in the great Catholic dynasty of the Habsburgs, Rudolph (1552–1612) was extraordinarily liberal-minded. Not only was he renowned for his patronage of the arts and learning but he was also an active and enthusiastic sponsor of the occult sciences, particularly alchemy. Rudolph employed Tycho Brahe as his Imperial Mathematician, who was himself succeeded by his assistant, Johannes Kepler. Shortly before Bruno arrived at his court, the

great Dr Dee had been a distinguished guest of the Emperor.

Rudolph never shared his dynasty's political or religious interests, and focused instead on his own enlightened pursuits. He moved the imperial court from Vienna to Prague in Bohemia, which under his patronage became a sparkling Renaissance city, where all learning and culture was encouraged. In Prague, Protestants and – extraordinarily for the time – Jews were free to practise their religion. Rudolph also worked for a unified Christian Europe, backing those who worked for tolerance and reconciliation between Catholic and Protestant. His own religious orientation is unclear. Although raised a Catholic, he was obviously lapsed, going so far as to refuse the last rites on his deathbed. But neither did he join any of the Protestant churches.

Rudolph acted like a magnet for occultists, artists and scholars, and Bruno was no

exception. But to Bruno an added attraction must have been the existence of a court of exceptional tolerance and open-mindedness. Having received some financial assistance from the Emperor, Bruno moved swiftly on to the University of Brunswick, all the while in a ferment of thinking and plotting.

Throughout his wandering years, Bruno's position on the Catholic Church and the nature of the Hermetic revolution shifted. Until his departure from Paris, he believed that an Egyptian reformation could begin within the Church, through collaboration between Hermes-friendly monarchs such as Henri III and allies in Rome itself. But not only was Henri losing the civil war against the Catholic League, he was soon to be assassinated by one of their agents, a Dominican monk. (Catherine de' Medici also died – surprisingly of apparently natural causes – at the beginning of that year.) Spain was bringing its whole might to bear on crushing

Bruno's next best hope for harmony in Europe, Elizabeth's England, building up the armada for the attack of 1588; few gave England much of a chance.

At this time, when Catholicism seemed on the brink of triumph, a strangely symbolic event took place in Rome. In 1586 a great ancient Egyptian obelisk that had remained neglected for over a thousand years was moved to the centre of St Peter's Square. During the Roman Empire, many obelisks and statues were carried off to the imperial hub from Egypt and erected around the city, usually in honour of some emperor or another. Unsurprisingly, they had been knocked over and vandalized as nasty pagan monuments when Christianity became the state religion, but many were left where they fell, either in pieces or whole, to disappear beneath the ground over the centuries. In the sixteenth century only one obelisk was still standing, albeit with its base deeply buried,

in a dingy alley behind St Peter's. Nearly three thousand years old, it had been taken to Rome on the orders of Caligula.

In 1586 Pope Sixtus V ordered that the obelisk be moved to its prominent place and following a monumental engineering effort that stretched the resources and skills of the day to their very limit, this 83-foot-tall (25-metre), 350-ton monument stood tall in the centre of the square. After being duly exorcised, it was topped with a large iron cross and had inscriptions honouring Christ (and of course Sixtus) carved into it.

Sixtus' declared motive was to assert the triumph of Catholic Christianity over paganism and to 'eradicate the memory of the superstitions of antiquity by raising the greatest footing ever for the Holy Cross'.<sup>33</sup> At first glance, this seems rather strange, since Christianity had put an end to paganism long before and the major threat to Catholicism at the time was Protestantism. But in the

context of the Hermetic, Egyptian undercurrent the desire of this ultra-conservative and reactionary ex-Inquisitor – of whom it was said that he wouldn't even forgive Christ of his sins – to symbolize his Church's superiority over Egypt certainly makes sense.

For his part, Bruno became much more confrontational, publicly denouncing the Catholic Church and the Pope as both tyrannical and the cause of disorder and violence in Europe. He also changed strategy and decided that the Hermetic revolution would now be brought about by stealth, using more clandestine methods. He devoted much of his time in Germany to organizing a secret society, the Giordanisti, to further his ambitions. This underground network would act as contingency should there be a Catholic take-over of Europe, which seemed only too likely. The Giordanisti were effectively a Hermetic resistance movement. One fellow guest

of the Inquisition in Rome said that Bruno had declared:

... that he had begun a new sect in Germany, and if he could get out of prison he would return there to organize it better, and that he wished that they would call themselves Giord-

anisti.<sup>34</sup>

The chief informer against him, Zuan Mocenigo, said that shortly before his arrest Bruno had ‘revealed a plan of founding a new sect’ to him.<sup>35</sup> Although this revelation suggests that Bruno was still at the initial planning stages, his activities just before returning to Italy suggest otherwise. In retrospect it seems improbable that such a messianic figurehead would *not* have organized cells of disciples wherever he went, linking them into an underground network. Forming secret groups is what Hermeticists do.

Bruno had certainly acquired disciples and devotees in France and England. During his return to Paris he published works under his followers’ names in order to disguise his



authorship – although this may not have been favourable for those whose names he adopted – another sign that he was becoming more cautious and secretive. He was now building a following in the states of Germany. And despite restrictions caused by the problems of transport, because the formal organization was university-based, there would have been a constant movement of professors and students to other parts of Europe, all carrying Bruno's message.

Part of Bruno's new project involved the publication, in 1590 and 1591, of three lengthy poems expounding his magical philosophy, the progress of which he controlled more meticulously than any of his more overtly arcane and philosophical works. He even travelled to Frankfurt to oversee their production. One of the poems, *On the Threefold Minimum and Measure* (*De triplici minimo et mensura*) included

symbols and diagrams for which – uniquely – Bruno made the woodcuts himself.

It has been suggested that Bruno lavished all this love on this particular work because it incorporated the Giordanisti's secret symbols and contained ciphered messages for its initiates.<sup>36</sup> Again, this makes sense in terms of a feared Catholic clampdown, in which his overtly Hermetic treatises would be banned. Of all Bruno's works this was the one that was ultimately responsible for his downfall.

Being such a high-profile possessor of Hermetic secrets was never going to be a passport to freedom of speech and a guarantee of personal safety, but clearly something in Bruno's character either persuaded him he would always lead a charmed life or he simply craved danger. Perhaps he also craved martyrdom.

A fiery fate was already waiting in the wings. While in Frankfurt, Bruno met

Giovanni Battista Ciotto, an innocent-seeming book dealer from Venice. Back home, Ciotto sold a copy of Bruno's poem *On the Threefold Minimum and Measure* to a wealthy Hermetic dabbler, Zuan Mocenigo, which prompted him to invite Bruno to be his guest and teacher. At the age of forty-three, and after ten years away from Italian soil, Bruno accepted the offer. This would not turn out to be his best idea.

To modern eyes it seems as if Bruno was somewhat overoptimistic, seeing his return to Italy as a golden opportunity to inveigle himself into the Pope's favour. He even wrote to an old Dominican acquaintance in Venice that he hoped to receive papal absolution. Certainly further political change had rekindled his hopes of establishing a new age of Hermetic religion through an internal transformation of the Catholic Church. He still envisaged a French monarch who would bridge the divide between Catholics and

Protestants, but fate would ultimately act against him there, too.

In the struggle over the succession that had followed the assassination of Henri III, another Henri, the King of Navarre, had triumphed (with the aid of English soldiers sent by Elizabeth). Navarre was a kingdom in southern France, on the Atlantic coast, the remnant of a larger and once predominantly Spanish kingdom that had straddled the Pyrenees. In 1589 the Huguenot king of Navarre also became King of France. In a politically expedient move, the new Henri IV converted to Catholicism, but as an ex-Huguenot it was widely anticipated he would unify the religious divide in France. Curiously and probably not coincidentally, he had his marriage annulled and married a Medici, Marie, daughter of Francesco de'Medici. Hermetic hopes once centred on Henri III now segued onto Henri IV. Bruno went so far as to tell his Inquisitors that he hoped that the new

king would 'confirm the orders of the late King' (Henri III) for the favours granted to him.<sup>37</sup>

Bruno's sense of destiny had also been bolstered by other events, and without the grim knowledge provided by hindsight, perhaps it is easy to understand how he might have been so tragically misled. In 1591, Francesco Patrizi, Professor of Philosophy at the University of Ferrara, published a new edition of the *Hermetica*. In his dedication to Pope Gregory XIV, Patrizi urged him to decree that Hermetic philosophy be incorporated into the heart of Catholic education. Gregory died soon afterwards, but his successor, Clement VIII, rewarded Patrizi for his efforts by bestowing him with the Chair in Platonic Philosophy at the University of Rome. Bruno told Mocenigo that he had taken heart from this, and expected the same kind of treatment from Clement. There was, however, a major difference. Patrizi was

advocating the incorporation of Hermeticism into Catholicism, not vice versa like Bruno. And, of course, while ostensibly rewarding Patrizi – or perhaps buying him off – Clement never actually acted on his proposition.

It was in this climate that Bruno accepted Mocenigo's suggestion to travel to Venice. Accompanied by his secretary, Jerome Besler, Bruno initially declined Mocenigo's invitation of hospitality, and stayed in his own lodgings. He gave talks at Ciotto's bookshop and frequented intellectual salons in private homes, besides spending three months at Padua, hometown of the *eminence grise* Gian Vincenzo Pinelli, whom he undoubtedly met. Only in the spring of 1592 did he finally give in and agree to stay with Mocenigo. During his two-month visit his host made notes of their conversations, which no doubt seemed innocent enough, perhaps even flattering at the time, but they were to provide the basis of the case against him.

There were other good reasons why Bruno and his network wanted to shift their focus to Venice. The republic was becoming a centre of opposition to the Pope's authority and there were moves to forge a political and religious alliance with England (although this only gathered momentum in the years after Bruno's death). Astonishingly there were even hopes that Venice might adopt Anglicanism, which probably explains why the Pope excommunicated the whole republic in 1606. The key figures in this plan were all associated with Bruno. They included the English ambassador (and former spy) Sir Henry Wotton, who had been at the Italian's controversial lecture in Oxford and was a great friend of Alberic Gentilio, the professor of law who facilitated Bruno's career in Germany. Another was Traiano Boccalini, author of *News From Parnassus* (*Ragguagli di Parnaso*), which, modelled on *The Expulsion of the Triumphant Beast*, called for a

‘general reformation of the whole wide world’.<sup>38</sup>

The unravelling of events such as these in Venice and Padua (part of the republic of Venice) in the aftermath of Bruno’s visit was unlikely to have been coincidental. Neither was it much of a coincidence that Padua appears to have become a sudden magnet for Hermeticists when Bruno left.

And then, suddenly, it all became too obvious. In May 1592, when Bruno was preparing to return to Frankfurt, Mocenigo refused to let him leave, hiring a gang of gondoliers to lock him in a room, and sent for the Inquisition. Bruno was to be their prisoner for the remaining eight years of his life, with the resulting agonizing ending usually reserved for those who spoke out against ignorance and tyranny.

No evidence remains to suggest why Mocenigo decided to play the villain. Some



believe his invitation was a trap from the start, or even that he had been in the pay of the Inquisition from the moment he bought *On the Threefold Minimum and Measure*. Others think that Mocenigo's enthusiasm for Bruno's philosophy was genuine but that he became disillusioned or alarmed. Perhaps Mocenigo simply feared for his immortal soul.

Bruno was questioned by the Inquisition and then tried in Venice. The major concern was the 'great reform' he preached. He did recant his heresies and begged for mercy from the judges, but the Supreme Inquisitor in Rome sent for him. Bruno was kept in prison in Rome for five years without so much as being questioned. After finally being interrogated, he was kept imprisoned for a further three years, without being tried. Heretics who admitted their errors – as Bruno appears to have done – were generally either given a prison sentence or released,

albeit with restricted movements. Those who didn't were tried and, if found guilty, imprisoned or executed. Either way, a prisoner was generally dealt with relatively swiftly. Why the Inquisition dithered over Bruno is a puzzle, although we can offer a possible explanation that relates to the Hermetic undercurrent.

The inexorable endgame for Bruno finally began with the arrival of the newly appointed Cardinal Inquisitor Roberto Bellarmino (1542–1621, canonized in 1930). One of the most formidable intellects of the Church, Bellarmino was a loyal and capable pair of hands trusted by a succession of popes. He was a member of the Society of Jesus – another prong of the Counter-Reformation formed some sixty years earlier. The Society, known commonly as the Jesuits, was and is a notoriously unsentimental brotherhood, zealously committed to the unswerving maintenance of Catholic doctrine.

Bellarmino's speciality was combating heresy, about which he knew a great deal, having taken infinite pains to comprehend the mindsets and arguments of heretics (although in his case, studying the subject was unlikely to see him accused of being suspect of a suspicion of heresy). A fierce and clever polemicist, he even engaged in a pamphlet war with James I of England.

Bellarmino had been an assistant to the papal emissary sent to negotiate with the Catholic League over the successor to Henri III after they assassinated him, negotiations that were trumped by the accession of Henri of Navarre. So he was aware of the Protestant and Hermetic expectations centred on the French kings.

When Pope Clement VIII appointed Bellarmino Cardinal Inquisitor in 1599, he reopened proceedings against Bruno, who asked that he be allowed to write a petition to Pope Clement VIII declaring that he was

prepared to defend the beliefs he was charged with, but that if Clement proclaimed them to be heretical, he would abide by his decision. Bellarmino didn't even show the petition to the Pope. According to the Cardinal Inquisitor, when Bruno was presented with a list of specific heresies in his work he abjured them, but then later withdrew this admission. This, as Bruno must have known, was the worst thing he could have done, as the most severe sentences were reserved for relapsed heretics. It was inevitable that he would be burnt at the stake. So had Bruno really changed his mind? No one will ever know. When he was led out to the pyre, his tongue was tied to prevent him speaking.

The record of the prosecution in Rome was lost after being taken to Paris in 1810 with the papal archives on the orders of Napoleon. However, we discovered from a summary of the Roman Inquisition's evidence found in 1942 (among the personal papers of

the nineteenth-century Pope Pius IX) that Bruno was condemned for holding opinions contrary to the teachings of the Catholic Church, in particular about the Trinity, Jesus' divinity and transubstantiation and speaking out against the Church; denying Mary's virginity; practising magic and divination; and claiming that there were many worlds in an infinite universe, and that the Earth moved. The German scholar Caspar Schoppe, who witnessed the execution, listed the heresies for which Bruno was being burned. These included the belief that there are innumerable other worlds; the promotion of the practice of magic; the claim that the Holy Spirit and the *anima mundi* are one and the same; that Moses learned magic from the Egyptians; and, finally, that Jesus Christ, too, was a magus. Any one of these would have ensured that Bruno be roasted alive – perhaps the Inquisition was furious

Bruno had only one life to lose in the crackling flames.

# MINERVA'S MAN

Bruno was sent to the pyre on 17 February 1600, ironically, or maybe deliberately, the day after Ash Wednesday, recalling the title of his infamous book. First he was taken from prison to the Inquisition's basilica, where he was handed over to the secular authorities (as was the procedure for the execution of heretics). Bruno may have seen the choice of location either as a cruel irony or perhaps as a source of comfort. The basilica is dedicated to Santa Maria sopra Minerva and was built on the foundations of a pagan Roman temple, which we now know was dedicated to Isis. However, when the basilica was built the deity was mistakenly identified as Minerva, the Roman goddess of wisdom and magic (among other things). Ironically, Minerva was the form of the goddess to

whom Bruno had specifically chosen to dedicate himself.

From the basilica, Bruno was led to the Campo de' Fiori (Field of Flowers), then a meadow (and now a market square in the heart of the city), where he was tied to a stake and burnt alive. Schoppe says that he turned his head aside when offered a crucifix to kiss – demonstrating he was a pagan Hermeticist to the very last, and perhaps indicating that his alleged recantation was in fact an Inquisitorial invention. Or perhaps, in the one final moment when he had nothing to lose, he felt that he could reveal his true self.

In 1870, when the city of Rome passed from the control of the Pope to secular authorities, there were immediate calls to erect a statue in Bruno's honour in the Campo de' Fiori. Luminaries such as Herbert Spencer, Victor Hugo and Henrik Ibsen supported the petition. This is probably what prompted the Pope of that time, Pius IX, to call for the



documents on Bruno's trial that were later found in his personal papers. However, it took until 1889 for the bronze statue, showing a rather sinister Bruno in his monk's robes and cowl, to be erected. The statue is today the focus for a variety of pilgrims even though they tend to be atheists, freethinkers and New Agers. But the original driving force behind the statue was Italian Freemasonry – the sculptor, Ettore Ferrari, was Grand Master of Italy and the statue was unveiled with the Campo 'festooned with flags bearing Masonic symbols'.<sup>39</sup>

The nineteenth-century adulation of Bruno was based on a serious misconception, which endured because of the gap in the official records. Many had come to believe that Bruno had been put to death solely for advocating the heliocentric theory or the infinity of worlds, making him a kind of forerunner of Galileo. This belief encouraged what one commentator calls 'a misguided

interpretation of Bruno as a martyr for science'.<sup>40</sup>

Bruno was actually a martyr for Hermeticism. Although there was a connection with the Copernican theory, but Bruno was condemned not for preaching heliocentricity, but because of its special significance to him, particularly his vision that proving it would herald the coming Hermetic age.

Even today, the Catholic Church's attitude to Bruno remains startlingly unchanged. When, in the Holy Year of 2000, a suggestion was made that Pope John Paul II might finally forgive him – as they had Galileo – the official response was that Bruno 'had deviated too far from Christian doctrine to be granted Christian pardon'.<sup>41</sup>

But the question remains: why had it taken eight years for Bruno to be condemned? Why had his teachings suddenly become too hot for the Inquisition?

We suggest that the answer to these questions lies in events of a few months before, in an attempt to establish the Hermetic republic on Earth by force.

## Chapter Two

[1](#) Arianism was an alternative view of the nature of Christ that had been rejected and condemned during the formative years of the Catholic Church in the fourth century. In contrast to what became the Church's official position – that God and Christ were of the same substance and that Christ had co-existed with God from the beginning of time – Arianism held that God had created Christ at a specific moment in time. This made him something more like the Gnostic Demiurge – or Hermes' 'second god' – implying that Christ was distinct from God and that there was a time when he had not existed. The Arian view, contrary to a common misconception, was not that Jesus was a mortal chosen by God.

[2](#) Yates, *Giordano Bruno and the Hermetic Tradition*, p. 11.

[3](#) Copenhaver, p. 83.

- [4](#) See Picknett and Prince, *The Masks of Christ*, pp. 371–81.
- [5](#) Quoted in Yates, *Giordano Bruno and the Hermetic Tradition*, p. 340.
- [6](#) *Ibid.*, p. 215.
- [7](#) Quoted in *ibid.*, *Giordano Bruno and the Hermetic Tradition*, p. 204.
- [8](#) Quoted in *ibid.*, p. 206.
- [9](#) *Ibid.*, p. 288.
- [10](#) See our *The Masks of Christ*, pp. 197–201 and 222–4.
- [11](#) Yates, *Giordano Bruno and the Hermetic Tradition*, p. 211.
- [12](#) Quoted in *ibid.*, pp. 281–2.
- [13](#) Quoted in Tompkins, p. 75.
- [14](#) Atanasijevic, p. xxiii.
- [15](#) *Ibid.*, p. xx.

- [16](#) Singer, *Giordano Bruno*, p. 363. Singer's book includes a full translation of Bruno's *On the Infinite Universe and Worlds*.
- [17](#) *Ibid.*, pp. 322–3.
- [18](#) Copenhaver, p. 83.
- [19](#) Gingerich, p. 23.
- [20](#) Stephen Johnston, 'Like Father, Like Son? John Dee, Thomas Digges and the Identity of the Mathematician', in Clucas (ed.), p. 65.
- [21](#) See Westman and McGuire, p. 24.
- [22](#) Singer, *Giordano Bruno*, p. 285.
- [23](#) Tompkins, p. 83.
- [24](#) Gribbin, p. 3.
- [25](#) Gatti, *Giordano Bruno and Renaissance Science*, pp. 80–5.
- [26](#) Gatti, 'Giordano Bruno's Copernican Diagrams', pp. 43–6.
- [27](#) Debus, 'Robert Fludd and the Circulation of the Blood'.

- [28](#) *Ibid.*
- [29](#) Copenhaver, p. 33.
- [30](#) Atanasijevic, p. xvii.
- [31](#) *Ibid.*, p. xviii.
- [32](#) Yates, *Giordano Bruno and the Hermetic Tradition*, p. 304.
- [33](#) Quoted in Tompkins, p. 23.
- [34](#) Quoted in Yates, *Giordano Bruno and the Hermetic Tradition*, p. 312.
- [35](#) Quoted in *ibid.*, p. 312.
- [36](#) See *ibid.*, pp. 320–1.
- [37](#) *Ibid.*, p. 341.
- [38](#) This is the description given to the extract from Boccalini's work that was included with the first of the Rosicrucian manifestos.
- [39](#) Findlen, 'A Hungry Mind'.
- [40](#) *Ibid.*

## CHAPTER THREE

# GALILEO AND THE CITY OF THE SUN

Bruno's exit from Padua for his fateful stay with Zuan Mocenigo left a space on centre stage for others to move in. This certainly marked a major opportunity for one aspiring scholar. Bruno had applied for the then-vacant chair of mathematics at Padua University, but owing to his untimely arrest the job went to another candidate – none other than Galileo Galilei.<sup>1</sup> Of more immediate significance, however, was the arrival in Padua, just a few months after Bruno's



departure, of a rising star of the Hermetic world who was his spiritual heir.

The similarities between the careers, philosophies and aims of Bruno and Tommaso Campanella (1568–1639) are so striking that they must have been working to the same plan. Indeed, twenty-three-year-old Campanella's arrival in the same circles so soon after Bruno's arrest suggests that he was picking up where the Neapolitan had been forced to leave off. And despite dramatic reversals of fortune, Campanella 'very nearly succeeded in bringing off the project of a magical reform within a Catholic framework, or, at least, in interesting a number of very important people in it'.<sup>2</sup>

Like Bruno, Campanella was born in the Kingdom of Naples, though much further south in the town of Stilo in the Calabria region, in 1568, which made him twenty years Bruno's junior. Also like Bruno, and probably for the same reason of being a bright lad

from humble origins – his father Geronimo was a cobbler – Campanella began his career in the Dominican Order, which he entered at the age of fourteen. After his novitiate he became a friar (a brother who lived in the outside world) rather than a monk like Bruno.

Campanella's own freethinking earned him the suspicion of heresy. In particular, he advocated that knowledge should come from the direct study of natural phenomena (remember the Hermetic motto: 'follow nature'), rather than from officially approved books. Not only was this – to modern eyes perfectly reasonable – approach deemed misguided but actually attributable to the Devil.

One of the major influences on Campanella's thinking was Marsilio Ficino, whose work was probably also responsible for attracting him to Hermeticism. Another esoteric influence was the venerable polymath Giovanni Battista della Porta (c.1535–1615),

author of the classic 1558 treatise *Natural Magic* (*Magiae naturalis*), with whom Campanella struck up a friendship during a two-year stay in the city of Naples in the early 1590s. As with Bruno, Campanella was open to every sort of idea, but Hermeticism was the glue that held them all together and gave all human knowledge a recognizable shape.

Della Porta's influence inspired Campanella to write his first book, which advocated the practice of magic. Although it was only published in 1620, *On the Sense of Things and of Natural Magic* (*Del senso delle cose e della magia naturale*), argued that the world is a living thing and for the existence of the *anima mundi*. At around this time he also wrote *On Christian Monarchy* (*De monarcha Christianorum*), agitating for a reform of society and the Church. Clearly he was another Neapolitan destined to give the Vatican sleepless nights.

In 1592 Campanella travelled to Padua on the well-worn path via Rome and Florence, meeting Gian Vincenzo Pinelli and Padua University's new Professor of Mathematics, Galileo.<sup>3</sup> Campanella and Galileo were to stay in touch for the rest of their lives. It was also in Padua that more questions were raised about Campanella's dangerous beliefs. As a result, early in 1594 he was arrested by the Inquisition and transferred to Rome towards the end of the year – to the same prison as Bruno, although it is unlikely that they were allowed to communicate. Compared to Bruno's continuous imprisonment ending in his execution, Campanella got off lightly. After agreeing to abjure his works he was released into a kind of house arrest in a Dominican monastery, although in 1597 his superiors ordered him back to Naples. Campanella had not been around long enough to make himself as much of a nuisance as

Bruno, and he had not so far made much headway with plans for Hermetic reform.

In fact, Campanella shared Bruno's vision of the great magical transformation that was glimmering over the horizon, and which was written in the stars. He also regarded the heliocentric theory as the trigger of the new age of Hermetic enlightenment, and – for astrological and other reasons – he believed it was destined to happen in 1600.

The approach of the new century encouraged Campanella to be much more politically proactive than Bruno even at the height of his career. Leaving Naples for the south, he threw himself into organizing the Calabrian revolt, which aimed to overthrow Spanish rule, beginning with Calabria – the arch of the Italy's 'foot' and 'toe', which had long been 'restive with political and religious dissidents'<sup>4</sup> – and then the whole of the Kingdom of Naples.

The Calabrian revolt is remarkable for the number of its Dominican supporters. Indeed, there was something very odd about the Order in Calabria, from at least the time it produced Bruno, but frustratingly after so many years it is impossible to pinpoint exactly the reason for this. This uprising was considerably more than just an expression of Calabrian nationalism. It was to be a preparation for the coming age, and aimed to establish a republic based on magical principles that would – under its messiah Campanella – hold aloft the torch of the new age for the rest of the world to follow. Bruno, too, had railed against Spanish rule over the Kingdom of Naples in *The Expulsion of the Triumphant Beast*.

If the revolt was successful it would bring the Hermetic republic geographically close to the Papal States – the two shared a long border, cutting across the whole of Italy from Mediterranean to Adriatic coasts. A truly

alarming prospect for the Pope and his henchmen.

The uprising, however, was not to be. Informants betrayed it to the Spanish authorities, and after the organization was ruthlessly crushed in November 1599, Campanella and the other leaders were arrested. This almost certainly accounts for the Inquisition's sudden desire to be rid of Bruno, the revolt's spiritual inspiration, and he went to the stake barely three months later. Stephen Mason of Cambridge University argues that he was executed as an example to the Calabrian rebels, because of the connection to Campanella, and that he had been held for so long as a kind of hostage because of his standing among the insurgents.<sup>5</sup> Publicly executing their spiritual leader at the beginning of their special year – 1600 – would also have been a calculated psychological move, rather akin to roasting the Pope on 25 December of a new millennium.

This was, however, by no means the end of Campanella's story. His continuing career sheds a rare light on Galileo's trial thirty years later – over which Bruno, too, would cast a giant shadow.

Campanella escaped the death penalty visited on the revolt's other leaders through feigning madness. According to the law of the times, the insane could not be sentenced to death, not out of compassion but because they couldn't comprehend the opportunity to repent of their sins before execution. If a judge did condemn them he, not the condemned, would take responsibility for their eternal damnation. However, there was considerably more to feigning insanity than a bit of Hamletesque raving about clouds looking like camels and some foaming at the mouth. The madness defence was hardly the easiest option. To prevent every miserable prisoner from using it to evade the death penalty, the Neapolitan authorities had come up with a



twist. The accused had to maintain their mad behaviour – or keep up the pretence – under prolonged torture.

Somehow the extraordinary Campanella managed to pass this test, and was duly sentenced to life imprisonment. For the next quarter of a century he was moved around a series of castle dungeons in the Kingdom of Naples. Although most prisoners in that place and time would have suffered horrors from the stark loneliness and the squalor of their own filth in the dark, fending off rats, Campanella's life was surprisingly non-onerous. Viewing his imprisonment as an extended opportunity for study and contemplation – much like being in a monastery – he spent his time refining his ideas and writing. Not only was he supplied with books and writing materials and had at least some light in his cell, but he also received a steady flow of scholarly visitors, mainly from Germany, who took his writings back home to be

published. Why his jailers were so obliging is a bit of a puzzle, especially as it must have dawned on them by now that he was as sane as they were – probably more so. Presumably bribes were involved from somebody, somewhere.

The revolt having failed, Campanella's goal now became the reformation of society through the Vatican and, perhaps oddly, the Spanish monarchy he had plotted to overthrow. Like Bruno, his ambitions were nothing if not excessive.

Only once in his books did Campanella mention Bruno directly – significantly in a defence of Galileo published from prison in 1622 – and even then he was careful to declare that Bruno was a heretic. But Campanella was manifestly familiar with his philosophy and writings, judging by allusions in his work, his favourite being *The Ash Wednesday Supper*. Of course, given Bruno's fate and the continued opprobrium attached to

his name, there was no way Campanella could be more open, especially given that he was trying to win support for Catholic reform – and doing so from prison.

Campanella's major work is *City of the Sun* (*Civitas Solis*), written in the first years of the 1600s but not published until 1623, in Frankfurt.<sup>6</sup> Basically concerned with a utopian society, the text takes the form of a dialogue between the Grand Master of the Knights Hospitaller and the captain of a ship that had sailed to the New World. The captain relates how, after being shipwrecked, he was found by the inhabitants of the City of the Sun, describing its society in detail to the Grand Master. Clearly Campanella's ideal republic, the kind he had hoped to establish in Calabria, the City of the Sun is designed and run according to magical and astrological principles. It is a Hermetic-Egyptian utopia, derived from the prediction at the end of *Asclepius'* Lament. George Lechner of the

University of Hartford, a specialist on magical and astrological symbolism in Renaissance art says of *City of the Sun*: ‘In it, Campanella developed the notion of a new city-state, led by a philosopher-priest-king, and guided by Hermetic magical principles.’<sup>7</sup> And of course it is no coincidence that it was a city of the sun that was being debated, reminiscent of the ‘*Civitas solis*’ that Bruno discussed with the librarian of the Abbey of St Victor in Paris, saying that the ‘Duke of Florence’ planned to build it.<sup>8</sup>

Even from prison Campanella played an influential role in events surrounding the next great champion of the sun-centred theory: Galileo Galilei. The Hermetic chain remained unbroken.

# THE THRICE-GREAT TRIO

Giordano Bruno had made heliocentricity the centre of his Hermetic revolution, the sign that would trigger either the downfall or the reformation of the Church, neither of which was regarded with any great enthusiasm by the Vatican. For Bruno and the Giordanisti, heliocentricity was not just a theory: they believed its acceptance would usher in a new Hermetic utopia. And even with Bruno out of the way, it was feared that he had left behind a secret society – who and where nobody knew – which was proactively committed to bringing the Hermetic revolution about. Tommaso Campanella, Bruno's spiritual heir, who shared his view of the importance of heliocentricity and was possibly even one of the Giordanisti, had conspired in a rebellion against the Kingdom of Naples

and therefore against the Spanish crown, aiming to attack those who were deemed most loyal to the Catholic cause.

Given this context, Copernicus' original evocation of Hermes Trismegistus' name in *On the Revolutions of the Celestial Spheres* was hardly likely to have been missed by those whose job it was to protect the Church. Perhaps placing the sun at the centre had been a devilish Hermetic plot all along? There was no way for those organizations whose task it was to defend the Church – the Inquisition and the Jesuits – to be sure, and every reason for them to be nervous. During the sixteenth century the Roman Church had only just survived its greatest trauma, a seemingly impossible undermining of its authority by the rise of the Protestant Churches. So who was to say what might happen next? The ideas of Bruno and other Hermeticists were being discussed across Europe, and even highly placed members of

the Catholic Church had embraced them. Hermetic principles were being openly advocated. And then there were the Giordanisti – how many there were, and how widely they were spread, nobody knew. Maybe the Inquisition and Jesuits were over-reacting, but these were times that engendered paranoia. And so it was considered that – at the very least – establishing heliocentricity would attract more converts to Hermeticism. More readers would devour Bruno's works, and possibly attempt to act on his agenda of radical reform.

As long as Copernicus' idea remained simply a theory, however, the Hermetic implications barely registered. But when an individual claimed he had come up with *proof*, then the Church began to become seriously worried. And ecclesiastical anxiety ran even deeper when it was discovered that the threat came from a direct associate of the mystical revolutionary Tommaso

Campanella and other Giordanisti suspects, such as Pinelli and his circle in Padua – in other words, Galileo.

The Hermetic interpretation of heliocentricity adds an important and otherwise missing element to the story of Galileo's persecution, finally making sense of some of its more puzzling aspects. Why, for example, were the Jesuits – Galileo's main enemies – so zealous about making an example of him? And why exactly did they consider his work so dangerous?

Galileo wrote to a friend in Paris as he was about to leave for Rome to face the Inquisition in 1633:

I hear from a good source that the Jesuit Fathers have impressed the most important persons 'in Rome' with the idea that my book 'the Dialogo' is execrable and more dangerous to the

Holy Church than the writings of Luther and Calvin.<sup>9</sup>

Comparing Galileo's work to Luther and Calvin seems rather excessive. How could proving Copernicanism possibly do anything



like the same damage to the Church as those famous pioneering Protestants? And during a time when other heretics were challenging fundamental doctrines such as transubstantiation, heliocentricity does seem rather tame. There was something else behind the Church's anxiety, something massive but unstated which lies somewhere in the significance of the heliocentric theory to the dangerous Hermeticists.

Because the Galileo affair has been used for so long to score points in the contest between science and religion it has become hedged round with assorted myths propounded by one side or the other. Take for example the well-worn story of Galileo finishing his public recantation of his belief in the motion of the Earth around the sun by muttering the aside, 'And yet it moves'. This was invented a century after the event, but has been repeated so often it is now considered by many to be the gospel truth. With

so many assumptions and so many myths, it is almost impossible to uncover the simple truth. Almost, but not quite.

Galileo has often been depicted as a modern rationalist-materialist scientist who had somehow been born out of time, and who was persecuted by superstitious – in other words cretinous – men whose intellects were stuck in the Middle Ages. Galileo is seen as a martyr for science and a victim of irrational religion. But of course the reality is that he was very much a man of his time, and we should no more assess his character and motivation by modern standards than we should Copernicus or Kepler.

While most educated people today still think that Galileo's trial was all about a clash between the scientific and religious mind-sets, historians have long realized that this is way off the mark. It has therefore become fashionable to see the affair as a collision between two great and obstinate egos, two

pathologically 'right men': Galileo, who refused to be told what he could do or say, and Pope Urban VIII, whose ego had been bruised by Galileo putting his views in the mouth of a character offensively named Simplicio. The prevalent view is that if only Galileo had not been so stubborn, and had made it clear that he was presenting heliocentricity simply as a hypothesis, then all of his trauma could have been avoided. The very fact that the myth of the clash of egos has endured is an acknowledgement that something is still missing. It seems that the elusive 'something' may have been a factor that neither side wanted to see the light of day ...

On the question of Galileo's attitude to Hermeticism, ironically other historians argue that he would have nothing to do with it because he was too staunch and conventional a Christian. Particularly after the way he was portrayed in Dan Brown's thriller *Angels*

*and Demons* (2000) there was a rush to paint him as an especially devout Catholic, respectful of the Church. But there is little evidence for this. Galileo's published works deal with matters of science, not religion, and his surviving personal letters contain very little on religious matters. Naturally he used the conventional Christian platitudes of the time, and observed the outward trappings – going to church, taking communion and so on – as everybody was compelled to do in that time and place; but no more than this.

In his published works, Galileo explicitly distanced himself from certain of the esoteric arts (most specifically numerology derived from Pythagoras), which is taken by today's commentators to indicate his modernity and rationalism. However, given what had happened to Bruno, this could equally have been simply an act of self-preservation: one specialist, Giorgio de Santillana, specifically

links the disavowal of numerology to Galileo distancing himself from Bruno and his ilk.<sup>10</sup> And in any case, dismissing one arcane system does not necessarily mean dismissing everything esoteric. And yet on the other hand, Galileo practised astrology. It is often stated in popular histories that, although he drew up horoscopes for wealthy clients, he only did this for the money, and never actually believed in it. In fact, there's no evidence at all that this was his attitude – it is yet another example of modern projection.

Galileo was undoubtedly a brilliant pioneering scientist who used observation and experiment to work out the laws governing physical phenomena and sought to explain them in mathematical terms. The methods he developed would inspire and shape the next generation and culminate in the genius of Isaac Newton. Both Einstein and Stephen Hawking have hailed Galileo as the father of modern science, and he has been described

as ‘the world’s first celebrity scientist’<sup>11</sup> – the Einstein of his day. But there are many ironies in his story and the way it has passed into history, or perhaps more precisely, legend.

The first irony is that what Galileo is best known for now – helping to establish the heliocentric theory – is actually one of the least important aspects of his work. His major contributions to science were in what we today would call the field of physics: motion, optics, acoustics and so on. In astronomy, his big innovation was to improve the telescope to the point that it was good enough for astronomical observations (although he originally thought in terms of military and maritime applications). And while the observations Galileo made with the telescope produced new evidence in favour of Copernicus, the arguments he thought proved the theory were, in fact, entirely mistaken. Galileo thought that the smoking gun was the phenomenon of the tides, arguing their ebb and

flow could only be explained by the Earth's rotation, airily dismissing Kepler's suggestion that they were caused by the pull of the Moon. In this, Galileo was, of course, completely wrong.

In fact, his whole attitude to heliocentricity was at odds with the methodical and meticulously worked-out approach that characterized the rest of his work and which rightly justifies his status as the founding father of the modern scientific method. Einstein thought Galileo was so determined to prove Copernicus right that he was blind to the obvious problems with his argument.<sup>12</sup> As the Danish science historian Olaf Pedersen, speaking at a conference on the Galileo affair in Cracow in 1984, observed:

In consequence [of his acceptance of the theory] it became imperative to find convincing reasons for its being true in a physical sense, as Galileo tried to do with his somewhat unsat-

isfactory theory of the tides ... <sup>13</sup>

In other words, Galileo became convinced by the theory and then set out to find evidence for it – hardly a true scientific approach. He enjoyed his celebrity status and the material benefits it brought. He had a flair for self-publicity, never being one to hide his innovations and discoveries, if anything exaggerating them. But he seems to have made it his mission in life to see the theory of heliocentricity proved, while being uncharacteristically circumspect about his support for it. Although writing to Kepler as early as 1597 that he had ‘become convinced by Copernicus many years ago’,<sup>14</sup> publicly he was keen to be seen as much more equivocal, even evasive.

Of course, Bruno’s fate must always have been at the forefront of his mind, and must have acted as a hideous, cautionary tale. Advocating the motion of the Earth had certainly contributed to his condemnation as a heretic, and Galileo, along with other



scholars in Catholic lands, may well have considered it prudent not to whip up any hype. But despite this, there is evidence of a more solid connection between Galileo and Bruno and the Giordanisti – including evidence that Galileo owed an intellectual debt to Bruno. And there is no doubt whatsoever that Galileo was fully aware of the significance that the Hermeticists read into heliocentricity.

Galileo was a lifelong friend of Campanella. One of his staunchest supporters during the controversy, Campanella composed the *Defence of Galileo* from his prison cell in 1622. And ten years later, by then a free man living in Rome under the protection of the Pope himself, he was still corresponding with Galileo during the latter's most difficult time, urging him to stand firm *because of the spiritual importance of his work*. Yates remarks when discussing *Defence of Galileo*:

Campanella is being careful to dissociate himself from the full implications of Bruno's Copernicanism. This was all the more necessary since, both in the apology and in letters to Galileo,

1977/900  
Campanella speaks of heliocentricity as a return to ancient truth and as portending a new age, using language strongly reminiscent of Bruno in the *Cena de le ceneri* [*The Ash Wednesday Supper*] ... And in other letters he assures Galileo that he is constructing a new theology which will vindicate him. It has therefore to be made clear that heliocentricity as a portent of a new age, and as integrated into a new theology did not mean for Campanella at this stage in

his career, acceptance of all Bruno's heresies.<sup>15</sup>

So Galileo was not only in contact with Hermeticists, but was also very aware of just how important they considered his work. But could the connection go much deeper? Was there a more mystical dimension to the whole affair?

Galileo was familiar with Bruno's writings. In the 1590s, when he first focused on heliocentricity, there was no problem with being a fan of the Neapolitan – just as after 1600 there were excellent reasons not to be seen to be. After the publication of Galileo's first book touching on the controversy, Kepler criticized him for not honestly acknowledging the intellectual debt he owed to Bruno.<sup>16</sup> Of course it was easy for Kepler,

who cited Bruno in his own work, to criticize Galileo from the safety of Bohemia.

But Galileo's interest in Bruno goes deeper than merely reading his books. There are close parallels between Galileo's 1632 *Dialogue Concerning the Two Chief World Systems* – which led to his downfall – and Bruno's *The Ash Wednesday Supper*, the first of his works to advocate Copernicus and to declare that establishing heliocentricity would free the human spirit. It may not be a coincidence that this was Campanella's favourite of Bruno's works.

Another clue suggesting Galileo's familiarity with Bruno comes from a passage in the *Dialogue* where he lays the foundation for the later theory of relativity. Although the term is popularly associated with Einstein, what he formulated were his special and general theories of relativity, which are in fact highly complex developments of Galileo's original principle (sometimes called

‘Galilean relativity’). This argues that physical phenomena can only be properly described according to the context in which they are observed – i.e., the same event can look completely different to observers in different places. This principle underpinned Newton’s laws of motion and Einstein’s own theories.

In *The Ash Wednesday Supper*, published over forty years earlier, Bruno made the same point with a very similar example: if two people, one on shore and the other on the deck of a moving ship, drop a stone, each will see their own stone move through an identical path, dropping the same distance at the same speed, but they will perceive the other’s stone as moving further – not only downwards but sideways – and therefore faster, since it covers a greater distance in the same time.<sup>17</sup> Descriptions of events therefore depend on the frame of reference.

Having never publicly referred to the Hermetic interest in heliocentricity, why should Galileo base his masterwork on a book by someone anathematized by the Church for championing precisely that theory? Perhaps this was a covert acknowledgement of his debt to Bruno, or even a coded hint that he was aware of his own significance to the Hermetic vision.

# THE DAY THE EARTH STOOD STILL

Galileo di Vincenzo Bonaiuti de' Galilei's career began in 1592 at the age of twenty-eight, when thanks to Bruno's incarceration he became Professor of Mathematics at the University of Padua. This is where he met Campanella and began an important and lifelong association. Another major influence at that time was Pinelli – often described as Galileo's mentor – who introduced him to the emergent science of optics, which was to make Galileo's reputation. Another of his dubious associates was Traiano Boccalini, author of the Bruno-inspired *News from Parnassus*, and a controversial friar and professor of canon law named Paolo Sarpi, who was at the forefront of the legal challenges to the Pope's authority and the attempts to

forge an alliance with James I's England in the first decade of the seventeenth century. With friends like these, the Inquisition must surely have kept a very close eye on Galileo from the beginning.

Galileo became convinced of the truth of the Copernican theory 'many years' before 1597, although precisely why he had this epiphany remains uncertain. We have also seen that he incorrectly considered the movement of the tides as the best evidence for, even the proof of, the theory. He persisted in this view even when he produced much better evidence through his pioneering use of the new cutting edge technology of the telescope, begun around 1610. His astronomical observations – that the Moon's rugged surface is reminiscent of our own world, the existence of the moons of Jupiter and particularly Venus' lunar-like phases – strongly supported Copernicus' theory. Galileo realized how sensational these discoveries would appear, and

cannily sought to use them as leverage to build a career. So he rushed into print before anyone could steal his thunder, premiering his first wave of discoveries in *Starry Messenger* (*Sidereus nuncius*) in 1610.

As he had guessed, the intelligentsia became greatly excited and he landed the position he craved as court mathematician and philosopher to Cosimo II de' Medici, Grand Duke of Tuscany. Perhaps this wasn't too surprising given that Galileo had been careful to dedicate the book to him and proposed calling the new moons of Jupiter the 'Medicean stars'. Even the world's loftiest thinkers obviously recognized the most basic principle: flattery will get you anywhere.

It seems odd that Galileo failed to use his discoveries to bolster the Copernican theory, even though he was an ardent supporter. In both *Starry Messenger* and a follow-up book on his discovery of the phases of Venus, he merely presented the observations. Perhaps,



as he was hoping to build a glittering new career on them, he decided that it was best to play down the Copernican implications of his discoveries.

But the row refused to go away. Most readers with an astronomical background got the point: Galileo's discoveries seriously undermined the traditional Ptolemaic system. But even this failed to shift the consensus to Copernicus. Hybrid systems, such as Tycho Brahe's, where some celestial bodies orbited the sun and some the Earth, were preferred.

From the Church's point of view Galileo's discoveries were already unwelcome news, and threatened worse to come. Not only was his work propelling scholars towards heliocentricity, but the telescope might lead to further discoveries that would decisively tip the balance in its favour. And now there was an added piquancy: if irrefutable proof was forthcoming, would it inspire the Hermeticists to kickstart their revolution,

philosophically, theologically – even politically?

Matters came to a head in 1615 when Galileo finally went public with his support for heliocentricity. He circulated an essay based on the biblical passages that implied the Earth did not move, including the unequivocal statement: ‘I hold that the Sun is located at the centre of the revolution of the heavenly orbs and does not change place, and that the Earth rotates on itself and moves around it.’<sup>18</sup> This was an extraordinarily dangerous declaration that would transform Galileo’s fame into notoriety overnight.

Pope Paul V ordered a group of cardinals to investigate the issue of heliocentricity on theological grounds, and they decided it was contrary to scripture. As a result, Copernicus’ *On the Revolutions of the Celestial Spheres* was finally banned, along with any other pro-heliocentric works. Galileo was summoned to Rome to be warned off and put right. The

sun moved round the Earth and not vice versa. It was true because the Vatican said so.

But there was an unspoken subtext: the cardinal tasked with warning Galileo was none other than Roberto Bellarmino, the same man who had interrogated Bruno in his last months, and was responsible for his condemnation and execution. This was not a coincidence – Bellarmino had been Archbishop of Capua since 1602, but was recalled to Rome specifically to deal with Galileo. He even interviewed Galileo in the same room as he had interviewed Bruno.

Bellarmino, of course, understood from his experience of Bruno the significance that heliocentricity possessed for the Hermetic revolution. Bruno was dead and Campanella incarcerated in Naples, but they had followers – nobody knew how many. And now here was Galileo, associated with both Campanella and Pinelli, getting dangerously close to

the proof that Bruno had declared would trigger the new Hermetic age. In the end, nothing harsh was done to Galileo. He was simply given a document written by Belarmino himself stating that the Pope had decreed that Copernicus' views could not be 'defended or upheld'. Galileo hastily agreed.

Even more telling is Galileo's immediate reaction after receiving his warning. Rather than return directly to Florence, he wanted to travel to Naples and was obliged to request permission from his patron, Duke Cosimo – but Cosimo refused. Why Naples? A crucial piece of the jigsaw fell into place when we read in a paper by Olaf Pedersen, a specialist in the religious aspects of the Galileo affair, that the reason for Galileo's request and the odd refusal was that he wanted to visit Tommaso Campanella in his prison cell.<sup>19</sup> In other words, the Church brings in the man who had condemned Bruno to warn Galileo off, and Galileo wants to consult

Bruno's successor Campanella; surely none of this was a coincidence.

Having been denied a meeting with Galileo, Campanella rallied to the cause, penning the *Defence of Galileo*, which his followers published in Frankfurt. However, given Campanella's reputation – one conviction for heresy and another for subversion, for which he was still doing time – the kind of support he could muster was hardly designed to enhance Galileo's reputation. Which is probably why, back in Florence, Galileo kept his head down. Nothing in the Pope's decree prevented the *discussion* of heliocentricity as a hypothesis, and many scholars were avidly doing just that. However, Galileo himself dropped the whole subject for many years, although he was clearly waiting for an appropriate time to re-emerge as its iconic figurehead.

A potential change for the better came in 1623 when one of Galileo's old friends,

Maffeo Barberini, became Pope Urban VIII. They had met at the Florentine court, and Barberini was an admirer of Galileo's work, especially his research into the laws of motion. Galileo went to visit Urban in Rome the year after he was elected, and they had six private meetings – during which, as he himself reported in a letter to a friend, Galileo described all believers in Copernicus' work as 'heretics'.<sup>20</sup> Clearly he had no desire for another confrontation with a Bellarmino clone.

In another of those astonishing reversals of fortune that litter the history of that era, Urban's election was also good news for Campanella. In 1626 Urban requested that the Spanish king release him from prison so he could travel to Rome to perform protective magic to ward off the evil effects of an eclipse that the Pope's enemies had predicted would kill him. After twenty-seven years, not only was Campanella free but appointed

adviser to the Pope. Urban even went so far as to grant him permission to found a college in Rome to train missionaries who espoused his religious and philosophical ideas. Such papal favour being bestowed on his greatest and most controversial supporter was another good sign for Galileo. In 1631, the year before it all fell apart, Urban even appointed him as a canon, which enabled him to draw income from two vacant benefices (without doing a day's work in either).

It was during this time that Galileo decided it was safe to have another stab at pushing the heliocentric theory. And so he wrote *Dialogue Concerning the Two Chief World Systems* – unusually for him in Italian rather than Latin, widening his potential readership – in which two scholars debate the Copernican and Ptolemaic systems, with a third adjudicating. It was published in Florence in 1632, having been granted formal approval by the Inquisition in that city.

Galileo had even sought permission from Urban to publish; the latter only asked that his own views on the matter be included.

The irony – which is seldom mentioned by modern historians of science – is that the main pro-Copernicus argument that Galileo puts forward in the *Dialogue*, his old ‘proof’ based on the tides, was wrong. His original title was, in fact, *Dialogue on the Ebb and Flow of the Sea*. The Inquisition in Florence forced him to change the title, which is odd, as the new one made it more obvious that the book was about the heliocentric debate. Galileo was careful to keep to the rule of discussing Copernicanism without actually advocating it. Nevertheless, the book caused rumblings, especially among the Jesuits, and Urban came under pressure to act.

Despite the myth of the ‘clash of egos’, it is clear that Urban had to be pushed into action. His position as pope was far from secure, as many in Rome thought him too soft



on Protestantism – there was even talk of deposing him.<sup>21</sup> This was largely because Urban was concerned about the power of the Hapsburg dynasty, which ruled Spain and the Holy Roman Empire, both of which were locked in battle with the Protestant nations. For his own political reasons he had refused to give his sanction to the war or to lend it diplomatic or military support, but it did lead some to wonder where his sympathies really lay. His many opponents among the Cardinal Inquisitors were making much of his endorsement of the *Dialogue's* publication as another sign of his softness on heresy. He therefore had to take action to keep his own position secure. This was no clash of egos. Urban was just running scared.

As a result of Jesuit pressure, Urban appointed a commission to investigate whether Galileo had broken his ban of sixteen years earlier. Some historians believe that this was an attempt to keep the Inquisition out of the

matter, another sign of the Pope's reluctance to let the Inquisition loose on his old friend. If so, it was remarkably unsuccessful. In September 1632 Urban instructed the Inquisition in Florence to deliver a summons to a shocked Galileo to present himself in Rome to answer questions about his book. He appeared before the Inquisition in April the following year, no doubt with Campanella's advice to stand firm – because of the *theological* (that is, Hermetic) importance of establishing that the sun was at the centre – ringing in his ears.

Galileo's defence was that his book had not upheld Copernican theory, but had merely discussed it. He declared that until the decree of 1616 he had regarded neither the Copernican nor Ptolemaic hypothesis as beyond dispute (contradicting his statements to Kepler thirty-six years earlier), but since then he had held the Ptolemaic view 'to be true and indisputable'.<sup>[22](#)</sup> While few would

blame Galileo for reneging on his own opinions and weaselling out of the situation – after all, this was the Inquisition he was facing – these were hardly the words either of a noble defender of intellectual freedom or willing would-be martyr. And yet neither does he seem an arrogant old man who refused to admit he was wrong.

Galileo lost. The inquisitors decided that the *Dialogue* was a disingenuous attempt to promote heliocentricity, which it probably was, and that his attempts to disguise it as a mere discussion were totally unconvincing. He was found ‘*veementemente sospetto d’eresia*’ – vehemently suspect of heresy – just one degree below actually being a heretic. The only way out was to ‘abjure, curse and detest’ the very ideas that caused the suspicion.

Galileo had to admit his error and renounce his ideas, kneeling before the altar of Santa Maria sopra Minerva, the same

basilica from which Bruno had set out to his horrendous death thirty-three years earlier. Publication of anything by Galileo – anything he had written or would write in the future – was forbidden (although in the event he did manage to get some works printed in Germany). He was sentenced to life imprisonment, but as he was over seventy years old, was instead committed to house arrest. He stayed first with a supporter, the Archbishop of Siena, where one of his first visitors was none other than Tomasso Campanella ... [23](#)

Later, Galileo was allowed to return to his own villa outside Florence, where he died in 1642. Less than a year before his death he wrote to the Florentine ambassador in Venice that:

The falsity of the Copernican system ought not to be doubted in anyway, and most of all not by us Catholics who have the undeniable authority of Holy Scripture, interpreted by the best

theologians. [24](#)

Perhaps Galileo had an unusually over-developed sense of irony.

But what of Campanella? In 1634, the year after Galileo's trial, there was another attempt to organize a revolt in Calabria. Whether Campanella was directly involved is unclear, but the leader was certainly one of his followers. So it was expedient, to say the least, for him to leave Rome for Paris – a well-worn route for fugitive Italian Hermeticists. There he became a favourite of Cardinal Richelieu, who persuaded the king to give him a pension. Encouraged by this, he transferred his hopes to the French monarchy, urging Richelieu to make Paris into his City of the Sun. His big hope settled on the future Louis XIV, born in 1638, who he expected to rule the world in partnership with a reformed papacy. Campanella was the first person to call the infant Louis the Sun King, as an acknowledgement of his great *Hermetic* potential.<sup>[25](#)</sup>

After Campanella's dizzyingly strange and extreme career, which took him from castle dungeons to the favour of some of the greatest figures in Europe, he died in Paris in May 1639. But there can be no doubt that his legacy lived on.

# GALILEO'S SECRET

Although during the nineteenth and first half of the twentieth centuries researchers perceived a connection between the trials of Bruno and Galileo, the notion of Bruno's fate being a more severe foreshadowing of Galileo's persecution, dying for his Copernican beliefs, is not borne out by the facts. There was indeed a connection between the two, but it is the other way round. Action was taken against Galileo because of the Hermetic – the Brunian – implications of his views.

Yet while not often recognized, the connection between the two trials is hugely significant. Although Galileo's trial is always cited as *the* moment when forces of reason and dogma collided head-on, the Hermetic factor is arguably the most important. It was, after all, the reverence that heliocentricity

was accorded by Hermeticists in general and Bruno's followers in particular that was the major reason the Church sought to damn heliocentricity, and therefore Galileo himself.

Neither side could admit what Galileo's trial was really about. While being aware of the Hermetic implications of the *Dialogue*, Galileo never made them overt, which meant that the Church couldn't use that against him. It is unlikely it would have wanted to draw attention to the importance of heliocentricity for the Hermetic revolution in any case. The Hermetic factor was therefore present, however, but simply relegated to the background – which is why there is a distinct sense of something missing in the conventional story of the trial.

Given the uncompromising Bruno and the revolutionary Campanella, the Inquisition and the Jesuits would have undoubtedly been only too fearful of the threat posed by Hermeticism. They would have traced the



same connections we have outlined – beginning with Copernicus’ references to Hermes Trismegistus, through Bruno’s reforming career and the hidden presence of the Giordani, to Galileo’s links with Pinelli and, most damningly, Campanella. They may even have seen the connection between Galileo’s *Dialogue* and Bruno’s *The Ash Wednesday Supper*. Even if they were putting two and two together and coming up with five – a not uncommon occurrence with the Inquisition – these connections would still have shaped their fears and consequently their actions.

It seems, however, Galileo was by no means as innocent as he tried to appear. There are valid questions, for example, about his relationship to the secret Hermetic reform movement. There is his continued association and correspondence with Campanella to take into consideration, especially his wish to see him in the wake of his warning-off in

1616. What would Galileo get out of such an association? Campanella was a religious, esoteric and political theorist – not a mathematician or scientist. For an ambitious man like Galileo, conscious of his image, Campanella was hardly the kind of company he should have wanted to keep.

And then there is Galileo's apparent use of Bruno's *The Ash Wednesday Supper* – which contains the first mention of the concept of the Copernican sun as the trigger for a new Hermetic age – as a model for his *Dialogue Concerning the Two Chief World Systems*. Was this merely a belated, and necessarily covert, acknowledgement of Galileo's intellectual debt to Bruno, rectifying the failure for which Kepler had criticized him? Or was it a covert signal to the Giordanisti that he was a sympathizer – perhaps even one of them? It is safe to say that at the very least Bruno's work influenced Galileo's, which yet again places

Hermeticism at the centre of the scientific revolution.

## Chapter Three

[1](#) Ferris, pp. 85–6.

[2](#) Yates, *Giordano Bruno and the Hermetic Tradition*, p. 360.

[3](#) *Ibid.*, p. 363.

[4](#) Mason, p. 462.

[5](#) *Ibid.*, p. 468.

[6](#) See Morley for a translation of *City of the Sun*.

[7](#) Interviewed in Burstein and de Keijzer, p. 242.

[8](#) Yates, *Giordano Bruno and the Hermetic Tradition*, p. 233.

[9](#) Quoted in Olaf Pedersen, ‘Galileo’s Religion’, in Coyne (ed.), p. 75.

[10](#) In his notes to Galileo, Salusbury translation, p. 15.

- [11](#) Oxford University science historian Allan Chapman, quoted in Couper and Henbest, p. 154.
- [12](#) In his forward to Stillman Drake's translation of Galileo, p. xvii.
- [13](#) Pedersen, 'Galileo's Religion', in Coyne (ed.), pp. 80–1.
- [14](#) Quoted in *ibid.*, p. 80.
- [15](#) Yates, *Giordano Bruno and the Hermetic Tradition*, p. 383.
- [16](#) This was in a conversation in 1610 with Martin Hasdale, the librarian at Rudolph II's court, who relayed Kepler's remarks to Galileo in a letter. (Singer, *Giordano Bruno*, p. 189.)
- [17](#) Bruno, *The Ash Wednesday Supper*, pp. 122–3.
- [18](#) Quoted in Finocchiaro, p. 88.
- [19](#) Pedersen, 'Galileo's Religion', in Coyne (ed.), p. 97.
- [20](#) *Ibid.*, p. 92.
- [21](#) Finocchiaro, p. 13.

- [22](#) Quoted in Pedersen, 'Galileo's Religion', in Coyne (ed.), p. 81.
- [23](#) *Ibid.*, p. 97.
- [24](#) Quoted in *ibid.*, p. 81.
- [25](#) Yates, *Giordano Bruno and the Hermetic Tradition*, p. 361.

## CHAPTER FOUR

# THE FALSE ROSICRUCIAN DAWN

The Hermetic cause suffered several major setbacks in the early years of the seventeenth century, and for a time it must have seemed as if its hopes for a new golden age had been dashed once and for all. The first setback was, of course, the grisly execution of audacious prime mover Giordano Bruno in 1600, but the second came fourteen years later and was to provide even more ammunition for those opposing the Hermetic movement.

When the *Corpus Hermeticum* was rediscovered in the mid-fifteenth century

everybody – whether they supported or opposed Hermeticism – accepted that the texts dated from the most ancient days of the Egyptian civilization. But suddenly a learned work exploded onto the scene that made the startling claim that the texts were of a much later provenance, not being written until the second or third century CE. The bombshell was *Of Things Holy and Ecclesiastical* (*De rebus sacris et ecclesiasticis*), by one Isaac Casaubon. Born to refugee Huguenot parents in Geneva in 1559, he was widely regarded as the most learned man in Europe, his speciality being classical languages. After a glittering academic career in Switzerland and France he found himself working at the royal library in Paris under the patronage of Henri IV, the great hope of the Hermetic reformers. In May 1610 Henri, like his predecessor, was assassinated by a Catholic fanatic. This prompted a lurch towards ultra-orthodox Catholicism in France, which made life



decidedly uncomfortable for Protestants such as Casaubon, who was more than happy to accept an invitation from James I to move to England.

Upon his arrival, the King of Scotland and England asked Casaubon to work on a rebuttal of a key text of the Counter Reformation, the gargantuan multi-volume *Ecclesiastical Annals* (*Annales Ecclesiasti*) by the Catholic cardinal Caesar Baronius – a sweeping history of Christianity that set out the historical case for the primacy of the Catholic Church and the correctness of its teachings. Unsurprisingly it articulated the conventional view accepted by many Catholic theologians that Hermes Trismegistus was one of the pagan prophets of the coming of Christ.

Casaubon only managed to write the first of many intended volumes giving a point-by-point critique of Baronius, as he died in July 1614, and was buried in Westminster Abbey. But in that single volume he still managed to

deal Hermeticism a blow that to some seemed terminal, although ironically he had intended to demolish the *Christian* tradition that accorded Hermes Trismegistus a privileged place in pagan history, rather than to attack Hermeticism itself.

Casaubon began with the observation that no ancient author – nobody, in fact, before early Christians such as Lactantius and Augustine – even so much as mentioned Hermes Trismegistus, still less cited him as the fount of all wisdom. Intrigued by this, Casaubon compared the Hermetic texts with other works to try to establish their sources. He concluded that, contrary to tradition, the writers of the *Hermetica* had drawn upon the works of Plato and the books of Old and New Testaments. He argued, for example, that the sections of the *Pimander* that had once been thought to prefigure the opening chapter of John's Gospel were themselves really based on it.

Since most European readers had used Marsilio Ficino's Latin translation, Casaubon revisited the original Greek to analyse the language, using a printed edition that had been published in 1554. His heavily annotated copy is now in the British Museum. Discovering that the *Hermetica*'s Greek dated from the early centuries CE rather than from antiquity, not unnaturally he concluded that the Hermetic texts were forgeries, created early in the Christian era in order to convert pagans to Christianity by building a bridge between their respective beliefs – a kind of ecclesiastical white lie. He accepted that although there had been a real person known as Thrice-Great Hermes in the high civilisation of ancient Egypt, the *Hermetica* was falsely attributed to him.

The implications for Hermeticists, particularly those who followed Bruno's extreme interpretation that Hermeticism represented the true original religion, were devastating.

Their sacred books did not represent the wisdom of the ancient days of Egypt that produced the pyramids and the Great Sphinx after all. Their sacred books were no longer sacred.

For the historian Garth Fowden, Casaubon's work is 'the watershed between Renaissance occultism and the scientific rationalism of the new age'.<sup>1</sup> Yates called the medieval and Renaissance belief in the antiquity of the *Corpus Hermeticum* 'the great Egyptian illusion'.<sup>2</sup> Ironically, of all illusions, this had been remarkably productive – after all, it had created the Renaissance – but it was an illusion nevertheless.

The great disillusionment, however, was not an overnight sensation. It took a while for Casaubon's arguments to filter through, especially as they were buried in an otherwise obscure and scholarly critique of Baronius. Tommaso Campanella, for

example, who continued his campaign for a Hermetic reform for another quarter of a century after *De*

*rebus* was published, was either unaware of it, or rejected its message. And with a huge irony, it also failed to galvanize Catholic Europe. If the Church's scholars even bothered to read Casaubon, they preferred to side with Baronius and retain their traditional view of Hermes. As we will see, it took ten years for Casaubon's discovery to be used against Hermeticists, and a full half a century to become widely known and accepted.

Despite this blow, Hermeticists often argued that if the philosophy worked, its age and provenance were pretty much irrelevant. Particularly in England, some argued that while the texts themselves might be later than had been thought, the philosophy and cosmology that they contained were much more ancient, having been passed down throughout the centuries before being

committed to writing. Perhaps along the way they had absorbed ideas from other philosophies, such as Plato's, but they still retained the essential beliefs of the Egyptians – a reconstruction that fits perfectly with recent findings. In fact, there were some glaring flaws in Casaubon's line of argument, which were recognized in his day and have become more apparent with the passage of time. Although we will deal with this more fully in a later chapter, suffice it to say here that modern discoveries show that Egyptian thinking was indisputably a major influence on the *Hermetica*. In addition, Casaubon's key argument that New Testament books such as John's Gospel had a direct influence on the *Hermetica* was refuted long ago. Whatever the Hermetic texts are, they are emphatically not Christian forgeries.

What was lost as a result of Casaubon's book, though, was the underlying belief, whipped up by Bruno, that the great reform

would mark a return to the most ancient religion of all, the *prisca theologia*. Even so, the zeal to reform did not simply disappear, instead it found a new mode of expression. Indeed, in the years immediately following the execution of Bruno and incarceration of Campanella, the reforming spirit was already being repackaged with the aid of another major 1614 publication. And this was to cause high anxiety and even paranoia among Catholics for many years, and is still the subject of many conspiracy theories, hotly debated to this day.

## ‘EUROPE IS WITH CHILD’

The second book of 1614 had a much more immediate impact than Casaubon's *De rebus*, one that has never really faded away. This was the appearance of the first of what became known as the ‘Rosicrucian manifestos’, which represented a key development of the reforming side of the Hermetic and esoteric tradition and launched a new and enduringly evocative term. The first of the two manifestos was *Fama Fraternitatis* (*Fame of the Fraternity*), or, *Discovery of the Order of the Rosicrucians* (*Fama Fraternitatis, dess Löblichen Orden des Rosenkreutzes*), usually known simply as the *Fama*. Written in German, it was published in Hesse-Cassel in Germany, but according to contemporary references had been circulating in



manuscript for at least four years prior to being printed.

If ever a book caused a sensation among German philosophical circles, this was it. But the furore had barely subsided when just a year later its sequel appeared. *Confession of the Fraternity R.C. to the Learned of Europe* (*Confessio Fraternitas R.C., ad eruditos Europae*) – usually referred as the *Confessio* – was this time written in Latin and was clearly aimed at a more scholarly audience.

The manifestos announced the existence of a secret order, the Fraternity of the Rose Cross, and invited those who shared its ideals and aims to join. The *Fama* momentarily declared that ‘Europe is with child’, trembling on the brink of a golden age. Great discoveries by recent generations had expanded mankind’s knowledge of the world, the universe and nature, and had also ushered in a new appreciation of the magnificence and

potential of humankind. In the words of the 1652 English translation:

[God] hath raised men, imbued with great wisdom, who might partly renew and reduce all arts (in this our age spotted and imperfect) to perfection; so that finally man might thereby understand his own nobleness and worth, and why he is called *Microcosmus*, and how far his

knowledge extendeth into Nature.<sup>3</sup>

This could have been Pico della Mirandola speaking, 130 years before.

The manifestos, however, went on to warn that the forces of popery and a rigid and outmoded scholarship were obstacles strewn in the path of the coming age.

Tantalizingly, the manifestos named no author, although the writer of a third work two years later, clearly continuing the theme, did eventually identify himself. This was entitled *The Chemical Wedding of Christian Rosenkreutz in the Year 1459* (*Chymische Hochzeit Christiani Rosencreutz anno 1459*). Although published anonymously, a Lutheran cleric and writer, Johann Valentin

Andreae (1586–1654), claimed authorship in his autobiography. As he was a prolific writer of plays, allegorical stories and theological and philosophical essays, and the *Chemical Wedding* is clearly in his style, he was probably telling the truth. So was he also responsible for the manifestos?

Andreae certainly had a connection with the *Fama*, and almost certainly wrote at least parts of the *Confessio* while studying theology at Tübingen University. But opinions are divided about whether they are solely his works or, as is more likely, whether others were involved as well. It seems that the physician and esotericist Tobias Hess, Andreae's close friend and mentor, provided considerable input. Perhaps the whole idea was his. Hess died in 1614, which would explain why the *Chemical Wedding* was a solo effort executed by the younger Andreae.<sup>4</sup>

The books outlined the foundation myth of the fraternity, which was, it claimed, created

by ‘C. R.’ – Christian Rosenkreutz – who was supposedly born in 1378. He aimed to effect a mighty reform of the arts, sciences and religion, and intended to fix all the ‘faults of the Church’. One can safely guess that such a man and such a shadowy organization, would hardly have been music to Vatican ears. Suddenly every shadow posed a potential Rosicrucian threat, every printing press a potential bombshell.

Interestingly, the *Fama* attributes Rosenkreutz’s wisdom to his earlier studies in the Arab world, particularly in Damascus. Not only did he learn magic and the Cabala, but also observed that the scholars and wise men freely shared their knowledge – unlike snobbish and buttoned-up Europe. It was in Damascus that he conceived the idea of establishing a fraternity of scholars in emulation of the eastern style of learning when he returned home.

Rosenkreutz, however, was rebuffed when he tried to introduce his idea for a brotherhood of ‘magicians, Cabalists, physicians and philosophers’ into Europe. So, after a few years back in his native Germany, he decided to form a secret fraternity, beginning with just three followers. The order grew swiftly, devoting itself mainly to healing the sick. Christian Rosenkreutz died at the age of one hundred and six – in 1484 or 1485 – and his burial place was considered lost until a long-hidden tomb was discovered in the House of the Holy Spirit, which the order had built as its headquarters. The discovery, which was – judging from the texts’ internal chronology, in 1604 – a vault lit by an ‘inner sun’ with walls covered in geometric shapes, and which contained all kinds of wonderful instruments and devices, and the founder’s body beneath an altar was the sign that the ‘general reformation of the world’ that Rosenkreutz anticipated was finally at hand.

The brotherhood declared itself to be Christian, but of a reformed kind, and to follow an alchemical philosophy whose focus was on transmuting base souls into divine gold. They firmly rejected the notion that their practice was ‘ungodly and accursed gold making’.<sup>5</sup> The *Confessio* declared ‘the Pope of Rome Antichrist’ in anticipation that the cooperation of the learned would overthrow His Holiness, and by implication the entire Catholic Church. The coming of the ‘light of truth’ had been heralded by new stars appearing in the constellations of Serpentarius and Cygnus in 1604, which links to the discovery of Christian Rosenkreutz’s tomb in the *Fama*. (Kepler also thought that these new stars presaged religious and political changes.)

In 1614 the *Fama* and *Confessio* caused great excitement – and unsurprisingly great hostility from those opposed to such innovations, most obviously Catholics. Tobias

Churton calls them ‘one of the most virulent intellectual hurricanes ever to hit Europe’,<sup>6</sup> while referring to the Rosicrucian furore as Europe’s ‘first multi-national conspiracy story’.<sup>7</sup> The manifestos announced the existence of a secret, elite brotherhood, which was privy to advanced knowledge, and invited applications for membership – but gave no clue about how to do so, implying that only those capable of working it out were worthy of joining. As a result, interested men of learning started writing their own tracts and open letters to the Rosicrucians, appealing for admission. On the other side, pamphleteers denounced the fraternity as subversive and dangerous, no doubt looking over their shoulders as they did so.

As one of the most effective publicity campaigns in history, the manifestos have been a source of perplexity ever since. Was there really a secret society behind them? Or was the whole point to make people *believe* that

such a thing existed? Was it all some kind of hoax? And what was the meaning of the rose and cross symbolism, which has exercised esoteric imaginations ever since? Many suggestions have been made: Martin Luther's emblem was a cross within a rose – and is reproduced in Andreae's *Chemical Wedding*. Yates suggested it could be a combination of two alchemical terms, *ros* (dew) and *crux* (cross).<sup>8</sup> And yet the answer might be much simpler: Andreae's coat of arms was a St Andrew's cross surrounded by four roses.<sup>9</sup> Or perhaps the answer lies in a conflation of all three, since Andreae was a Lutheran and an alchemical influence strongly pervades the manifestos. And while subtlety might be the key to understanding the texts, many commentators over the years have erred on the side of one of the two extremes and have taken everything in the manifestos literally or dismissed them completely as a hoax or fantasy.



Andreae himself often used the term *ludibrium* in relation to the manifestos and the Rosicrucians in general. He also applied this term to his own *Chemical Wedding*. *Ludibrium* basically means a jest, game or a play, which given Andreae's moonlighting activities as a playwright, and his love of the theatre – he particularly admired English drama – probably best describes his intentions. While not literally true, the manifestos were, in Churton's words, 'a dramatic joke with serious intent'.<sup>10</sup> This description calls to mind other similar manifestations, including the relentless social sarcasm of Charles Dickens' comic scenes, the steely undercurrent of today's political satire or, as we claim elsewhere, the subtext behind Leonardo's 'Holy Shroud' of Turin, which we also describe as a *commedia*, or serious joke.

Disappointingly, the story of the great Christian Rosenkreutz in his strangely lit entombment and the origins of his Fraternity

are certainly not factually true. After examining Andreae's later voluminous writings, Tobias Churton proposes that the manifestos are an allegorical account of the transmission of the philosophy that Rosicrucianism continues. Originating in the Middle East, it was preserved in the Arab world before entering Europe via Spain (the *Fama* describes 'C.R.' returning from Arab lands through Spain). But as Andreae decries in other writings, after a promising start that tradition came to a shuddering halt when the brotherhood had to go underground. Now the time was right for it to re-emerge, heralding the coming of a new world fit for heroes.

In the same way that the writers of utopian works, which Rosicrucian were very much in vogue at the time (for example Campanella's *City of the Sun*), hoped to inspire people to attempt to achieve their perfect society, the Rosicrucian manifestos aimed to provoke readers into banding together to create a

learned philosophical brotherhood based on the principles they described. Inviting membership was one method to achieve this. By bringing fellow travellers into the open, they could then begin to build their own utopia, completing a self-fulfilling prophecy.

But was there a secret society behind the tracts? Although this question is harder to answer, clearly the publications were part of a campaign organized by a group of like-minded individuals, who we can legitimately call Rosicrucians, if only for want of a better term. As we will see, there is a suggestion that this group called itself 'Antilia'.

However, in answering the above question, let us also not forget about one group in particular. Experienced in operating underground and passionately dedicated to creating a brave new world from its heartland in Lutheran Germany, Bruno's Giordanisti, formed a quarter of a century before, certainly presents itself as a potential candidate

for the secret society behind the manifestos. As we will see, there were specific connections between Andreae's circle and the Italian radical Hermeticists connected with Bruno and Campanella, and the Giordanisti would be a natural conduit between the two.

# HERMETICISM REPACKAGED

The underlying esoteric philosophy contained in the manifestos was the Renaissance occult philosophy, which as we have seen had Hermeticism at its core. It also highlighted another tradition that had yet to feature prominently in the Hermetic revival: alchemy. A word derived from ‘Al Khem’, the ancient Egyptian word for their country, ‘alchemy’ is also the root of the modern word ‘chemistry’. Despite being derived from Hermetic principles – essentially their application in the field of chemistry – alchemy had yet to become a major part of occult philosophy, coming into Rosicrucianism through the works of the early sixteenth century physician and esotericist Paracelsus.<sup>[11](#)</sup> This is

especially fitting given that the Rose Cross' main concern was always healing.

Another Hermetic giant whose philosophy heavily influenced the manifestos was John Dee. His masterwork, *The Hieroglyphic Monad* (*Monas hieroglyphica*, 1564), presented a new symbol, derived from astrological and other magical glyphs, which he believed embodied the secrets of the cosmos. The significance of Dee's arcane treatise can be deduced from the fact that it was the basis of the Latin tract *A Brief Consideration of a More Secret Philosophy* (*Secretioris philosophiae consideratio brevis*) that prefaced the Rosicrucian *Confessio*. Attributed to Philip à Gabella, who was almost certainly fictitious – his surname probably a reference to the Cabala – it presents explanations, complete with handy diagrams, which shed some light on Dee's distinctly abstruse work. The clear suggestion is that the 'more secret philosophy' behind that penned by the

Rosicrucians is Dee's, whose importance to the movement is underscored by the fact that Andreae's *Chemical Wedding* is decorated with his *monas hieroglyphica* symbol.

The legacy of the great English Hermeticist was obviously hugely important to the shadowy occultists behind the Rosicrucian manifestos. This is perhaps not only true in the world of magic, for Dee was a friend of Elizabeth I, besides being her astrologer, spymaster (whose codename was 007) and a major figure behind the explosive expansion of the emerging English Empire. His was a very useful name.

Andreae was a deeply committed Christian – the motto ascribed to the Fraternity of the Rose Cross, and used elsewhere in Andreae's writing, is *Jesus mihil omnia*, 'Jesus above all'. However, in Tobias Churton's words, 'There are clearly many elements of Andreae's thought – not counting his early and fecund immersion in the world of

alchemy – which are clearly of Hermetic provenance.’<sup>12</sup> In one of his later works, Andreae praised Pico della Mirandola for being one of the pioneers of the philosophy and spirit that he wanted to see more of, besides lamenting its decline in his own day.

The Hermetic basis of Rosicrucianism can be seen in the works of two major devotees, one English and the other German, who both recognized Rosicrucianism as a development of Hermeticism.

The English physician Robert Fludd (1574–1637) was a major intellect of the period, and like any good Renaissance man was passionately devoted to the pursuit of all knowledge. His work was heavily influenced by – really, a continuation of – that of Pico, Ficino and Agrippa, and he quotes constantly from the *Corpus Hermeticum* and *Asclepius*. There are resonances with Bruno’s works that indicate Fludd was familiar with them,



although he never mentions the Hermetic martyr directly.

It would be surprising if Fludd had not studied Bruno, since he was a great exponent of the magical art of memory for which Bruno was most famous. In Fludd's version, the basic 'memory buildings', the interior of which the practitioner holds in his or her imagination, mentally placing talismanic images at specific points within them, are conceptualized as theatres. And, it seems, the theatre on which Fludd based his system was none other than Shakespeare's legendary Globe, highlighting the theatrical and dramatic undercurrents that run throughout this story.<sup>13</sup>

Fludd attempted to attract the interest of the Rosicrucians by publishing, in 1616 and 1617, two books on the subject defending them

from attack. In both he elucidates his belief that the works of 'Mercurius Trismegistus' are the supreme source of the tradition of ancient wisdom of which he himself and the Rosicrucians were a proud part. He was also a devout Anglican, again showing that Christian piety was considered utterly compatible with the arcane.

Later, in 1633, Fludd was to write that the name of the Brothers of the Rose Cross is 'so odious to contemporaries that it is already buried away from the memory of man'.<sup>14</sup> While some see this statement as repudiation in all but name, the reality is very different. Fludd was actually explaining why the brotherhood changed its name to 'the Wise'. As we will see, by the time Fludd wrote this, the Rose Cross had endured attacks that had given it a dark reputation.

Michael Maier (1568–1622) was a very similar figure to Fludd. A respectable physician and committed Lutheran, he was also a

distinguished alchemist. For a time he was also doctor and counsellor to the great esoteric patron Emperor Rudolph II, to whom he dedicated a study of Hermes Trismegistus. From 1611 Maier also spent five years at James I's court in London. Long after his death, his work came to influence the genius that was Isaac Newton. But once again, as Maier was a likely candidate for the Giordani, we find the shadow of Bruno towering in the background.

Both Fludd and Maier were dyed-in-the-wool Hermeticists, basing their work firmly on the Hermetic philosophy. This is particularly significant given that they seem to have dismissed Isaac Casaubon's damning historical critique, despite undoubtedly being aware of it. Both moved in the same English intellectual circles as Casaubon, and Maier was even at James I's court when he published his book at the King's instigation.

When we look more closely at the traditions behind the manifestos, and their direct connections with the Hermetic reform movement, it becomes very obvious that Rosicrucianism was a repackaging of the not-very-secret agenda of Bruno and Campanella.

The essential message of the manifestos was that a new reformation was needed. And the increasingly chaotic world in which the manifestos emerged certainly showed that change was needed. The Protestant reformation was failing externally through Catholic pressure as well as through internal division. The Counter Reformation that spawned the likes of the Jesuits was causing great havoc and threatening to take Europe back into the Dark Ages. The situation was slipping out of Protestant control.

The Rosicrucians sought a return to primitive, unadorned and non-popish Christianity, blended with unashamed mysticism and shot through with a kind of spiritualism.

They advocated a form of shamanism or mediumship, by which practical and magical information was communicated from the spiritual dimension. Overlying all this, however, was the drive towards self-transformation through alchemy of the body and soul. All things would be possible to the initiate, who was radiant with Christ's love and power and would stride forth into transcendence as a human god. This was the ultimate glittering prize and its seekers would do everything in their power to see that they remained in the race to win it.

It is surely beyond coincidence that the Rosicrucians should emerge in the same circles and espouse the same principles as the Giordanisti that Bruno founded in Germany in the late 1580s and early 1590s, little more than a decade before his death. But there were more direct connections between the Rosicrucians and the Italian side of the Hermetic reform movement. The *Fama* was

bound with a German translation of a chapter from the Venetian Traiano Boccalini's *News from Parnassus*, which had appeared two years earlier, calling for, in the words of the *Fama*, a 'general reformation of the whole wide world'. We recall that unsurprisingly the Bruno-inspired Boccalini was an enthusiastic member of Galileo's intellectual circle. This pairing of books links the German Rosicrucian current with 'secret, mystical, philosophical and anti-Hapsburg currents of Italian origin'.<sup>15</sup> As if to remove all doubt of this connection, Andreae defends Boccalini in his *Three Books of Christian Mythology* (*Mythologiae Christianae Libri tres*, 1618).

The conclusive link, however, is found in the two German disciples who visited Tommaso Campanella in prison in Naples and got his books published in Frankfurt. Tobias Adami and Wilhelm Wense were Andreae's close friends and members of the Societas

Christiana that he founded in or around 1618. This society embodied the same spirit and principles advocated by the manifestos – religious reform based on the Christian principle of ‘love thy neighbour’, and the use of scientific enquiry to improve the human condition – but in a more overt and less esoteric way. It was to be the first of a network of Christian Unions, which Adami proposed should be called the City of the Sun, explicitly based on Campanella’s as-yet unpublished work of the same name (which Adami finally managed to get published in 1623).<sup>16</sup> *City of the Sun* was also notably a strong influence on Andreae’s utopian *Christianopolis* (1619).

Which leaves us with the big question, why choose that particular time to introduce Rosicrucianism to the waiting world?

# THE WEDDING

In 1612 James I bequeathed his daughter Elizabeth to the mystical Frederick V, the Elector Palatine, hereditary ruler of the German state of the Palatinate of the Rhine and leader of the Protestant Union, a coalition of Protestant German states formed four years earlier for mutual defence against the Catholic powers. This was seen as a great sign in esoteric circles; it revived those hopes that had once centred on Elizabeth I, Bruno's great goddess, the self-created living icon of the bewigged and jewel-encrusted Gloriana. Her successor James I (of England and VI of Scotland) was notoriously suspicious of all forms of occultism. Upon his succession in the first decade of the seventeenth century, he withdrew royal patronage from Dr Dee,



causing a serious decline in the old man's fortunes and a sad slide into death. But the union between James' daughter and the Elector unequivocally aligned England with the Protestant Union, which had a direct political appeal to James. But it was viewed among those hostile to the Church of Rome with a fervour bordering on the apocalyptic.

As the geopolitics of seventeenth-century Europe spiralling into the Thirty Years War often seems like a morass of confusion, it is worth revisiting the Hermetic agenda at that time. Bruno and Campanella had worked to head off what seemed set to become a catastrophic confrontation between the forces of Protestantism and Catholicism by attempting to reconcile each sides' claims to primacy. The Catholic Church claimed the authority of the apostolic succession going back to Saint Peter, while the Protestants, although a new movement, claimed to be returning Christianity to Jesus' original vision.

Meanwhile, the Hermeticists, by claiming Egypt as an antecedent to Christianity itself, were trying to offer a middle path to both sides – with astonishing naivety, or so it seems with hindsight. On a more realistic, political level, the Hermeticists plotted to gain influence over the most enlightened monarchs from both camps, for example when Bruno wooed Elizabeth I on one side and Henri III on the other. By the time of the betrothal of James' daughter and the Elector Palatine, however, it was crystal clear that the Catholics, now headed by the Spanish Habsburg monarchy, were in no mood to compromise. So while individual Catholic Hermeticists such as Campanella stuck to their agenda, those on the Protestant side had to work and pray for a more robust counter movement to take shape – until there was another opportunity for reconciliation.

The prospects of a new Elizabethan age, and of a united Protestant Europe, were made more likely by the fact that Princess Elizabeth, who was seventeen at the time of her marriage, was very likely to become queen. The heir to the throne, her older brother Henry, Prince of Wales, had died of fever just a few months before, and her younger brother, twelve-year-old Charles, had been in such poor health since infancy that few expected him to reach adulthood. (As things turned out, Charles did succeed his father as Charles I, but was doomed to be beheaded at the hands of Cromwell's Parliament.)

Frederick came to England at the end of 1612 for the wedding and fell for his bride at first sight. The celebrations ran on for months, extravagant even by the standards of royal weddings. The great poets of the day wrote rapturously of the couple, songs were composed and elaborate masques were written and designed by the greatest names. The

celebrated metaphysical poet John Donne wrote of Elizabeth:

Be thou a new star, that to us  
portends

Ends of great wonder; and be thou  
those ends.

Shakespeare's company, the King's Men, performed a series of plays at court during the months leading up to the wedding. His most overtly esoteric work, *The Tempest* (whose magician character, Prospero, was allegedly based on Dee) was performed on the betrothal night, 27 December 1612, with some additional scenes specially written for the occasion.

With a deft PR touch Frederick and Elizabeth of Bohemia were married on Valentine's Day 1613, after which the couple went to live in the romantic Heidelberg Castle in the Palatinate. Frederick had constructed for his

love what was regarded as the eighth wonder of the world, the famed *Hortus Palatinus*, an Italian Renaissance-style landscaped garden, laid out in deeply symbolic fashion, complete with mechanically animated statues, imported tropical plants and a celebrated water organ.

The appearance of the Rosicrucian manifestos in the two years immediately following the wedding was intimately bound up with the expectations of esoteric reform that centred on the couple. It was probably no coincidence that the Palatinate bordered on the Duchy of Würtemmberg, home to Johann Valentin Andreae. The works were preparing philosophical circles in Germany and beyond for the new era that they believed this golden couple would usher in – a unified Protestant Europe that would confront the ultra-Catholic nations.

Other events underscore the connection between the marriage and the manifestos.

Robert Fludd's works of that period were published in two volumes in 1617 and 1619 in the Palatinate (despite being written in England), as were Michael Maier's books (by the same publisher, in fact). Under Frederick and Elizabeth, the Palatinate became the centre of Rosicrucianism.

So we see that the Rosicrucian movement was a continuation of the Hermetic reform kick-started by Bruno and Campanella. The manifestos were gearing their readers up for the triumph of the Protestant reformers, personified in Frederick and Elizabeth, and the new golden age that they would usher in. This golden age would finally realize Christian Rosenkreutz's dream of an open and co-operative brotherhood of philosophers, working for the benefit and betterment of mankind. The signs are that they thought that 1620 would be the destined year of change. Sadly, although it would indeed be a notable year, it was one of utter disaster.

In 1619, Frederick V, the Elector of the Palatinate, accepted a new crown: the kingship of Bohemia. Seven years earlier Emperor Rudolph II – patron of Dee, Bruno, Maier and Kepler, among many others – had died, and the titles of Holy Roman Emperor and King of Bohemia had passed to his uncompromising cousin Ferdinand II, pawn of the Jesuits and a leader of the German Catholic League, which had been created specifically to oppose the Protestant Union. His crack-down on Protestants and Jews led the Bohemians to offer their crown to Frederick, head of the Protestant Union. Frederick accepted, and with Elizabeth, moved from Heidelberg to Prague.

Frederick and Elizabeth reigned in Prague for just twelve months, and were given the wonderfully romantic title Winter King and Queen of Bohemia, evoking beautiful but doomed heroes of a Hans Christian Andersen fairy tale. In the autumn of 1620 a

coalition of Catholic forces led by Ferdinand closed in, and after a bitter war at the Battle of the White Mountain on 8 November, the Bohemian forces were broken. The Rosicrucians could only watch. Meanwhile, outside forces presented them and the whole of Europe with the much wider horror of the Thirty Years War.

Like the curse of the mythical Fisher King, the end of the Wedding's great promise led to the devastation by war, massacre, famine and disease of large swathes of Germany. Protestantism and Jewry were wiped out in Bohemia. Frederick and Elizabeth, the iconic alchemical bride and groom and figureheads of the Protestant movement and Rosicrucian hopes, fled into exile at The Hague, where they maintained a semblance of grandeur on hand-outs from sympathetic relations. The Alchemical Wedding degenerated into bathos, the once-golden couple being sadly tarnished.



For a while it really seemed as if all Protestantism was about to be snuffed out. The Hapsburgs would rule Europe, allowing the Catholic Church to re-establish itself by 'punishment and pain', to draw on Bruno's all-too-accurate phrase. The future looked to be inescapably priest-ridden and grimly black with the smoke from the fiery pyres of martyrdom. The Hermetic reformers hurriedly regrouped. Rosicrucian mania in Germany abruptly ceased in the year that Prague fell. Campanella changed from opposing to advocating reform of the Spanish monarchy in the same year.

# THE INVISIBLES

The Rosicrucian craze then shifted to France. In 1623 notices appeared in Paris announcing that members of the 'College of the Brothers of the Rose Cross' were present in the city, on 'a visible and invisible stay', prompting the rather evocative nickname of the Invisibles – a sure carrot to dangle before all conspiracy theorists.

Announcing the presence of the Invisibles generated a Jesuit propaganda campaign whose hysteria matched that of a witch hunt. Here were members of a secret magical brotherhood – *sorcerers*, no less – abroad in the city, up to God knows what and only God would know what because they were invisible. Books and pamphlets speedily appeared warning that the Invisibles were part of a devilish plot. The anonymous but

presumably delightful *Horrible Pacts made between the Devil and the Pretended Invisible Ones* claimed that the Invisibles were part of a global Satanic conspiracy, that six groups of six members in different areas around the world were plotting mankind's downfall. Another pamphlet specifically named Michael Maier as their leader. The Jesuit François Garasse called them 'a diabolical secret society who should be broken on the wheel or hanged on the gallows'.<sup>17</sup>

If this seems all rather sensational, then no doubt that was the intention. After all, claiming to be invisible Rosicrucians was likely to provoke over-heated imaginings. The PR genius involved in whipping up this type of frenzy suggests that the notices were actually the work of Rosicrucian haters, or more accurately enemies of Rosicrucianism.

Why should anyone want to stir up anti-Rosicrucian paranoia, especially at that

particular place and time? As Parisian intellectuals became fervently hooked on the manifestos' furore and the works of their defenders such as Michael Maier, generating a major scare would have acted like a cold shower on potential new devotees. If all the hot air about pacts with the devil gave the impression that to dabble in Rosicrucianism would guarantee an eternity of being prodded by poker-wielding demons, then a similar fate would surely await them whilst they were still alive, care of the Pope's men.

It is unlikely to be a coincidence that in the same year the Hermetic tradition also came under a sustained onslaught in Paris, from Marin Mersenne, a Jesuit-educated monk. In works published from 1623 he attacked everyone from Pico della Mirandola onwards, reserving special hostility for Robert Fludd, with whom he engaged in a high-profile war of words. He wrote of Bruno that he had 'invented a new way of philosophy in

order to secretly fight against the Christian religion'.<sup>18</sup> And tellingly, Mersenne was the first to use Casaubon's re-dating of the Hermetica against its devotees.

# ‘FROM MAGIC TO MECHANISM’

The Invisibles scare and Mersenne's onslaught on Hermeticism also forms the unexpected backdrop to the rise of *the* arch-rationalist of the time, he who also set the tone for the coming age of science. The philosophy of René Descartes (1596–1650) would ultimately lead to the divorce between the magical and scientific components of the Hermetic tradition. But, importantly, his career also demonstrates how this divorce was due as much to the exigencies of the time as it was to a change in the intellectual direction.

Descartes was the Jesuit-educated French philosopher who argued that all physical phenomena could be reduced to, and

explained in, purely mechanical terms. 'Cartesianism' represented a 'shift from magic to mechanism'.<sup>19</sup> But he also introduced the idea of a duality between mind and body, the consequence of which we are still coming to terms with.

Although his work is usually portrayed as a reaction against religion, the target of his argument was actually Rosicrucianism. Descartes was certainly not viewed as an enemy of Catholicism at the time. Quite the opposite – his ideas were actively encouraged by at least one leading Catholic theologian because of the ammunition they provided the Church in its onslaught against Rosicrucianism and Hermeticism.

Indeed, in his young days Descartes had been something of a Papist swashbuckler; as a twenty-four-year-old he had fought with the Catholic forces at the Battle of the White Mountain of 1620 that smashed the hopes of the Alchemical Wedding. He entered Prague

with the victorious troops. It was when quartering during the long months of the previous winter that he heard talk of the Rosicrucians and – perhaps oddly for a Pope’s man – found himself interested in them. Realizing that the Fraternity’s ideals and principles chimed with his own developing ideas, Descartes tried to make contact with it. He failed, but while he was holed up at Ulm in the summer of 1620 he met the mathematician Johann Faulhaber, who had tried to approach the Fraternity as a would-be member and had some useful knowledge to share with him.

Descartes returned to Paris in 1623, and found himself in the middle of the ‘Invisibles’ scare. This threatened to be somewhat dangerous for him given that it was known that he had been interested in the Rosicrucians while in Germany. As the anti-Rosicrucian hysteria was threatening to turn into a lynchmob scenario, to save his skin, Descartes



made a point of denouncing the Rosicrucian 'calumny'.

As we saw earlier, in the vanguard of the opposition to Rosicrucianism in Paris and beyond was the monk Marin Mersenne (1588–1648), of the exquisitely named Order of Minims. As we have seen, he was the first to use Isaac Casaubon's re-dating of the *Hermetica* against the likes of Robert Fludd. Eight years Descartes' senior, he had been a fellow student at the Jesuit college at La Flèche in the Loire, and the two men were close friends.

Besides being a theologian, Mersenne was a mathematician and scientist, best remembered today for his work on acoustics and prime numbers, a dubious combination of interests for a devout Catholic at the time, as the Counter Reformation, and particularly the overthrow of Frederick V, had invested these subjects with a heavy taint of occultism. Mersenne was eager to rescue his fields

of interest from any suggestion of diabolism. In 1623, the year of the 'Invisibles' scare, Mersenne published *Famous Questions in Genesis*

(*Quaestiones celeberrimae in Genesim*), which despite its title was a fierce attack on the occult philosophy and its advocates, especially Pico della Mirandola, Ficino, Agrippa and, particularly, Robert Fludd. On the other hand, he was a defender of Galileo, and expressed some admiration for the intellect of Campanella, who he met in Paris when the Italian was under Cardinal Richelieu's patronage, although he dismissed his philosophy outright.

To Mersenne, Descartes' concepts were potentially an excellent way of ridding natural philosophy of any suggestion of the esoteric, so he encouraged him to publish and helped promote his work, effectively acting as his agent for his first book, *Meditations on First Philosophy* (*Meditationes de prima*

*philosophia*). Ironically, the full title was *Meditations on First Philosophy, in which is Demonstrated the Existence of God and the Immortality of the Soul* – Descartes wasn't as extreme a rationalist as he is often portrayed today. In fact, given Descartes' religious beliefs and Mersenne's support, the Cartesian revolution was, if anything, a Catholic reaction against Rosicrucianism and Hermeticism.

After Descartes, natural philosophy bifurcated into two camps, each advocating a different way of acquiring knowledge. There was the mechanist philosophy, in which everything could be reduced to and understood in terms of physical properties – the characteristics of bodies and the forces that act on them. On the other side was the Hermetic approach, which saw things more holistically, every imaginable thing being inextricably part of a great living whole. Ultimately, of course, mechanism won the day,

although it was by no means an overnight victory.

With the rise of Descartes' influence, the philosophy that had driven the Renaissance was at its lowest ebb, seemingly heading for complete extinction. In half a century Isaac Casaubon had challenged it historically, the Thirty Years War had dashed its hopes politically and now Descartes was undermining it philosophically. But this was not the end of the story. There were those who kept the Hermetic torch alight, even in the heart of Rome itself. And it was yet to see its greatest triumph in the scientific world.

## Chapter Four

[1](#) Fowden, p. xxii.

[2](#) Yates, *Giordano Bruno and the Hermetic Tradition*, p. 21.

- 3 From Thomas Vaughan's 1652 English translation of the *Fama*, reproduced in the appendix to Yates, *The Rosicrucian Enlightenment*, p. 238.
- 4 See Churton, *The Golden Builders*, pp. 105–17.
- 5 Yates, *The Rosicrucian Enlightenment*, p. 250.
- 6 Churton, *The Golden Builders*, p. 93.
- 7 *Ibid.*, p. 132.
- 8 Yates, *The Rosicrucian Enlightenment*, p. 47.
- 9 Churton, *The Golden Builders*, p. 131.
- 10 *Ibid.*, p. 143.
- 11 Theophrastus Bombastus von Hohenheim (1493–1541) – he adopted the name Paracelsus to show he was greater than Celsus, the Roman author of a classic encyclopaedia of medicine – was a Swiss botanist, herbalist and physician. He was heavily influenced by the works of Pico and Ficino, applying the principles of Hermeticism and talismanic magic to healing. His ideas about the combination and manipulation of the elements also led to him to alchemy. Some think that Christian Rosenkreutz was

intended to represent Paracelsus, despite the fact that the *Fama* explicitly says that he wasn't a member of the Rosicrucian fraternity, although adding that it did allow him access to the book containing their accumulated wisdom, the 'Book M'.

[12](#) Churton, *The Golden Builders*, p. 157.

[13](#) See Yates, *The Art of Memory*, chapters XV and XVI.

[14](#) Quoted in Yates, *The Rosicrucian Enlightenment*, pp. 101–2.

[15](#) *Ibid.*, p. 136.

[16](#) Purver, p. 223.

[17](#) Quoted in Tompkins, p. 86.

[18](#) Quoted in Yates, *Giordano Bruno and the Hermetic Tradition*, p. 445. (Our translation from the French.)

[19](#) Yates, *The Rosicrucian Enlightenment*, p. 113.

## CHAPTER FIVE

# **SIGNS,        SYMBOLS AND SILENCE**

One might be forgiven for thinking that as the Age of Enlightenment moved inexorably towards the Age of Science, Hermeticism was, if not actually dead then pretty much moribund. But in fact, for the most part, it just continued in disguise. For obvious reasons of self-preservation after the polarization of the Thirty Years War, most thinkers who were inspired by the Renaissance occult tradition downplayed that fact, while quietly continuing on their path. Others, meanwhile, took little care to be circumspect, and astonishingly, got away with it. These two approaches – covert and overt – were



respectively adopted by two of the seventeenth century's most remarkable minds: Gottfried Wilhelm Leibniz and Athanasius Kircher.

# TRUE CABALA

Leibniz (1646–1716) vies with his exact contemporary, Isaac Newton, for the title of the century's greatest intellect. His output covered every conceivable field of his day, from linguistics through engineering to biology, his mind leaping chaotically from subject to subject. In his lifetime he published about a dozen works, but most of his thoughts, ideas and discoveries were scattered in a vast number of papers, letters and half-completed books, the majority of which have yet to be published. Yet we do know something particularly significant about Leibniz: he was heavily rumoured to be at the very least a Rosicrucian sympathizer.

Leibniz's major contributions were in the increasingly important fields of

mathematics, logic and metaphysical philosophy. As he devised infinitesimal calculus in the late 1670s, at the same time as Newton, a protracted row between the two men erupted, with Newton accusing Leibniz of stealing his invention. In the end it was Leibniz's notation that became the standard. He also invented the binary system on which our digital world depends and without which, in fact, most of the modern world could not exist.

Like many intellectual giants of the time, Leibniz's career was an odd mix of science, philosophy and diplomacy. While working for Georg Ludwig, Duke of Brunswick, he even got involved in negotiations over the English Act of Settlement of 1701. This Act bestowed the crown on the descendants of the Duke's mother Sophia, establishing the run of over-stuffed and not always totally sane Hanoverian Georges on the British throne. Sophia, to whom Leibniz was mentor

and adviser, was the Electress of Hanover and daughter of the Winter King and Queen, Frederick V and Elizabeth Stuart. And so we find a rumoured Rosicrucian working for the family of the alchemical bride and groom – suspiciously neat.

Born in Leipzig, Leibniz's first job after receiving his doctorate in law was as an alchemist in Nuremberg, where he was rumoured to have joined a Rosicrucian society. There is probably some substance to the story, which was accepted, for example, by the French mathematician Louis Couturat, author of a 1901 study of Leibniz.<sup>1</sup> There are potential Rosicrucian connections with Nuremberg: in 1630 Johann Valentin Andreae tried to revive his *Societas Christiana* in that city, so there may still have been a coterie of fellow travellers there three decades later.

Not only did Leibniz practise as an alchemist, but his later works reveal a deep

familiarity with the Rosicrucian manifestos and with Andreae's writings. He proposed the formation of an Order of Charity and drew up its constitution – part of which is lifted directly from the *Fama Fraternitatis*.<sup>2</sup> So at a conservative estimate, Leibniz certainly had Rosicrucian leanings.

Leibniz's first major work, *Dissertation on the Art of Combination* (*Dissertatio de arte combinatoria*), published in 1666 when he was just twenty, is about the art of memory – although the non-occult version, simply as an aid for remembering. In the introduction, he acknowledges his debt to previous practitioners such as Bruno, and goes so far as to lift the term *combinatoria* from him.<sup>3</sup>

But did Leibniz, as many historians assume, completely abandon these interests when he realized mathematics and logic were the way forward? Certainly Leibniz's career did seem to be set to embrace all things

mechanistic. He devoted himself to absorbing the latest thinking – including certain of Descartes' then-unpublished writings – during a four-year sojourn in Paris on a diplomatic mission for the Elector of Mainz. During that time, in 1673, he took a trip to London, where he wowed the Royal Society with his innovative calculating machine and was duly made a Fellow.

But later Leibniz realized that the mechanistic approach was limited, writing to a correspondent two years before his death:

But when I looked for the ultimate reasons for mechanism, and even for the laws of motion, I was greatly surprised to see that they could not be found in mathematics but that I should

have to return to metaphysics.<sup>4</sup>

Any search for the source of Leibniz's metaphysical inspiration begins with his devotion to Marsilio Ficino's 'perennial philosophy'<sup>5</sup> – Hermeticism. Bruno's influence, too, filters directly through to Leibniz, possibly through the conduit of the Giordanisti.

Leibniz's search for a metaphysical explanation for 'ultimate reasons' led him to formulate his theory of monads, which, to put it politely, is a somewhat abstruse idea. His monads are a kind of metaphysical or spiritual equivalent of atoms, the indivisible building blocks from which everything in creation is comprised and which are attached to physical atoms. Monads all originated at the beginning of the universe and, since they can neither be created nor destroyed, all change consists merely of their transformation.

*Monad* is the Greek word for unity, and since the time of the Greek philosophers it has been used to describe basic units and first causes in many different philosophies – it is an important concept in Neoplatonism, for example. Leibniz's concept of monad, however, was directly influenced by Bruno.<sup>6</sup> As Frances Yates pointed out:

the Hermetic tradition.<sup>7</sup>

In the interests of self-preservation, Leibniz himself was reluctant to acknowledge the influence of the Hermetic tradition. On the one hand, in the volatile new climate after the Thirty Years War Hermeticism was tainted with the whiff of heresy and diabolism, almost entirely because of Bruno. On the other hand – and partly as a consequence of being tainted – the reputation of Hermes' system in scientific and intellectual circles had suffered, and it was beginning to look old-fashioned and misguided.

But even if Leibniz was wary of shouting it from the rooftops, his works quite clearly owe a major debt to the Renaissance occult philosophy. Even Leibniz's system of calculus evolved from this tradition. It developed from his quest to reduce everything, not just



scientific principles and laws but also religious and ethical questions, to a common symbolic language: a universal calculus. Building on the art of memory, both the classical and 'occult' versions, in order to establish a language of symbols or *characteristica universalis*, Leibniz envisaged a set of images to which all the fundamentals of knowledge could be reduced. This naturally necessitated the cataloguing and codification of all that was known, a growing eighteenth-century preoccupation. By manipulating and setting the symbols in different relationships, he believed that new discoveries could be made.

He specifically likened such a system to Egyptian hieroglyphs, which along with Bruno, he believed were used in a similar way. Leibniz also considered, but eventually rejected, Dee's innovative *monas hieroglyphica* symbol. The Cabala, too, was an influence, since it is based on the idea that

certain principles are present in all things. Leibniz even described his *characteristica universalis* as ‘true Cabala’<sup>8</sup> – hardly the words of a modern-style rationalist.

Eventually Leibniz came to realize that the best tools for the job were mathematical symbols. This realization then led to the development of his version of infinitesimal calculus, which he intended to be a first step towards the universal calculus. Although Leibniz developed his concepts in a mathematical and mechanical direction, in focusing on a universal calculus he was closely following Bruno, who had extended the esoteric art of memory to include complex techniques for combining the images held in the mind in different ways.

In addition to his formulation of the binary system, in this mode of thinking Leibniz was anticipating modern computer modeling, which is based on the idea that any system can be defined in mathematical terms,

reduced to values, variables and relationships that can be manipulated in the computer to predict how the system will behave under varying conditions. Leibniz laid the foundation for contemporary information theory, and also saw the potential for creating machines to do the hard work of combining his *characteristica universalis*. Not only did he invent mechanical calculating machines that could do basic arithmetical operations, but he also tried to design one for more complex algebraic calculations. He even conceived a device that used binary mathematics.

Is it a stretch to say that mathematical equations, the modern scientific use of formulae and even some of the basics of computer science comes from an occult idea? Clearly Leibniz himself saw his work that way, even defensively describing the *characteristica universalis* as ‘innocent magia’.<sup>9</sup> There is no denying Leibniz’s unique

contribution to mathematics and computer science – but significantly it may also be fair to say that these were largely inspired by the Hermetic tradition.

# EGYPT'S LAST STAND

In the midst of all the Hermetic reversals in fortune there seems to have been a last and perhaps desperate attempt to carry the tradition into the very heart of Rome in a way that would have made Giordano Bruno very proud.

As we saw earlier, in the 1580s Pope Sixtus V had presided over the raising of an ancient Egyptian obelisk in Saint Peter's Square to mark the final trouncing of paganism. But there was another episode of obelisk-raising in the 1650s and 1660s that was motivated by the exact opposite. Inspiration for the second wave can be traced back almost entirely to one man, another acknowledged genius of the age, one of those paradoxical figures who according to the usual simplistic view of the period should not really have

existed: the extraordinary Hermetic Jesuit Athanasius Kircher.

Kircher was a polymath and gifted mathematician – he has been called the ‘last Renaissance man’ and ‘the last man who knew everything’ – and is regarded by many as the founder of Egyptology. He was born in either 1601 or 1602 (he didn’t know which, although happily he knew his birthday) in Hesse-Cassel in Germany. After being educated at the Jesuit College in his hometown of Fulda, he entered the Society of Jesus in 1618.

There is no way that Kircher could not have been aware of the furore over the Rosicrucian manifestos. Not only were they published in Hesse-Cassel and widely debated during the 1610s and 1620s, but also the Jesuits spearheaded the opposition to them. And all the key Rosicrucian elements turn up in Kircher’s works – everything but the name, in fact.

In 1631, during the Thirty Years War, Kircher was forced to flee, swimming across the Rhine to escape Protestant forces. He made his way to Avignon where he taught mathematics in the Jesuit College, before becoming a professor of mathematics at the Society's most prestigious establishment, the Collegio Romano in Rome. By that time he was widely thought of as a brilliant mathematician and polymath, and had gained the confidence of the Pope. As befitted the 'last Renaissance man', Kircher studied medicine, besides being a great inventor and a musician. He experimented with the magic lantern and the projection of images. He was a geologist and fossil-collector whose intellectual curiosity was so great he ventured into the crater of Mount Vesuvius when it was threatening to erupt. Perhaps as a result of an association of ideas, he also designed firework displays. By any standards, Kircher's career was extraordinary. So much so, in

fact, that in 2002 a number of distinguished scholars convened at the New York Institute of the Humanities to debate ‘Was Athanasius Kircher the coolest guy ever, or what?’ They concluded that he was, no ‘whats’ about it.<sup>10</sup>

His work with microscopes led him to argue that little ‘worms’ propagate plague, the earliest statement of the germ theory of disease based on microscopic observation. He also calculated that the height needed for the Tower of Babel to reach the Moon would knock the Earth off its path through the skies, which was particularly interesting as he shouldn’t have acknowledged that the Earth had an orbit in the first place! He argued that animals would have had to adapt to life after the Flood, one of the first recognitions of evolution. But like Leonardo before him, there was an element of the joker about Kircher. He launched little hot air balloons with ‘Flee the wrath of God’ written underneath, and dressed cats up as cherubs.



He also designed – but mercifully probably never built – a katzenklavier, a musical instrument that produced a range of sounds when a semicircle of cats had pins stuck in their tails. It was clearly not a good idea to be a cat around Kircher.

But most of all, he was passionate about ancient Egypt. To him, deciphering hieroglyphs would reveal the language that God gave to Adam, bestowing all the secrets of the universe. Indeed, thanks to a book that Kircher encountered in the Jesuit college library during his training, his main obsession was hieroglyphs – which nobody could read then (and wouldn't until the discovery of the Rosetta Stone in 1799). Kircher became convinced he had made the longed-for breakthrough required to crack the code, although we now know that this was wishful thinking. However, his passion is one of the reasons he was so enthusiastic about getting involved in the re-erection of obelisks, as he lusted after

the opportunity to study their inscriptions. While he was a professor in Rome, Kircher even dispatched a student to Egypt to measure the Great Pyramid, inside and out, and to copy hieroglyphs from two standing obelisks in Alexandria and Heliopolis – probably not the quickest or easiest assignment the young man had ever been given.

Like most scholars in those days, Kircher was convinced that the hieroglyphs inscribed on temples, statues and obelisks embodied the wisdom and science of ancient Egypt. Surely it would only be a matter of time before a genius such as himself would claim to be the first to understand it all? In Avignon he benefited from a friendship with the astronomer and antiquarian Nicolas Claude Fabri de Peiresc, who had not only travelled to Egypt, but had also brought back various relics. As a fellow astronomer, Fabri de Peiresc was one of Galileo's correspondents who leapt to his defence. Less

understandably, he also publicly defended Tomasso Campanella.

Kircher's interest in the mysteries of Egypt naturally brought him into contact with Hermeticism, for which he made no attempt to hide his enthusiasm. But he completely ignored Casaubon's dismissal of the antiquity of the *Hermetica*, arguing that the texts represented the authentic philosophy, cosmology and religion of his beloved ancient Egypt. In fact, he not only tried to decipher and translate hieroglyphs but also attempted to relate them to the teachings of the *Hermetica*. He regarded Hermes as the inventor of hieroglyphs, and the inscriptions on the obelisks as the keys to unveiling his wisdom. He even called the Egyptian looped cross, the *ankh*, the '*crux Hermetica*,' or Hermetic cross.

Kircher was also an astronomer, and while he privately accepted Copernicanism, he was careful to state in public that he denied 'both

the idea of the mobility of the earth, and of the inhabitants of the other heavenly globes'.<sup>11</sup> The last part of this refutation suggests that it was Bruno, rather than Galileo, who he had in mind. In fact, Kircher's work often displays such close parallels with Bruno's that he *must* have read his works. It is hard to find anyone more in tune with Bruno's thinking: Kircher, too, believed his religion of Catholicism was heir to the Egyptian tradition, and he took Bruno's cosmology as the basis for his own. For obvious reasons, however, it would not have been a great idea to make this too obvious.

Kircher wrote voluminously, his masterwork being the four-volume *Oedipus Aegypticus*, published between 1652 and 1654, which contains a synthesis of all mystical and esoteric traditions, with Egypt squarely positioned as their foundation. And naturally, he acknowledged the significance

of the name of the sacred city of the Egyptians, Heliopolis, City of the Sun.

Kircher greatly admired the ancient Egyptian civilization, upholding it as the ideal model for both politics and religion. This understanding is very close to Bruno's vision of Egypt – dangerously so, one might have thought, for a Jesuit working at the very epicentre of Catholicism in Rome. After all, it does seem a perilously short step from believing that Ancient Egypt is the perfect model to advocating the reform of religion and state to match.

Kircher's other beliefs included the idea that Moses had been schooled in the religion of Egypt, which he had then passed on to the Israelites, who subsequently corrupted it. Again, this is dangerously close to Bruno's thinking. The suggestion here is that, given it was believed that Jesus had been sent to put the Jews back on the right track and to open their religion to the rest of the world, then he

was actually restoring the Jewish religion to its *Egyptian* roots. Kircher never made this line of thinking overt – after all, he of all people was no fool.

Not only is *Oedipus Aegypticus* liberally studded with quotes from the *Hermetica*, but Kircher takes both Hermes Trismegistus' authorship of those books and his antiquity for granted, believing him to be a contemporary of Abraham. He includes a hymn of Hermes from *Pimander*. To this he added, in the words of Peter Tompkins, 'a hieroglyph enjoining silence and the secrecy concerning these sublime doctrines – the colophon employed by the Brothers of the Rosy Cross!'<sup>12</sup> More overtly (and bizarrely), Kircher placed great importance on John Dee's *Monas hieroglyphica*, from which he frequently quotes, linking the symbol to the Egyptian *ankh*.

We can see that Kircher shared exactly the same ideals and influences as the authors of

the Rosicrucian manifestos, which seems decidedly odd given that the Rosicrucian movement was a Protestant expression of the Hermetic reform agenda. However, as the Hermeticists were working behind both Protestant and Catholic lines for a common cause, even a Catholic Hermeticist like Kircher would share a similar mindset with the Rosicrucians. Kircher's German background even suggests the possibility of a connection with the Giordanisti.

Maybe Kircher was trying to change Catholicism from within, reviving the old dream of Bruno and Campanella. This is by no means just idle speculation, as he managed to interest two popes in Egyptian ideas, and his work with the ancient obelisks points to more than a mere academic interest in those monuments. In this Kircher collaborated with his great friend, the artist, sculptor and architect Gianlorenzo Bernini (1598–1680), who is most famous for designing St Peter's

Square with its magnificent colonnades. (Or rather, he is probably most famous now for featuring so prominently in Dan Brown's *Angels and Demons*.)

Unsurprisingly, Kircher and Bernini's joint projects incorporated a wealth of Egyptian symbolism and motifs, which Bernini incorporated into his other works. George Lechner, an expert on magical and astrological symbolism in Renaissance art – a real-life version of Dan Brown's character Robert Langdon – acknowledges that Bernini's use of Egyptian motifs probably derives from the *Hermetica*.<sup>13</sup> Kircher and Bernini first worked together on a project, later abandoned, to re-erect a 40 foot (12 m) obelisk that had been discovered in a vineyard. Cardinal Francesco Barberini, the nephew of the then-Pope, Urban VIII, sought to set it up in his palace gardens.

Kircher and Bernini conceived that the base of the monument should feature a life-



size sculpted elephant, which would bear the upright obelisk on its back. But what did the elephant signify? Was it simply an error – did Kircher and Bernini perhaps believe elephants came from Egypt? The answer reveals something important about the men's otherwise concealed attitude to their religion.

In the twentieth century the Italian painter Domenico Gnoli, among others, identified the inspiration as an image in the allegorical and highly symbolic book *Poliphilo's Strife of Love in a Dream* (*Hypnerotomachia Poliphili*), published in Venice in 1499, an identification accepted by the American art historian William S. Heckscher.<sup>14</sup> The romance is anonymous, although the first word in each chapter spells out a sentence containing the name 'Frater Franciscus Columna', apparently the name of a Dominican monk in Venice. Despite this clue, other authors have been suggested, including Lorenzo de'

Medici and Leon Battista Alberti, the polymath mentor of Leonardo da Vinci.

The tale describes a dream within a dream in which Poliphilo ('Lover of many things' or 'Lover of Polia') searches for his *amorata* Polia, who has rejected him. Inevitably, throughout his adventures he encounters many strange creatures along the way, all illustrated by superb woodcuts. *Hypnerotomachia Poliphili* has exerted a hold over the esoteric imagination to this day, as it seems to convey a profound, if elusive, something in symbolic form. Decoding its hidden message provides the central plot of the 2004 bestseller *The Rule of Four* by Ian Caldwell and Dustin Thomason, and it is mentioned in Roman Polanski's powerful and unsettling movie *The Ninth Gate* (and in the novel on which the movie is based, *The Dumas Club* by Arturo Pérez-Reverte).

In the story of Poliphilo the obelisk on the back of a stone elephant is not only

described but also illustrated by one of the woodcuts. Before Gnoli identified it as Bernini and Kircher's joint inspiration, *Hypnerotomachia Poliphili* was already acknowledged by researchers of the esoteric as a major influence elsewhere. An Italian writer on Rosicrucianism, Alberto C. Ambesi, considered that it 'marks the true birth of the Rosy Cross, but in code'.<sup>15</sup> This was not to suggest that either the fraternity or the group that produced the manifestos existed in Venice in 1499, but that the currents of esoteric thought that came together in *Hypnerotomachia Poliphili* later influenced the Rosicrucians.

Although the obelisk-on-an-elephant project was aborted, the idea resurfaced in Kircher and Bernini's last collaboration.

Shortly after Pope Innocent X's election in 1644, Kircher proposed that another recently unearthed obelisk, broken into four pieces, should be re-erected in his honour. The first-

century emperor Domitian originally commissioned the 55 foot (16.5 m) obelisk (its height was nearly doubled by Bernini's elaborate fountain base) for Rome's Temple of Serapis. Innocent agreed to Kircher's proposal and put him in charge of the project, again working with Bernini. Together they reassembled the obelisk, Kircher designing the missing pieces – complete with inscriptions – and it became the centrepiece of the elaborate statue-covered Fountain of the Four Rivers in the Piazza Navona, which took until 1651 to complete. The obelisk was topped not with a cross, as one would expect, but a dove, which wasn't a reference to the Holy Spirit or dove of peace but to the emblem of Innocent's family, the Pamphili.

Kircher's own account of the raising of the monument, *Obeliscus Pamphilius*, begins with the mysteries of Egypt, and in particular the secrets of the hieroglyphs, but is again heavily studded with Hermeticism and even

includes a lengthy discussion of John Dee's *Monas hieroglyphica*. Incongruous to say the least for a book by a Jesuit commissioned by the Pope himself!

*Obeliscus Pamphilius* can be said to conceal almost as much as it reveals, and there is a strong suggestion running throughout that Kircher is still hiding something. The frontispiece has occupied esotericists and art historians for generations. In front of a fallen obelisk the winged Mercury (i.e. Hermes) hovers holding a scroll inscribed with hieroglyphs in front of a woman who represents Kircher's muse. She rests one foot on a cubic block of stone, on which Egyptian tools that are clearly the equivalent of the square and compass of the classic Masonic symbol are inscribed. This is most odd – historically and geographically Masonic symbols should not have been in Rome at that time. The frontispiece also features a cherub holding one finger to his lips. Tod Marder, professor at the

State University of New Jersey and a fellow of the American Academy in Rome, a specialist in the works of Bernini, writes:

Cabalistic in the extreme, Kircher claimed to be purposefully obscuring the real meaning of the obelisk, lest he deprive some other erudite soul of the enlightenment that comes from personal decipherment. Kircher wrote a book about the Pamphili Obelisk, as it was called. On the title page appears a little cherub with his forefinger raised to his lips to signal silence

– if you know the secrets herein, it seems to say, keep them to yourself. <sup>16</sup>

The symbolism of the frontispiece is obvious: through the inspiration of Hermes, Kircher is seeking to restore the great Egyptian secrets.

Kircher's charmed life continued when Innocent X died in 1655 and Fabio Chigi was elected as Alexander VII. Alexander was responsible for commissioning Bernini to remodel St Peter's square, with the Caligula obelisk as its centrepiece. Peter Tompkins describes the new Pope as:

an Hermetic scholar who took a personal interest in Kircher's hieroglyphical studies, contributing generously to the publication of Kircher's many more works, and so, indirectly, to keep-

ing alive the wisdom of Ficino, Pico, and their Thrice Great Master. <sup>17</sup>

The year of Alexander's election was also remarkable for a great discovery. During the digging of a new well, a smallish, 18 foot (5.5 m), pink granite obelisk in good condition was found in the garden of Santa Maria sopra Minerva, a remnant of the original temple to Isis. Having solicited the commission from Alexander, Kircher and Bernini had this set on the back of a stone elephant in the piazza in front of the basilica. With this accomplished, the statue was instantly recognisable as the outward and visible form of the woodcut from the *Hypnerotomachia Poliphili*. It appeared that Kircher and Bernini had finally manifested its extraordinary symbolism in hard stone.

The obelisk is topped with a small and discreet cross, as opposed to the unmissable ironmongery that Sixtus V set on the Caligula obelisk. This one, however, is devoid of smug trumpeting about the victory of Christianity. Instead it provides a perfect

reflection of Hermeticism such as Bruno's, proclaiming as it does that Christianity is built upon and supported by the ancient Hermetic religion of Egypt.

The obelisk's inscription is remarkable because it features a Pope honouring Isis, in what is perhaps an echo of the decorations that could be found in the Appartamento Borgia two centuries earlier: 'Alexander VII erected this obelisk once dedicated to the Egyptians' Pallas [Isis], to the divine wisdom and to the deipara mother.'<sup>18</sup> 'Deipara' means 'mother of God' (so 'deipara mother' is either tautology or heavy emphasis), and is an official Catholic title for the Virgin Mary. But why not make a more explicit reference to Jesus' mother, if she is being honoured here? Clearly because it is referring to Isis, not the Virgin Mary.

To Hermeticists, Santa Maria sopra Minerva was nothing less than a sacred site. Although outwardly a Dominican basilica, it



was also the spot where Bruno was taken before his execution and where Galileo abjured his heliocentric beliefs. So here we have an obelisk made in honour of Isis, raised again as part of a Rosicrucian monument by an adherent of Hermeticism, outside the place where Bruno had been condemned and Galileo forced to recant. This was not your average Catholic statue.

But there is still more to this elephantine sculpture, which recalls to us at least the quite jaw-dropping symbolism Leonardo built into his *Virgin of the Rocks*, which we discuss elsewhere.<sup>19</sup> As Peter Tompkins notes gleefully:

... the satirist Segardi, taking the symbolism one step further, used the fact that the elephant's rear end is turned towards the monastery of the Dominicans to compose the epigram, '*Vertit terga elephas vertague proboscide clamat Kyriaci Frates Heid Vos Habeo*' or,

in short, 'Dominicans, you may kiss my arse!'<sup>20</sup>

Few are afforded the opportunity to make such extravagantly heretical gestures, and indeed this was a last hurrah for Kircher and

Bernini. When Pope Alexander died in 1667, Kircher lost papal favour and patronage and resigned from the Jesuit college to concentrate on his intellectual pursuits. In particular he wanted to create a museum preserving artefacts (such as a lizard encased in amber) which he had collected and which Jesuits sent from around the world. With what seemed like destined precision, he and Bernini died on the same day, 28 November 1680.

With the huge confidence (many would say overwhelming arrogance) of a gifted polymath, Kircher, the self-declared Hermeticist and probably a closet Rosicrucian, worked right in the heart of Catholicism, hidden in plain sight. Had he attempted to carry out Bruno's apparently impossible idea of celebrating the compatibility between Hermeticism and Christianity? Surely strangest of all is his success in managing to operate

within the rabidly anti-Hermetic and Rosicrucian-hating Jesuit order.

Of course many readers will have noticed that this late flowering of Hermeticism within the Vatican is echoed in the plot of Dan Brown's second novel *Angels and Demons* (2000), as well as in its action-packed movie adaptation. In fact it was returning to the subject of Bernini and Kircher for our roles as contributors to the truth-behind-the-fiction TV documentary tie-in to the movie that led us to unravel many of the above connections.

The fictional basis of Brown's thriller is the supposed existence of a secret society of scientists and freethinkers called the Illuminati, created in the face of persecution by the Church and which boasted Galileo as a prominent member. Because of persecution, particularly Galileo's, the Illuminati became rabidly anti-Catholic, eventually seeking to bring down the Church, which they had

infiltrated. One of their secret grand masters was Bernini, who had encoded certain of his Roman works with directions to guide initiates to the Society's hidden base. The hero, Robert Langdon, has to follow the 'Path of Illumination' against the rapidly ticking clock in order to avert an enormous cataclysm and save the day.

As with *The Da Vinci Code*, Brown's grasp of history in *Angels and Demons* has been roundly criticized for its inaccuracies and anachronisms. On the surface, it does seem that liberties have been taken with the facts. Although there was a real secret society called the Order of the Illuminati, whose aims were roughly similar to the organization in *Angels and Demons* – the advancement of free-thinking and the overthrow of the Catholic Church – it wasn't formed until 1776 and was only active in the state of Bavaria. So on geographical and chronological grounds it was impossible for Galileo and

Bernini to have been part of it. Critics also focused in particular on the choice of Bernini as the secret Illuminati master, on the grounds that he was a dedicated Catholic who worked for most of his life under the patronage of popes.

The essentials of Dan Brown's story do fit with our own reconstruction, however. If you replace the Illuminati with the Giordanisti then the plot falls into place very neatly, as the latter secretly encouraged scientific thinking and aimed at either the radical reform or overthrow of the Catholic Church. And the Giordanisti was connected with Galileo and, through Kircher, to Bernini. Certain works of Bernini's that Brown used as a framework for Robert Langdon's apocalyptic trip to Rome, such as the Fountain of the Four Rivers in the Piazza Navona, one of the landmarks of the Path of Illumination, are also significant in our own version of the story. So if we substitute the 'Giordanisti' or

‘Rosicrucian’ for the Illuminati in Brown’s novel, we see that, perhaps surprisingly, there is a solid historical basis for *Angels and Demons*. (And perhaps it is significant that Bruno’s *On the Heroic Frenzies* culminates in a scene in which nine blind men receive not just sight but insight, becoming the nine ‘Illuminati’.) It seems that Dan Brown tapped into a rich vein of synchronicity and serendipity that sometimes, somehow makes life-follow-art-follow-art.

But what of the objection that Bernini was too Catholic to be involved in such shenanigans in the first place? Was he just an innocent fall guy for Kircher’s secret Hermetic agenda, as some have suggested? Neither of these objections stand up. As we have seen, even certain popes were devotees of Hermes, and strong Christian beliefs – be they Catholic or Protestant – presented no obstacle to developing an enthusiasm for the works of Thrice Great Hermes. Unless

Bernini lived in a bubble and never actually read Kircher's books, he must have understood that the symbolism of their joint works was unequivocally Hermetic.

More importantly, Kircher showed that Bruno's intellectual legacy was not only still alive but also still shaping the development of science. Ingrid D. Rowland, art historian and Fellow of the American Academy in Rome, writes:

Kircher's cosmology and its attendant concept of a universal *panspermia* ... show that however dramatically the eight-year trial and gruesome public execution of Giordano Bruno had been designed to prove that the heretic philosopher was a lone and terrible fanatic, the performance had failed. Bruno's books had been read by Kepler, Galileo and Athanasius Kircher, and they were enough to change the course of natural philosophy. For both Bruno and Kircher argued with passionate eloquence that nothing but an infinite universe did justice to an omnipotent God, and once the idea of that vastness immeasurable had been

conceived, it really did burst the crystalline spheres of Aristotelian physics. [21](#)

But Hermetic science still had one more giant to gift to the world whose contribution was to exceed anything that had gone before.

# Chapter Five

[1](#) Couturat, p. 131.

[2](#) Yates, *The Art of Memory*, pp. 387–8.

[3](#) *Ibid.*, p. 382.

[4](#) Quoted in the online Stanford Encyclopaedia of Philosophy: [plato.stanford.edu/entries/leibniz](http://plato.stanford.edu/entries/leibniz).

[5](#) Stanford Encyclopaedia of Philosophy website: [plato.stanford.edu/entries/cambridge-platonists](http://plato.stanford.edu/entries/cambridge-platonists).

[6](#) See Yates, *The Art of Memory*, p. 388, and Atanasijevic, p. xviii.

[7](#) Yates, *The Art of Memory*, p. 388.

[8](#) Quoted in *ibid.*, p. 385.

[9](#) Quoted in *ibid.*

[10](#) Strange Science website: [www.strangescience.net/kircher.htm](http://www.strangescience.net/kircher.htm).

[11](#) Quoted in Tompkins, p. 90.

[12](#) *Ibid.*, p. 97.



- [13](#) Interviewed in Burstein and de Keijzer, pp. 239–40.
- [14](#) See ‘Bernini’s Elephant and Obelisk’ in Hecksher. This is a reproduction of an article that appeared in *The Art Bulletin* in 1947.
- [15](#) Quoted in Tompkins, p. 88.
- [16](#) Tod Marder, ‘A Bernini Expert Reflects on Dan Brown’s Use of the Baroque Master’, in Burstein and de Keijzer, p. 255.
- [17](#) Tompkins, p. 97.
- [18](#) Quoted in Ingrid D. Rowland, ‘Athanasius Kircher, Giordano Bruno, and the *Panspermia* of the Infinite Universe’, in Findlen (ed.), *Athanasius Kircher*, p. 56.
- [19](#) See Picknett, *Mary Magdalene*, pp. 27–9.
- [20](#) Tompkins, p. 100.
- [21](#) Ingrid D. Rowland, ‘Athanasius Kircher, Giordano Bruno, and the *Panspermia* of the Infinite Universe’, in Findlen (ed.), *Athanasius Kircher*, pp. 201–2.

## CHAPTER SIX

# ISAAC                      NEWTON AND THE                      INVISIBLE BROTHERHOOD

After the collapse of Rosicrucian dreams in Bohemia and Germany and the eruption of the Thirty Years' War that engulfed Europe for a generation, Hermetic hopes for the great reform of society focused on England, which had remained largely uninvolved with the war, if only because Charles I's expedition of the late 1620s had been ignominiously defeated. And when he ran out of funds for another such venture, it was the issue of

how to raise money for the army that deeply divided the English.

The ensuing Civil War between the king and Parliament convulsed the country from 1641 until 1649, and ended with the public beheading of Charles I in London and the foundation of Oliver Cromwell's Commonwealth. The years of the Commonwealth and the Protectorate under Cromwell's personal rule, although largely miserable (Christmas was cancelled, for example), were relatively stable.

But before England endured its own upheavals, a number of scholarly refugees who cherished the Rosicrucian dream arrived. England quickly became the repository of the Hermetic reform movement.

The Hermetic tradition had by no means died out in the country. In 1654 John Webster – a Puritan Parliamentary chaplain, astonishingly – wrote a tract proposing that

the universities should base their teaching on ‘the philosophy of Hermes revived by the Paracelsian school’<sup>1</sup> – in other words, Rosicrucianism. He mentioned the Fraternity of the Rose Cross and strongly recommended John Dee’s mathematical works, as well as those of Robert Fludd.

Another important vehicle for the Hermetic tradition in England was a group of philosophers centred on Christ’s College, Cambridge, known some what misleadingly as the Cambridge Platonists, who were most active in the middle of the seventeenth century. They took the founding philosophy of the Renaissance and blended it with contemporary currents of thought, but at their core was the *philosophia perennis* of Marsilio Ficino – whose heart was Hermetic through and through.<sup>2</sup> One of their most prominent members, Henry More, wrote that his thinking derived from ‘the Platonick Writers, Marsilius Ficinus, Plotinus himself,

## Mercurius Trismegistus and the Mystical Divines'.<sup>3</sup>

Given that list it would be just as accurate, if not more so, to describe this group as the Cambridge Hermeticists, although most historians are content to maintain their bias away from the Hermetica and towards the Greeks. The Cambridge group was in effect the direct continuation of the Florentine Academy of Ficino, the brotherhood of Hermeticists that drove the Renaissance. As historians J. Edward McGuire and Piyo Rattansi demonstrated in the 1960s, the Cambridge Platonists mainly derived their philosophy from the *Corpus Hermeticum* via Ficino and Pico della Mirandola. In a 1973 essay on the Cambridge Platonists, Rattansi wrote that: 'It is now clear that the Neo-Platonism of Ficino and Pico was deeply intertwined with the magical doctrines of the *Corpus Hermeticum* and the later Neo-Platonists.'<sup>4</sup>

The Cambridge Platonists accepted Isaac Casaubon's dating of the *Hermetica*, but did not acknowledge that this invalidated the philosophy. Henry More regarded only those parts that reflected Christian teaching as 'fraud and corruption in the interests of Christianity',<sup>5</sup> and the rest as genuinely ancient. So, ironically, in More's view, in looking for the original, true theology, the *prisca theologia*, we should pay most attention to those aspects of the *Hermetica* that are the *least* Christian.

The philosopher regarded as the leader of the group, Ralph Cudworth, while accepting that significant parts of the *Hermetica* were Christian forgeries, challenged Casaubon's logic. Why did proving some of the Hermetic books to be fraudulent mean that all of them must be? He also argued that if the aim of the forgers had been to build a path into the Church for Egyptian pagans, it would have made more sense to either have adapted

genuine books of Hermes or incorporate the major themes of Egyptian thinking into their fakes. So, in Cudworth's view, enough of the underlying philosophy and cosmology remained to draw valid conclusions. And as we will see, his was very close to the current historical position.



# THE INVISIBLE COLLEGE

Among the distinguished refugees from the Continent, a key figure was the Polish polymath Samuel Hartlib (1600–62): Hermeticist, Paracelcist, promoter of Dee's mathematical and geometrical works and an astrologer. With his Europe-wide circle of correspondents and contacts he was an 'intelligencer', a sort of one-man clearing house for information. He was a devoted networker in the interests of dissemination of all knowledge, from the intellectually obscure to the political – rather like Gian Vincenzo Pinelli in Padua during Bruno's day.

Hartlib was clearly a Rosicrucian. He worked to found a 'pansophic college' – an institution for the study of all-embracing wisdom, the acquisition of knowledge and its use for the betterment of society. Together

with fellow traveller John Amos Comenius (1592–1670), a Czech scholar who also took refuge, briefly, in England, he proposed setting up a Collegium Lucis, or College of Light, for the advancement of learning, but primarily to train up a body of ‘teachers of mankind’.<sup>6</sup>

Apart from being influenced by Andreae and the ideal of a learned society working for the advancement of humanity, he took the name for his projected movement, ‘Antilia’, from Andreae’s utopian work *Christianopolis*, which uses the word as a reference to an inner group within his perfect society. Presumably inspired by this was the utopian tale Hartlib wrote, a short pamphlet entitled *A Description of the Famous Kingdome of Marcaria* (1641). However, his Rosicrucian connection is made most explicit in his letter he wrote to one of his chief correspondents, John Worthington (1618–71), Master of

# Jesus College, Cambridge – and one of the Cambridge Platonists:

The word Antilia I used because of a former society, that was really begun almost to the same purpose a little before the Bohemian wars. It was as it were a tessera of that society, used only by the members thereof. I never desired the interpretation of it. It was interrupted and

destroyed by the following Bohemian and German wars.<sup>7</sup>

A tessera is a piece of a mosaic, but as the word was also used in ancient Rome to refer to a ticket, voucher or token, Hartlib seems to be hinting that ‘Antilia’ was the code name Rosicrucians used to recognize each other. This kind of knowledge implies he was himself a member. Yet another clue lies in the fact that his patron was Elizabeth of Bohemia who, as we have seen, together with her husband was the focus of intense Rosicrucian support.

Try as he might, Hartlib failed to get his projected pansophic college off the ground, writing despairingly to Worthington in October 1660: ‘We were wont to call the desirable Society by the name of Antilia, and

sometimes by the name of Macaria, but name and thing is as good as vanished.’<sup>8</sup> Like many other academics and intellectuals who had flourished under the Commonwealth, he had probably simply lost favour at the restoration of the monarchy.

But a month later came the first meeting of what was to become the Royal Society. And it seems that, wherever the initial idea came from, there was an attempt to use it to achieve the ‘Antilian’ dream.

The train of events that led to the foundation of the Royal Society is more complicated and more esoteric than many modern writers would have us believe. Despite the restrictions of the ongoing Civil War, it began in London in 1645 with an informal meeting of scholars who set out to explore new ideas in natural philosophy – as science was then called. In what was almost certainly no coincidence, the two prime movers were in the retinue of the exiled Charles Louis, Elector

Palatine, Frederick and Elizabeth's son. The two were Charles Louis' secretary, Theodore Haak, and his chaplain, John Wilkins. Charles Louis had been invited to live in London by Parliament, whose cause he backed. All very odd for the son of a Stuart – especially given that he was the nephew of the king who Parliament was fighting against.

John Wilkins – the future Bishop of Chester, inventor of the metric system and something of an oddball for a Church of England chaplain – was really the driving force behind the formation of the Royal Society. At the age of forty-two, the highly ambitious Wilkins married Cromwell's sixty-three-year-old widowed sister, presumably a move that did nothing to prevent his inexorable rise. He also wrote a defence of Copernicanism in 1641 (*Discourse Concerning a New Planet*), and more creatively, a flight of fancy with the self-explanatory title, *The*

*Discovery of a World in the Moone* (1638). His attempt to introduce a new universal language to be used by natural philosophers instead of Latin was terminally halted when his entire print run was lost in the Great Fire of London.

In his hugely popular book *Mathematicall Magick*, published in 1648, Wilkins specifically references the *Fama Fraternitatis*. His book was based – as he freely acknowledged – on mathematical works by Dee and Fludd and even declared that he took the title from Cornelius Agrippa.

It was at this juncture that the now-famous references to an ‘Invisible College’ appeared. These were in letters written in 1646 and 1647 by one of the most eminent founders of the Royal Society, the chemist Robert Boyle (1627–91) – credited with turning alchemy into chemistry – who alluded to a gathering of scholars and philosophers of

which he was a part and which called itself by this mysterious name.

Not only was the intriguing term 'invisible' used in the Rosicrucian manifestos, but it carried clear echoes of the mysterious, even sinister, 'College of the Brothers of the Rose Cross', otherwise known as the 'Invisibles' in Paris. Boyle's comments were almost certainly a kind of Rosicrucian in-joke.

Many writers have seen a connection between this enigmatic group and the founding members of the Royal Society, and hinted at the existence of an anonymous behind-the-scenes cabal. But maybe too much mystery has been read into these connections since the group Boyle refers to is relatively easy to identify. Historian Margery Purver, in *Royal Society: Concept and Creation* (1967), shows that the Invisible College was the circle centred on Hartlib.

The references to the Invisible College appeared in letters that the young Boyle wrote to Hartlib and make the connection between Hartlib and the activities of the college very explicit. On 8 May 1647 he wrote: ‘You interest yourself so much in the Invisible College, and that whole society is so concerned in all the accidents of your life ...’<sup>9</sup> In other correspondence from around the same time, Boyle calls Hartlib the ‘midwife and nurse’ of the college.<sup>10</sup>

The Invisible College was Hartlib’s Antilia, or more accurately the group of learned men he hoped would become Antilia. Considering this in combination with the ‘invisible’ clue suggests that it is essentially a Rosicrucian brotherhood. However, this doesn’t mean the connection with the Royal Society is nonexistent: Hartlib hovers in the background during its inception and at least initially it embodied his Rosicrucian ideals. And



significantly, Boyle was one of the most active founder members.

# THE ROYAL SOCIETY

As John Gribbin points out in *The Fellowship* (2005), the Royal Society was the result of two groups coming together. The first was a group that had met informally in John Wilkins' rooms at Wadham College, Oxford, from 1648 and throughout the years of the Commonwealth and which included Boyle and Christopher Wren. The second consisted of royalists with an interest in natural philosophy returning from exile with the Restoration in 1660. The two groups met when attending a series of open lectures at Gresham College in London.

At a meeting on 28 November 1660 a group of twelve natural philosophers and enthusiastic amateurs – including Boyle, Wilkins and Wren and led by William, Viscount Brouncker – decided to form a society

for promoting the emerging ‘experimental philosophy’, or what we now know as the scientific method, using experiment to test hypotheses. They took as their motto ‘*Nullius in verba*’, literally ‘on the word of no one’, but ‘take no one’s word for it’ certainly has a more modern ring.

The new society was particularly inspired by the work of Francis Bacon (1561–1626), the English courtier, lawyer and philosopher. His major work is the 1605 book *The Advancement of Learning*, which, as its title suggests, surveyed the state of scholarship in his day and proposed ways in which natural philosophers might extend their knowledge. He argued for a methodical and systematically organized approach to investigating the natural world, and also called for a united international ‘fraternity in learning and illumination’.<sup>[11](#)</sup>

Historians long regarded Bacon as the archetypal voice of reason, a beacon of light in

an age of superstition, but in 1957 the Italian historian Paolo Rossi's *Francis Bacon: From Magic to Science* challenged this long-held view. From closely examining Bacon's life and work, Rossi showed that he was as much a devotee of the Hermetic tradition as the other thinkers we have so far discussed. Rossi notes in particular the 'influence of the hermetic doctrine' on Bacon's ideas on the nature of metals.<sup>12</sup> He also, according to Rossi, firmly believed in the *anima mundi*. Basically the great man was another passionate disciple of the Renaissance occult philosophy (although he wanted to reform that, too). He included natural magic, astrology and, particularly, alchemy, within his fields of knowledge. He was just careful not to draw attention to them.

Ernest Lee Tuveson observed that Bacon's 'conception of natural processes owes much to hermeticism, and other traditional [i.e. esoteric] sources',<sup>13</sup> and asked why he

therefore condemns the likes of Dee, Fludd and Paracelsus. He concludes that, although Bacon shared their underlying philosophy, he disagreed about the methods that should be used to put it into practice, advocating the application of objective reasoning instead of magic. However, we can suggest a rather more expedient, if not cynical, motive: Bacon was in need of the king's favour, and was all too aware that there were certain subjects that were best avoided.

King James I, offspring of the doomed Mary, Queen of Scots was a weird little man with a paranoid terror of witches and would go to any lengths to protect himself from the threat of witchcraft, real or imagined (mostly the latter, but your innocence would hardly matter if you were accused and condemned and rolled down a hill inside flaming barrels on his orders). It was James' horror of all things occult that had been responsible for Dr Dee's decline.

In many ways Bacon was Dee's successor, another man of many talents who was involved in court and diplomatic activity under the patronage of the monarch. He rose to prominence at court immediately after Dee's fall from grace, for example producing the masque performed on the day following the wedding of Princess Elizabeth and Frederick V of the Palatine. But rising to prominence in those days was no guarantee of a long happy life – one had to work at it constantly, which usually involved shameless amounts of regal boot-licking.

Bacon was a highly ambitious man. As Arthur Johnston notes in his introduction to a 1973 edition of *The Advancement of Learning*, Bacon's life was 'a long pursuit of political power'.<sup>14</sup> In practice this meant mounting a campaign to attract the king's attention and favour – which certainly worked. In fact, as Jerome R. Ravetz, lecturer in the History and Philosophy of Science at the University

of Leeds cautions: 'All Bacon's published writings are propaganda; their function was to convert his audience, and their relation to his own private views was purely incidental.'<sup>15</sup>

Bacon enjoyed a succession of appointments at court that culminated in his elevation as Lord Chancellor in 1618. As an appeal to James I's intellectual pretensions, *The Advancement of Learning* opened his campaign of self-advancement and eventually earned him a job putting his proposed reforms of learning and education into practice. Fittingly, the very first paragraph includes the hardly subtle appeal for 'the good pleasure of your Majesty's employments'.<sup>16</sup> The book was addressed directly to James, whom he overtly flatters: 'There hath not been since Christ's time any King or temporal Monarch, which has been so learned in all literature and erudition, divine and human.'<sup>17</sup> Bacon certainly knew how to lay it on

with a trowel, echoing Bruno's wildly over-the-top flattery of James' predecessor, Elizabeth I. More interestingly he dared to liken James to Hermes Trismegistus:

Your Majesty standeth invested of that triplicity, which in great veneration was attributed to the ancient Hermes; the power and fortune of a king, the knowledge and illumination of a priest, and the learning and universality of a philosopher.

[18](#)

This particular description of Hermes is taken from Marsilio Ficino – which presumably Bacon relied on James not knowing.

Bacon's call for a 'fraternity in learning and illumination' may have influenced the authors of the Rosicrucian manifestos, but if so he was also influenced in turn by them. There are clear signs that he was familiar with the *Fama Fraternitatis* in his utopian *New Atlantis*, published in 1627, the year after he died, and which was a particular influence on the Royal Society's founders. Bacon seems also to have read and digested Campanella's *City of the Sun* (published four



years earlier) – or maybe it is a coincidence that his plot, too, involves shipwrecked sailors encountering the inhabitants of a perfect society (the preservers of an early, pure form of Christianity, whose officials wear white turbans bearing red crosses)?

Given Bacon's unofficial interests, it is rather ironic that he is seen to represent the beginning of the divergence of magic and science.

A more elusive and unequivocally arcane influence on the origins of the Royal Society was Freemasonry. Although the origins of Freemasonry are still controversial and obscure, whatever its roots it had certainly emerged as a significant institution by the mid-seventeenth century. Many historians have seen the Brotherhood as a repository of the Hermetic tradition, though this is not to suggest that Freemasonry is only about Hermeticism.<sup>19</sup>

Significantly, Masonic writer Robert Lomas points out that one of the rituals an initiate undergoes when entering the second degree makes specific reference to the heliocentric theory: 'The sun being at the centre and the Earth revolving around the same on its own axis ... the sun is always at the meridian with respect to Freemasonry.'<sup>20</sup> Even in the mid-seventeenth century heliocentricity was still not fully accepted – and in Catholic countries was an outright heresy – so the Masons' emphasis is all the more telling.

A Masonic influence on the early Royal Society is now generally acknowledged, but its extent and significance are more controversial. However, what is less contested is that the Society's main connection with the Freemasons was embodied by one of the driving forces behind its foundation – the man who secured its royal patronage, Sir Robert Moray (1609–74). His Masonic initiation in

1641 has the distinction of being the first to be recorded on English soil.

Described by Lomas as ‘a first-rate fixer and born survivor’,<sup>21</sup> Moray was a mixture of James Bond and soldier of fortune – but with mystical trappings. His origins remain obscure, but he first made his mark as a member of the Scots Guard of Louis XIII’s army in 1633, when he spied for Cardinal Richelieu. He then turns up as the quartermaster of the Scottish Covenanters’ Army when it marched on England in 1640. This campaign was part of the struggle over control of the Church in Scotland during which the Scots occupied parts of northern England including Newcastle, where Moray was initiated into a Masonic lodge on 20 May 1641. It is generally thought that he used his Masonic connections for intelligence work. After the end of the Covenanters’ campaign, he returned to the French court for yet more soldiering and spying and eventually

established himself as an emissary between the French court and that of Charles I, who knighted him in 1643. Moray went on to become the King's secretary, and after Charles' execution he joined Charles II's exiled court in Paris and became heavily involved in the negotiations to set him on the restored English throne.

With the monarchy restored, Moray based himself in London, where he became one of the twelve that formed the nucleus of the Royal Society. At their second meeting in December 1660, he took the encouraging news that the King approved of their aims and was prepared to give the society his royal endorsement.

However, all was not well within the ranks of the early Society. It is evident that there was a struggle behind the scenes between those who followed a more Hermetic/Rosicrucian model of a learned society and those who shared Bacon's vision. The

Hermetic version lost. This happened during the securing of the royal charter, which is normally portrayed as a simple intervention by Sir Robert Moray, enthusiast for the project and close friend of the King. But papers lost for three hundred years and rediscovered in the mid-twentieth century reveal a welter of plotting behind the scenes. Prime mover in this was Baron Skytte, a Swedish nobleman and confidant of King Karl Gustav, who was in London to promote the creation of a Protestant Alliance. Also interested in the new learning, Skytte attended the lectures at Gresham College.

On 17 December 1660, Hartlib wrote to John Worthington that since his last letter of ten days before:

I have recd some other papers, that have been confided tome, holding forth almost the same things as the other Antilia (for be not offended if I continue to use this mystical word) but, as

I hope, to better purpose. 22

These papers, he goes on, were sent to him by Skytte, and comprise:

... the propositions which were made to his Majesty by the Lord Skytte, and ... a draught for the royal grant or patent wch is desired for the establishment of this foundation. Thus much is certain, that there is a meeting every week of the prime virtuosi, not only at Gresham College, in term time, but also out of it ... They desired that his Maj leave that they might thus meet or assemble ymselves at all times, wch is certainly granted. Mr Boyle, Dr Wilkins, Sr

[23](#)

Paul Neale, Viscount Brouncker are some of the members.

Skytte had evidently resurrected Hartlib's plans. However, although Boyle supported them, they ultimately failed because of opposition from other founding members. Hartlib wrote to Worthington in April 1661, 'There becomes nothing of Lord Skytte's business, & I believe the other virtuosi will not have it that it should go forward.'[24](#) After the royal charter was granted in July 1662, Skytte returned to Sweden, and Hartlib died the following year.

In response to the Royal Society's publication in 1667 of its early official history, by Thomas Sprat – later Charles II's chaplain and the Bishop of Rochester – Worthington

railed that the society was ‘materialistic and for nothing but what gratifies externall sense.’<sup>25</sup> His outburst underlines criticism that the Society had failed to realize its full potential because it had rejected the more philosophical and metaphysical elements championed by Hartlib and Baron Skytte. The essential difference between the two visions of a learned society is that Hartlib’s had the reforming aspect that went back to Andreae and the Rosicrucian manifestos, and beyond that, to Campanella – and ultimately to Bruno.

One wonders exactly why a society, no matter how well connected, would be in quite so much of a hurry to rush out its official history, just seven years after it was founded. Their haste may represent a desire for the victors to etch their triumph in the minds of its readers, but it also suggests the promotion of a version of events that was economical with the truth.

Another sign of the Royal Society's Hermetic eclipse was the sidelining of John Wilkins, the man who had started the club at Wadham College and a Rosicrucian-friendly founder. Although he was appointed as the Society's secretary, he shared this role with a newcomer, the German-born theologian and diplomat Henry Oldenburg, and was soon marginalized.

Was the struggle over the direction and control of the Royal Society simply about the scientific philosophy it should adopt? In fact there appears to have been more to it even than that. One result of the organization of the new Society was that Oldenburg, as its foreign secretary, inherited Hartlib's network of correspondents, and he undoubtedly used his position for intelligence-gathering of a more politically sensitive kind.

Robert Hooke, the Royal Society's curator responsible for organizing experiments, complained that Oldenburg 'made a trade of



intelligence'.<sup>26</sup> In fact, he used his network for gathering not just scientific but also political information, the latter on behalf of the Secretary of State, Lord Arlington, even arranging for all the Society's correspondence from abroad to be delivered to the office of Arlington's under-secretary. Oldenburg was imprisoned in the Tower of London for two months as a suspected spy during the Anglo-Dutch war of 1667, only being released when peace was made.<sup>27</sup>

As Sir Robert Moray was also a spy, this raises the question of whether one reason the Royal Society was created was as a cover for intelligence-gathering. After all, it still remains unclear why Charles II was quite so interested in the Royal Society. What was in it for him? Although this suggestion might seem absurd, bear in mind that in its early days the Society was not the celebrated and distinguished institution it is today. It was only when Isaac Newton became its

president in 1703 (his presidency lasted for twenty-four years) that it could bask in his immense prestige. John Gribbin writes that by the end of Newton's tenure the society has completed 'the process whereby a gentleman's club became a pillar of the establishment'.<sup>28</sup>

If the reforming side of the Hermetic tradition had been extinguished by the time the Royal Society came into being, its influence over the scientific revolution had not waned. And it reached its final, and greatest, flowering in the person of 'the most outstanding scientific intellect of all time',<sup>29</sup> Isaac Newton.

# THE GREATEST SCIENTIFIC GENIUS

Isaac Newton (1642–1727) is widely regarded as the greatest scientific genius in history, and his masterwork, *Mathematical Principles of Natural Philosophy* (*Philosophiae naturalis principia mathematica*) – usually known, reverentially, simply as the *Principia* – is deemed the single most influential book ever written. His elucidation of the laws of motion and of gravity effectively *created* the modern world: mechanics and most forms of transport, including space travel, would be impossible without them. Newton even created the mathematical system, infinitesimal calculus, needed for his work – in itself no small achievement. After all, this and most other books would never see the light of day if writers had first to

invent laptops – or, more appositely, writing itself. But Newton had the vision to know what he needed to be great, then went ahead and made it all happen with the unswerving, if often anti-social dedication of genius.

Despite being from a humble background, Newton managed to rise to fame and fortune. He was the only child of a Lincolnshire farmer who had died by the time he was born on the farm at Woolsthorpe near Grantham, in the first year of the Civil War, on Christmas day 1642. He was a sickly child, and for his whole life he would be a solitary soul. From the age of three Newton was brought up by his grandmother, his mother having married the rector of a nearby parish. He hated her and his stepfather for abandoning him and went so far as to threaten to set fire to their house with both of them in it, but the rector's relative wealth would in the end prove useful to him.

From the beginning, Newton was fascinated by mechanics and delighted in making machines such as a mini mouse-powered windmill. He was entranced by how things worked. A life-changing moment came at the county fair when he bought a prism from an itinerant salesman, which stimulated his obsession with the phenomena of optics. Naturally, he was expected to be a farmer like his father, but when he was twelve an understanding uncle – a Cambridge graduate – realized that was not his destiny and secured him a place in a school at Grantham, where he also had to work as a servant to wealthier students.

John Gribbin describes one of Newton's early practical jokes:

He ... caused one of the earliest recorded UFO scares by flying a kite at night with a paper lantern attached to it, thereby causing 'not a little discourse on market days, among the

country people, when over their mugs of ale'. [30](#)

Newton was also not afraid of experimenting on himself. On one occasion he stuck a bodkin behind his eye to test its effect on his eyesight. On another he stared at the sun until he almost went blind – mercifully the effects were only temporary. Some might think he carried being a genius to a ludicrous degree.

Newton won a scholarship to Trinity College, Cambridge, in 1661, where he seemed merely an average student. Little from his time at Cambridge suggested the historic genius he would become. In 1665, just after he graduated, the college closed because of the plague that was sweeping the country, and he returned to Woolsthorpe for two years. What was a disaster for so many actually ended up being the making of Newton. It was at Woolsthorpe that he experimented with the prism, unravelling the secrets of light. It was also there that he devised the calculus, which he termed ‘fluxions’.

And momentarily, it was also at Woolsthorpe that he first began to think about the problem of gravity. The story of the falling apple stimulating his thinking of about gravity was Newton's own. The apple tree is still there – the original was cut down long ago but a new one grew from the stump. He realized that whatever caused apples to fall also kept the Moon in its place and determined and governed the motions of the other planets, and therefore the Earth. It would take him twenty years and a radical shift in his thinking to refine and build on his original intuition.

Once the plague was over, Newton returned to Cambridge as a Fellow, and became professor of mathematics in 1669, at the age of twenty-seven. Immediately this caused a problem. At that time newly elected Fellows had to be ordained priests (Anglican, of course) although Newton argued – ultimately to Charles II, who had to approve the

appointment – that he should be exempt from this rule.

Although Newton was deeply spiritual, he kept his beliefs so private that even today no one is certain what they were. But the very fact he was so circumspect – and had challenged the ordination rule – suggests that his beliefs were at odds with the dogma of the Church of England. Newton certainly seems to have been a Christian but of a heretical kind, although there is no consensus about its exact nature. Ironically for a Fellow of Trinity College, he definitely doubted the doctrine of the Trinity, as he wrote a book about it that he wisely decided not to publish. He seems to have doubted that God and Christ were ‘of one substance’, and may even have regarded Jesus as non-divine. He refused the sacrament on his deathbed.

Newton first attracted the attention of his peers through his pioneering work on optics and light, for example inventing the first



practical reflecting telescope, using a mirror instead of a lens. As a result, he was elected a Fellow of the Royal Society in 1671. It was at the end of the decade that he returned in earnest to his research into gravity, prompted by a dispute with Robert Hooke.

Newton poured all his thoughts and the results of his experimental work into his monumental achievement, the *Principia*, begun in 1684 and published three years later. The full title of the *Principia* was itself revolutionary, since it declared that natural philosophy was explicable and expressible in mathematical form. Astronomers such as Copernicus and Kepler had used mathematics and geometry, and Galileo had taken their application a step further, but to Newton mathematics was at the very heart of science.

One consequence of the *Principia* was the final proof of Copernicus' theory. Newton demonstrated that his theory of universal

gravity accounted for Kepler's laws of planetary motion, which was in turn derived from Copernicus' heliocentric model. This was the great watershed in the history of cosmology: after the *Principia* was published, it was impossible to doubt the heliocentric theory. To Bruno, of course, this would have represented only a partial success. Global acceptance of heliocentricity was due to usher in a golden age of universal Hermeticism, after all. But things had changed since Bruno's day ...

The *Principia* was an immediate sensation, although rather like Stephen Hawking's *A Brief History of Time*, it was 'one of the least-read bestsellers of the age'.<sup>31</sup> After it was published Newton moved to London, where he became a celebrity, albeit a rather reclusive and curmudgeonly one. He was knighted in 1705 by Queen Anne – the first 'scientist' to be honoured in this way. Both she and her successor George I would heap

great honours on him. Newton became Warden of the Mint in 1696, then Master of the Bank of England, and was elected President of the Royal Society in 1703, a position he retained until his death. He was a Member of Parliament for two short periods. When he died in 1727 it was a cause for national mourning. In honour of the occasion of his state funeral in Westminster Abbey, the poet Alexander Pope penned the famous lines:

Nature, and Nature's Laws lay hid  
in night:

God said 'Let Newton be!' and all  
was light.

However, Newton was anything but the sort of materialist-rationalist so prevalent today among the ranks of scientists, who believe all spirituality is a form of superstition. It is now well known that Newton's major

preoccupation was not gravity or the laws of motion or optics, but alchemy. The first biography that mentioned this was in 1855 but even after that it was a subject that was glossed over fleetingly and apologetically. More recently, however, historians of science have begun to acknowledge that Newton's esoteric interests did not only play a vital part in his thought processes, but also actively assisted him in making his great discoveries.

Richard S. Westfall, Professor of the History of Science at Indiana University and author of a major biography of Isaac Newton, wrote in 1972:

One lively and active facet of the lively and active enterprise that is Newtonian scholarship today is the continuing revelation of the presence in Newton's mind of modes of thought long

deemed antithetical to the modern scientific mind.<sup>32</sup>

One of the first to realize the importance of Newton's esoteric side was John Maynard Keynes, the leading twentieth-century

economist and great collector of Newton's alchemical writings, who in a paper read to the Royal Society in 1946 commented that 'Newton was not the first of the age of reason. He was the last of the magicians ...'<sup>33</sup> He went on (his emphasis):

Why do I call him a magician? Because he looked on the whole universe and all that is in it as *a riddle*, as a secret which could be read by applying pure thought to certain evidence, certain mystic clues which God had laid about the world to allow a sort of philosopher's treasure

hunt to the esoteric brotherhood.<sup>34</sup>

On Newton's death, 169 books on alchemy were found in his personal library – making up one-third of his collection. In fact, it transpires from all his writings that his main esoteric preoccupation was the quest for the philosopher's stone, and he was particularly fascinated by the work of the French alchemist Nicolas Flamel (c. 1330–1418).

Most of Newton's alchemical papers – of which he produced a vast number, over a million words – collected by Keynes and others, are now in Jerusalem, in the Jewish

National Library. As befits the work of a genius with a need to be secretive, they are written in elaborate codes, and many of them have yet to be deciphered.

Alchemy was against the law, and could even attract the death penalty (although in a curious excess of official spite, alchemists were to be hanged on gilded scaffolds adorned with tinsel, so at least their demise was pretty in a trashy sort of way). Legal disapproval existed not for reasons of religious intolerance, or because alchemy was considered fraudulent, but because of the fear that alchemists might succeed in making gold, and thereby undermine the economy. So it is an exquisite irony that the Establishment saw nothing wrong in putting Newton – an alchemist to his gilded fingertips – in charge of the Bank of England and of the Royal Mint, even entrusting him with the re-minting of the entire currency in the 1690s.

Like many esotericists before and after him, Newton was a great believer that the earliest civilizations, such as Egypt, knew more than people in his own day – that they possessed the *prisca sapientia*, or ‘ancient wisdom’. He was in no doubt that the Greeks had learned everything they knew from the Egyptians. He also believed that the Bible was one of the sources of the ancient wisdom, and that it contained prophecies relevant to his own time, particularly in the Book of Revelation. Besides studying many other ancient temples and buildings, he was fascinated by the Temple of Solomon, and devoted considerable energy to the study of its design, dimensions and proportions, which he believed incorporated ancient truths.

Like many thinking people of the post-Renaissance world, Newton was also particularly interested in Rosicrucianism, possessing copies of the English translation of the manifestos and Michael Maier’s works,

which he annotated heavily. In his copy of the English translation – now held in Yale University Library – he wrote a lengthy note on the purported history of the Fraternity of the Rose Cross. Referencing Maier in particular, the note ends, ‘This was the history of that imposture.’<sup>35</sup> This quote is often cited as evidence that Newton rejected Rosicrucianism. However, it actually refers only to the Christian Rosenkreutz legend in the *Fama*, which Newton recognized as an allegory or *ludibrium*.

The source of Newton’s obsession with the esoteric is particularly illuminating. He undoubtedly started out as a mechanist, pure and simple, reserving a special admiration for Descartes. However, in the mid-1670s he changed radically, embracing a far more arcane worldview. The reason for this can be traced back to the influence of the Cambridge Platonists, especially that of Henry More, who – nearly thirty years Newton’s



senior – was an old boy of the same school in Grantham. As we saw earlier, this woefully misnamed group were fundamentally Hermeticists, part of an unbroken line of a spiritual brotherhood stretching back to Marsilio Ficino, who rediscovered the works of Hermes Trismegistus. At least one member of the Cambridge Platonists, John Worthington, was also part of Hartlib's Invisible College, itself a direct continuation of the Rosicrucian Antilia, which was intimately connected to Bruno's reforming campaign and the Giordanisti.

One of the first papers to recognize the importance of Newton's Hermeticism was by J. Edward McGuire and Piyo M. Rattansi, both lecturers in the history and philosophy of science at Leeds University. Published in the *Notes and Records of the Royal Society* in December 1966, the paper, 'Newton and the "Pipes of Pan"', was based on a study of Newton's draft of rewritten sections of the

*Principia*, which he wrote in the 1690s for a proposed new edition that was to have included more on the esoteric. McGuire and Rattansi explore the influence of the Cambridge Platonists on Newton's thinking, concluding that:

In re-examining Newton's relation to the Cambridge Platonists, we shall see that he did not merely borrow ideas from them, but was engaged in a private dialogue whose terms were set

by a certain intellectual tradition.<sup>36</sup>

But which 'certain intellectual tradition'? They go on to identify it as the 'most elaborately developed Renaissance *prisca* doctrine' found in the works of Ficino and Pico, which were derived from the *Corpus Hermeticum*.<sup>37</sup> McGuire and Rattansi add that 'Newton, and the Cambridge Platonists, saw their task as the unification and restoration of this philosophy.'<sup>38</sup> In the words of Richard Westfall, as a result of Newton's association with the Cambridge Platonists, 'the Hermetic

influence bade fair to dominate his picture of nature at the expense of the mechanical.’<sup>39</sup>

Newton frequently cited Hermes Trismegistus in his alchemical and esoteric private writings and wrote a detailed commentary on the *Emerald Tablet* (which was considerably longer than the original). An American historian who specialized in Newton’s alchemy, Betty Jo Teeter Dobbs, comments on the extent of Newton’s passion for Hermes explaining that ‘Newton’s study of Hermes Trismegistus extended over a period of at least twenty years, possibly longer.’<sup>40</sup>

Newton’s Hermeticism transformed his thought in precisely the opposite direction to that which we have come to expect in the twenty-first century. The modern perspective is that people started with vague and supernatural explanations for how things work, but eventually came to understand them in purely mechanical and logical terms. But

# Newton moved from mechanics to magic. As Westfall writes:

In Newton, peculiarly Hermetic notions fostered the crucial development of his scientific thought, and in the concept of force became a central element both in the enduring science of mechanics and the accepted ideas of nature. The fundamental question for Newtonian scholarship, as it appears to me, is not the presence of Hermetic elements in his philosophy of nature; their presence has been demonstrated beyond reasonable doubt. The fundamental question is the mutual interaction of the two traditions in the development of Newton's sci-

entific thought. <sup>41</sup>

It is now recognized that it was not an apple falling on Newton's head – or even less dramatically simply plumping to the ground in front of him – which gave him his eureka moment, but delving into the pages of the *Hermetica*. And it bestowed on him nothing less than the key to unlock the mysteries of nature.

It is not simply a matter of Newton hitting on the physical laws of nature by drawing analogies with the Hermetic principles. He *applied* those principles to physical systems. For example, the big resistance to his explanation of gravity was that many considered it

to be too 'occult'. His notion of gravity as a force that acts across space, at a distance, and does so in the way it does purely as a consequence of the nature of the universe, was drawn straight from the magical laws of sympathy and attraction as expounded in the *Hermetica*. (Newton put it more succinctly, declaring 'Gravity is God'.) The law of gravity invokes principles relating to forces that act between the Earth and heavenly bodies that feature – in very different language, of course – in *Asclepius*, the same work that inspired Copernicus.

And Newton's certainty that the heliocentric model was correct also seems primarily to have been drawn from his knowledge of the *Hermetica*, rather than from the works of Copernicus or Kepler. In a discussion of the mysteries of ancient Egypt he wrote:

It was the most ancient opinion that the planets revolved about the sun, that the earth, as one of the planets, described an annual course about the sun, while by a diurnal motion it

turned on its axis, and that the sun remained at rest.

Of course, the obvious source for this understanding of the Egyptians is, once again, *Asclepius* and the other Hermetic texts.

While most scholars recognized Newton's *Principia* as a work of genius, a sizeable number immediately dismissed it as a far-rago of occultism. Richard S. Westfall comments:

The cry of occult qualities greeted the publication of the *Principia*. In more than one sense, the mechanists who raised the cry were justified. Not only did the concept of attraction violate their sense of philosophic propriety, but the origin of the concept was the very Hermetic tradition they suspected ... The champions of mechanical orthodoxy failed to realise what be-

nefit the Hermetic idea could bestow on the mechanical philosophy of nature. <sup>43</sup>

Of course nobody today would dare side with Newton's contemporary detractors. Newton's genius is now universally recognized. And yet there are still those who can't see the significance of the esoteric facet of his life and work. If nothing else, his modern critics show themselves on this major point to be

giants of condescension and pygmies of understanding.

In his *God is Not Great* (2007), Christopher Hitchens unhesitatingly describes Newton as ‘a spiritualist and alchemist of a particularly laughable kind’.<sup>44</sup> Apparently in today’s era of education and enlightenment even your average journalist and literary critic possesses a greater intellect than poor befuddled old Isaac Newton. But the reality is simple: if Newton had never had become privy to the Hermetic philosophy, he would never have achieved his work and the world would be – literally – much the poorer for it. It is universally acknowledged that if the *Principia* had never been written, our modern technological world would not exist. But without the Hermetica, Newton would never have written the *Principia*. Emphatically Newton did not make his great scientific discoveries *despite* his esoteric beliefs, but *because* of them.

The same is true of Copernicus, Kepler, Gilbert, Galileo, Kircher and Leibniz. All of these great scientific minds either drew their inspiration directly from the *Hermetica* or indirectly from the works of other Hermetic masters – usually Bruno. Without that extraordinary philosophy and its accompanying curiosity, they would never have realized that mere men could be giants, gods of thought to whom anything was possible and that freedom from the tyranny and poverty of intellect that marked the reign of the Church of Rome was, indeed, possible.

This raises some other important questions: If the *Hermetica* was this wondrous intellectual instrument, where did it originate? How did its authors come to know such secrets? Who were *they*? And was Newton right? Did the Hermetic texts embody the greatest wisdom of ancient Egypt?



## Chapter Six

- 1 Quoted in Yates, *The Rosicrucian Enlightenment*, p. 186.
- 2 Stanford Encyclopaedia of Philosophy, online: plato.stanford.edu/entries/cambridge-platonists.
- 3 Quoted in Dobbs, *The Foundations of Newton's Alchemy*, p. 115.
- 4 P. M. Rattansi, 'Some Evaluations of Reason in Sixteenth- and Seventeenth-Century Natural Philosophy', in Teich and Young (eds.), p. 151.
- 5 Quoted in Yates, *Giordano Bruno and the Hermetic Tradition*, p. 424.
- 6 Purver, p. 217.
- 7 Quoted in *ibid.*, pp. 221–2.
- 8 Quoted in *ibid.*, p. 219.
- 9 Quoted in *ibid.*, p. 198.
- 10 Quoted in *ibid.*, p. 199.
- 11 Bacon, p. 67.

[12](#) Rossi, pp. 13–14.

[13](#) Tuveson, p. 52.

[14](#) Bacon, p. ix.

[15](#) J. R. Ravetz, ‘Francis Bacon and the Reform of Philosophy’, in Debus (ed.), *Science, Medicine and Society in the Renaissance*, vol. II, p. 101.

[16](#) Bacon, p. 1.

[17](#) *Ibid.*, pp. 2–3.

[18](#) *Ibid.*, p. 3.

[19](#) E.g. Tuveson, pp. 170–9, Yates, *The Rosicrucian Enlightenment*, chapter XV.

[20](#) Lomas, p. 320.

[21](#) From Lomas’ lecture ‘Sir Robert Moray – Soldier, Scientists, Spy, Freemason and Founder of the Royal Society’, given at Gresham College, 4 April 2007. A transcript is available on the Gresham College website: [www.gresham.ac.uk/event.aspPageId=45&EventId=589](http://www.gresham.ac.uk/event.aspPageId=45&EventId=589).

[22](#) Quoted in Purver, p. 221.

[23](#) Quoted in *ibid.*, pp. 221–2.

[24](#) Quoted in *ibid.*, p. 232.

[25](#) Quoted in *ibid.*

[26](#) Quoted in Bluhm, p. 185.

[27](#) *Ibid.*, pp. 183–6.

[28](#) Gribbin, p. 229.

[29](#) Lord Rees, today's President of the Royal Society, quoted in Bragg, p. 22.

[30](#) Gribbin, pp. 238–9.

[31](#) Hollis, p. 262.

[32](#) Richard S. Westfall, 'Newton and the Hermetic Tradition', in Debus (ed.), *Science, Medicine and Society in the Renaissance*, vol. II, pp. 185–6.

[33](#) 'Newton, the Man' in Keynes, p. 363.

[34](#) *Ibid.*, p. 366.

[35](#) Quoted in Yates, *The Rosicrucian Enlightenment*, p. 200.

[36](#) McGuire and Rattansi, p. 109.

[37](#) *Ibid.*, p. 127.

[38](#) *Ibid.*, p. 124.

[39](#) Westfall, 'Newton and the Hermetic Tradition', in Debus (ed.), *Science, Medicine and Society in the Renaissance*, vol. II, p. 193.

[40](#) Dobbs, *The Janus Face of Genius*, p. 68.

[41](#) *Ibid.*, pp. 185–6.

[42](#) Quoted in Westfall, *Never at Rest*, p. 434.

[43](#) Westfall, 'Newton and the Hermetic Tradition', in Debus (ed.), vol. II, pp. 194–5.

[44](#) Hitchens, p. 65.

## CHAPTER SEVEN

# EGYPT'S LEGACY

# TRUE

The mysterious collection of works known as the Hermetica may have illuminated the path for many of the world's greatest scientists and philosophers, who believed it to be the authentic repository of ancient Egyptian wisdom, but in 1614 Isaac Casaubon threatened to completely undermine their position, declaring authoritatively that the books were 'only' about a millennium and a half old, dating from the early centuries CE. Modern historians agree that Casaubon, who employed philological techniques (the analysis of language and literary style), reached roughly the right conclusions, even if for the

wrong reasons, at least as far as the actual composition of the *Hermetica* is concerned. Its sources, however, are quite another story.

As we have seen, Casaubon demonstrated that the Greek of the *Hermetica* does not belong to the classical period but is a later style altogether, which dates from the late centuries BCE and early centuries CE. This time-frame makes sense, as this was when Egypt was ruled successively by the Greeks and the Romans, a period beginning in the 330s BCE during Alexander the Great's most feverish bout of empire building.

After Alexander's death his general Ptolemy declared himself pharaoh, establishing the Ptolemaic dynasty that lasted for three centuries until the death of Cleopatra. During this time Hellenic customs, lifestyle and language took hold, at least among the top strata of Egyptian society. In 30 BCE, after the second most famous snake in history (after the chatty tempter that appears in

Genesis) had done its worst to the Queen of the Nile, the Romans took over, although Egyptian-born Greeks continued to be overrepresented among officialdom. Greek, rather than Latin, remained the *lingua franca* of the eastern half of the Roman Empire.

This means that the Hermetic texts were composed at some point between the beginning of the Greek domination and their first mention in Christian works in the third century CE, a period that lasted around 500 years. This may not pinpoint the precise historical moment of the Hermetica, but it still places them well after the golden age of the Egyptian civilization. So how could they contain the secrets of the pyramid builders?

This question highlights a flaw in Casaubon's argument. Establishing that the Hermetic books dated from the period of Greek and Roman domination was hardly earth-shattering. If they had been composed any



earlier they wouldn't have been written in Greek of any style, but in Egyptian. And of course the fact that they were composed in Greek does not necessarily mean that the ideas they expressed were conceived at that time. They could, for example, have been written to explain an Egyptian belief system to Greek-speakers, or just as easily have comprised a translation of Egyptian wisdom texts. These fairly obvious objections didn't escape the seventeenth-century Cambridge Platonists, who used similar arguments against Casaubon.

That is why the second part of Casaubon's case, which he based on the content of the *Hermetica* rather than the style, was important. By demonstrating that certain sections had been influenced by concepts from Plato and the New Testament – particularly John's Gospel and some of Paul's letters – Casaubon believed he had proved that the texts

were composed from scratch after the time of Christ.

Modern historians have roundly rejected this part of Casaubon's argument, seeing no direct connection at all between the New Testament and the *Hermetica*. Any potential link is indirect, as both texts derive from the same blend of theological and philosophical speculation, drawn from various cultures including the Hellenic, Iranian, Judaic – and, of course, the Egyptian – which were being explored at that time.

As we have seen, what really excited Renaissance Hermeticists was the parallel between the description of God's Word in the *Pimander* and the Word/Logos passage that opens John's Gospel. However, the unknown writer of this gospel took the concept from the work of Philo of Alexandria (c.20 BCE–c.50 CE), a Hellenized Jew who blended Jewish theology with ideas from the great intellectual melting pot that was his

own city. The Hermetica also drew from the same pool of ideas, so any connection between the Word in *Pimander* and the Gospel of John is indirect. It doesn't even necessarily mean that the Hermetica came after Philo, since the ideas he drew on had been in the philosophical mix for some time. And – as we are about to see – part of this included home-grown Egyptian traditions, which almost certainly provided the inspiration for the Hermetic description of God's Word. Although it is only too easy to pity Casaubon, there was simply not enough information available in his day to make a proper analysis.

So now we're back where we started. As was believed before Casaubon put the feline among the feathered creatures, the Hermetic books may have contained traditions, not to say secrets, from the old Egypt, the Egypt untainted by the trendy Hellenic glamour of its occupiers. So is it possible to deduce

when the Hermetica were written, and by whom? And, more importantly, what were their sources? Was Hermeticism invented in Greek or Roman Egypt, or did it draw on older traditions?

# THE ORIGINAL TIME LORD

During the eras of Greek and Roman rule, Egypt – and particularly Alexandria with its famous library – was the crucible where the intellect's gems of the known world came together. As well as native Egyptians, those of Greek descent and peoples from all over the Empire, the great seaport also boasted large Jewish and Samaritan communities. Trade routes brought Iranians, Arabs and even Indians to the city, carrying their traditions with them.

Even so, and despite the flaws in Casaubon's work, for a long time historians still assumed that the philosophy and cosmology found in the *Hermetica* were derived from Greece. It just had to be Greece – after all,

weren't the best things always Hellenic? Positively pickled in the classics, the academic world refused to dip a toe into any other culture. But over the years this became increasingly untenable, and with scholarly huffing and puffing, beard stroking and dragging of feet, it was gradually acknowledged that native Egyptian thought must have had at least a supporting role in shaping the Hermetic books.

Doubts about the purely Greek origin of the Hermetica began to surface in the early twentieth century, when university men and women realized key elements of its philosophy and cosmology could not be attributed to any identifiable Greek source. But there was controversy about where they did come from, the main candidates being native Egyptian, Judaic and Iranian traditions.

Perhaps understandably, at first it was mostly Egyptologists who held out for a home-grown influence. Then in 1904

Richard Reitzenstein, the eminent German scholar of Gnosticism and the Hellenic religions, made the groundbreaking suggestion that the *Hermetica* was the product of a religious community in Egypt. (He did, however, later change his mind, looking towards Iran instead.) From the mid-twentieth century many scholars – particularly in France – joined the pro-Egypt camp. It gradually became a question of not *if* there was an Egyptian influence, but of its true extent. A consensus also emerged that at least the core parts of the *Hermetica* dated from the early years of Greek domination, rather than towards the end of the era, as Casaubon came to believe.

Key scholars in this process were the French historian Jean-Pierre Mahé and, more recently, Garth Fowden, the British professor of antiquity who is currently Research Director at the Institute for Greek and Roman Antiquity in Athens. As the title of

his 1986 book *The Egyptian Hermes* suggests, he amassed the considerable evidence of a strong home-grown, Egyptian, influence on the Hermetica.

Although presented as a characteristic Greek dialogue, the Hermetic texts don't quite fit that genre. Instead of presenting a discussion between philosophers, as in the Greek tradition, the texts present a question-and-answer session between master and pupil – which is more in keeping with the traditional Egyptian wisdom literature.<sup>1</sup> The Hermetic texts are therefore a kind of stylistic hybrid of the Egyptian and Hellenic forms. Maybe the writers were consciously striving to make their work more Greek-friendly.

The books are obviously the product of different writers – which accounts for their inconsistencies – although they belonged to the same school or cult. All the authors here are anonymous, simply attributing their works to Hermes, a typically Egyptian



practice.<sup>2</sup> This was quite different from the Greeks or Romans, who routinely hyped up their celebrity philosophers without making any claims of divine authorship. This is another important indication that, while written in Greek, the mindset behind the *Hermetica* was authentically Egyptian.

Another clue comes from the astrology and astronomy described in the *Hermetica*. The Egyptians divided the night sky into thirty-six parts or decans, each linked to a prominent constellation or star. During the Greek period, the more familiar twelve-sign zodiac took over, but the astronomy described in the *Hermetica* sticks to the thirty-six-decan system, so at least the origin of the *Hermetica* in this one major respect was truly Egyptian.<sup>3</sup>

A more important clue comes from the attribution to Hermes, the Greek deity who was always associated with the Egyptian

wisdom-god, Djehuty, or Thoth in its Greek rendering. He ruled over learning and was the inventor of writing and the calendar and 'keeper of the divine words',<sup>4</sup> hence his titles, 'Lord of Time' and 'Reckoner of Years'.<sup>5</sup> A minor function was his association with healing: he was, for example, credited with inventing the enema.

Hermes and Thoth are by no means direct parallels, though. In the Greek pantheon, Hermes was the patron deity of many aspects of life, but not of knowledge and learning. He was the god of cunning and cleverness, but that isn't the same thing. It is thought that the association developed as a result of Hermes' more significant role as guide of dead souls, which oddly echoes Thoth's rather secondary job as helper and guide of the deceased, specifically the dead Osiris.<sup>6</sup> The telling fact is that the characteristics of Hermes Trismegistus as portrayed in the *Hermetica* are more in line with those of

Thoth, not the Greek Hermes, strongly suggesting that the cult or school behind the Hermetica was Egyptian.

Then there is the famous epithet for Trismegistus, 'Thrice Great', which only makes sense as a Greek translation of a typical Egyptian honorific. For emphasis Egyptians repeated the glyph for 'great', literally saying 'great great'. But in cases of truly mind-blowing greatness, they would use it three times, as in the all-important 'great great great Thoth'. The most natural Greek translation would be 'three times great'. More significantly, this practice seems to have been specifically reserved for Thoth, which seems to be the origin of 'Thrice Great Hermes'.

In 1965 an inscription was found dating from around 160 BCE, written by a priestly scribe named Hor (Horus). Inscribed in the late form of Egyptian script known as Demotic, it appeals to 'Isis, the great goddess, and Thoth, the three times great'<sup>7</sup> – the

last phrase simply repeating the Demotic character for ‘great’ three times. This is the earliest known use of this form of address. In his account of this inscription, Egyptologist J. D. Ray makes the following highly pertinent observation:

It is not the point of least interest in our document that they should provide the earliest clue to the origins of a most remarkable figure in the history of thought, a philosopher, whose reputation as the sage ‘Trismegistus’ was transmitted through the Middle Ages and Renais-

sance to influence even such forerunners of modern thinking as Bruno and Copernicus.<sup>8</sup>

In fact, the mindset behind the *Hermetica* as a whole is Egyptian. The authors ‘think in terms of a whole milieu populated with ancient Egyptian gods and sages’.<sup>9</sup>

The other characters in the dialogues are a mix of Egyptian – including Isis and Thoth himself, who appears under the name of Tat – and Greek. But even the Greek characters have specifically Egyptian associations. Agathodaimon (or Agathos Daimon), a minor god in the Greek pantheon, became patron deity of Alexandria, where he was

associated with Osiris and his Hellenised semi-alter ego, Serapis. More central to the *Hermetica* is the character of Asclepius, a supposed descendant of the Greek healer-god, who appears in several books. But even here there is an important clue to the origins of the *Hermetica*. The Greeks identified Asclepius with the Egyptian god of healing and medicine, Imhotep, who was a rare example of Egyptian deification of a living person.<sup>10</sup> In *Asclepius*, Hermes declares that the eponymous main character's illustrious ancestor was a man who became a god. Imhotep was vizier to the pharaoh Zoser and architect of the first of the great pyramids, the Step Pyramid of Saqqara, built around 2620 BCE. The cult of Imhotep clearly survived into classical times: our priestly scribe Hor records in 160 BCE that he was instructed by the 'priest of the chapel of Imhotep' in the sacred city of Heliopolis.<sup>11</sup>

For all these reasons, there is no doubt that the minds behind the Hermetic books were Egyptian, even if they chose to express themselves in the *lingua franca* of the day. But who were they?

In the second half of the twentieth century a number of historians began to argue that the Hermetica are the 'bible' of an Egyptian mystery cult.

In recent decades a new theory of the Egyptian origins of the Hermetica has emerged. Rather than simply being the sacred books of a mystery cult, they were part of a concerted, and perhaps desperate, effort to *preserve* its teachings in the face of the great threat to their culture posed by the Greek hegemony. This anxiety found its ultimate expression in the *Asclepius*' Lament. By expressing their beliefs in the language and style of their cultural oppressors, there was a chance that the Egyptians' precious ideas would survive. This was all the more urgent

because of the myriad streams of thought flooding together in Alexandria, threatening to submerge Egypt's own religious and philosophical traditions. Fowden points to the cities of Panopolis and (for obvious reasons) Hermopolis as centres of the Hermetic cult.<sup>12</sup>

By the time the Romans took control of Egypt, the Greeks had been in charge for generations, so their culture was already entrenched at the top echelons of society. But the conquerors and conquered largely kept their distance. The Greeks regarded their culture as more advanced, while the native Egyptians saw their civilization as older and wiser. The religious and cultural resistance to the Greeks was embedded in the city of Memphis, the ancient capital, whose western plateau was the 'Libyan mountain' mentioned in the Lament, site of a great necropolis that includes Saqqara where it was believed Imhotep himself was buried.<sup>13</sup>

The native Egyptian cults survived until Christianity became the dominant religious force in the Empire. Although the Emperor Constantine famously gave it imperial recognition, it was only in 380 CE, more than fifty years later, that Theodosius I declared it to be the one legitimate religion of the Empire. Eight years later he ordered the pagan temples of Egypt and elsewhere in the Middle East to be closed down forever, a task enthusiastically overseen by Theophilus, the Christian patriarch of Alexandria.

The Hellenic period also produced some hybrid cults that adapted traditional Egyptian worship to be more Greek-friendly. A major example of this is the cult of Serapis, a new version of Osiris worship – ‘Serapis’ being a conflation of the Egyptian Asar (Osiris in Greek) and Apis, the bull-god assimilated to him. The origins of the cult are controversial: was it, as long believed, a complete invention of Greek times or, as evidence now



suggests, a pre-existing religion that was merely adapted for the purpose? Wherever it came from, the early Ptolemaic rulers adopted it as the official cult that could be practised jointly by their Greek and Egyptian subjects. The main temple, the Serapeum, was located in the new coastal city of Alexandria, which was founded by the Greeks in honour of Alexander the Great. However, Theophilus' over-zealous thugs destroyed it during the anti-pagan pogrom of 392 CE.

For several reasons, the cult of Serapis is a good candidate for the school that produced the *Hermetica*. The writers would have been associated with a temple, since in Egypt not only did learning and religion go together, but so did temples and libraries. The 'daughter' library of Alexandria's celebrated library was housed in the Serapeum – revealing the extent to which the cult valued the preservation of knowledge. The appearance in the *Hermetica* of Agathodaimon, the patron god

of Alexandria associated with Serapis, also suggests that there was a connection with the same cult.

There certainly were some Egyptian priests who made an effort to explain their religion to the Greeks, probably in an attempt to preserve it. The major example of this is the Heliopolitan priest Manetho who, in the early third century BCE – under the first Ptolemaic rulers – wrote a history of his people, the *Aegyptica*, which is still a particularly useful sourcebook on the reigns of the various dynasties (a term he invented). Manetho is a Greek rendering of his name, but the syllable ‘tho’ probably derives from Thoth (perhaps ‘Beloved of Thoth’), perfect for a scribe and historian of the great wisdom-centre of Heliopolis. Manetho was apparently also a key figure in establishing and promoting the Serapis religion, such was his desperation to make his people’s traditions understandable to Greeks.<sup>14</sup> If they

knew them they might like them, and if they liked them enough, they just might want to conserve them.

Manetho's agenda was the same as the one Garth Fowden ascribes to the authors of the *Hermetica*, which at the very least shows that some Egyptian priests were proactively trying to preserve their traditions.

In an ironic twist, a text ascribed to Manetho may – if genuine – contain the earliest reference to Hermes Trismegistus. This is found in a dedication to the ruler Ptolemy Philadelphus at the beginning of the astronomical *Book of Sothis*, which is attributed to Manetho. Although this would make a particularly satisfying connection between the great Egyptian chronicler and the *Hermetica*, unfortunately most historians regard the book as a later work and the dedication a forgery because, following Casaubon, the term 'Trismegistus' is thought to have been

invented in the early centuries CE, and therefore Manetho could never have used it.

If an attempt to preserve the Egyptian traditions was what underpinned the Hermetica, then clearly its religious and cosmological ideas would hardly have been new. They must have been the key philosophy in a belief system that predated the Greek conquest, perhaps by many centuries. Evidence for this is found in the works of the Neoplatonic philosopher Iamblichus of Syria (c.245–c.325 CE) who studied in Athens before founding his own academy in Antioch. His major work *On the Egyptian Mysteries* (*De mysteriis Aegyptiorum*) opens with the words:

Hermes, the god who presides over rational discourse, has long been considered, quite rightly, to be the common patron of all priests: he who presides over true knowledge about the gods is one and the same always and everywhere. It is to him that our ancestors in partic-

ular dedicated the fruits of their wisdom, attributing all their own writings to Hermes. <sup>15</sup>

So Iamblichus understood that not only did the priests attribute their books on the

nature of the gods and universe to Hermes, but also that this was already a venerable tradition that dated from the era of the ‘ancestors’. As Iamblichus lived very close in time to the writing of the *Hermetica* – which he frequently references – he is unlikely to have been fooled by an unashamed recent fabrication.

The Iamblichus connection is, to us, particularly satisfying. Modern academia labels him a Neoplatonist, but the opening of his masterwork, with words of praise for Hermes, suggests that his philosophy was in some way related to Hermeticism. He also ‘made use of Hermetism in formulating his own widely influential doctrine’.<sup>16</sup> But the relationship has even more to reveal about the antiquity of the Hermetic cosmology.

# **‘THE DIVINE IN THE ALL’**

Neoplatonism was another product of Greek- and Roman-dominated Egypt. As with Hermeticism, the pro-classics bias meant that the fact that Neoplatonism developed in Egypt was considered irrelevant. Instead, scholars assumed that it was actually built on Greek ideas. However, recent studies have shown that Neoplatonism, too, owed far more to Egyptian traditions than previously acknowledged.

The ‘neo’ or ‘new’ part of the entirely modern term Neoplatonism refers to the re-establishment of Plato’s Academy in Athens by fourth-generation followers of the Egyptian philosopher Plotinus. The original academy provided a meeting-place for philosophers in a grove sacred to the goddess of wisdom Athena, a mile outside Athens, for 300 years

until it was destroyed by the Romans when they besieged the city in 86 BCE. With their usual disregard for local sensibilities, they cut down the sacred trees to make siege engines.

Five hundred years later, in the early years of the fifth century CE, a group of philosophers led by Iamblichus' pupil Plutarch of Athens, who considered themselves Plato's intellectual heirs, founded a new Academy in Athens. This became a renowned centre for learning in its own right, but being a pagan school it was closed down on the orders of the Emperor Justinian in 529.

The revived Platonic academy was particularly interested in exploring and developing some of the metaphysical aspects of Plato's teaching. Following his own mentor Socrates, Plato distinguished between the material and spiritual worlds, arguing that the material world, which is knowable through our five senses, is basically an illusion.

Everything belonging to the material sphere is a kind of shadow of a perfect, ideal form – an archetype – that exists in the spiritual realm. Plato thought that it is possible for human beings, through intellectual effort, to transcend their perception of this world and gain experience of the spiritual realm, thus becoming enlightened.

In *Timaeus*, written around 360 BCE, Plato also introduced the concept of the Demiurge, the creator-god of the material universe. Just as everything in the physical world is a reflection of its eternal ideal, so the Demiurge is a lesser version of the one great God who created everything – including the Demiurge himself, whose power is necessarily constrained by the limitations of matter.

It was these aspects that the revived Academy was most interested in, focusing especially on the process of enlightenment through direct experience of the normally



hidden spiritual realm. Rather than purely intellectual exercises of the kind advocated by Plato, the new wave of philosophers attempted to develop rituals and other practices ('theurgy') to enable the human soul to find its way back to its divine source during life rather than after death, aiming at 'the purification of the soul from the barnacles of matter'.<sup>17</sup> This objective was encapsulated in the last words of Plotinus (c.205–270), who is regarded as the founder of Neoplatonism: 'Strive to give back the Divine in yourselves to the Divine in the All'.<sup>18</sup>

Plotinus is an odd character. All he allowed to be known about himself comes from his pupil and biographer, Porphyry of Tyre, who also organized his fifty-four treatises into six collections of nine, hence the 'Enneads', or 'group of nine'. Plotinus was born and lived in Egypt until he moved to Rome at the age of about forty; he never revealed even to those closest to him anything

about his origins or parentage. He no doubt picked up the habit of secrecy from his own master Ammonius Saccas – Ammonius the Porter – who tutored him for eleven years in Alexandria. It was from Ammonius that Plotinus learned his ‘Neoplatonism’.

Unsurprisingly, Ammonius is another oddity. Described by one modern historian as ‘the most shadowy figure in the chronicles of Hellenic philosophy’,<sup>19</sup> virtually nothing is known about his life except his name, which was derived from the god Ammon, strongly suggesting he was a native Egyptian. Ammonius was known as *theodidaktus*, ‘God-taught’, which might be another way of saying he was divinely inspired. In any case, it suggests that his knowledge owed no debt to any formal school of philosophy recognized by the Greeks.

Ammonius Saccas set down nothing in writing, as was the custom for Egyptian priests, and placed his students under a vow

of secrecy not to publish his lectures. But he had two disciples who left their mark on history, Plotinus and the Christian philosopher and theologian, Origen. It was through the latter – who apparently broke his vow – that Neoplatonic ideas were imported into Christian theology.

Mystery man he may have been, but it is still clear that Plotinus' philosophy owed more to an indigenous Egyptian source than it did to Plato. But this background cut little ice with historians of philosophy, again because of the scholarly bias in favour of the classical world. The logic behind the label 'Neoplatonist' is that Ammonius Saccas taught Plotinus, who taught Porphyry, who taught Iamblichus, who taught Plutarch of Athens, who re-established the Platonic Academy – so they all must have been Platonists, mustn't they? And in any case they were all Greek(ish), or at least very

Hellenized and admirers of Plato, who was definitely Greek.

However, in the early decades of the twentieth century the avoidance of Egyptian tradition was becoming embarrassing. Even the most conservative academics had to acknowledge that large parts of Plotinus' work had no parallels with earlier Greek thought and seemed to derive from some other tradition entirely. While it is true that his writing does contain many references to Greek philosophical concepts, particularly Plato's, when these are removed, his basic principles and reasoning hold their own internal logic.<sup>20</sup> In other words, he may have used the Greek concepts to bolster his philosophy, but didn't *derive* it from them.

French historian of philosophy Émile Bréhier was one of the first to suggest in the early 1920s that Plotinus wasn't purely inspired by Greece – causing a huge furore among the ranks of venerable beards. At the

end of his life in the 1950s, in an introduction to an English translation of his original papers, Bréhier cheekily dropped in a quote from *Asclepius*, hinting that he recognized a relationship between Plotinus and the Hermetica:

After Alexander the Greeks, without doubt, did 'Hellenize' the Orient; but, inversely, Egypt, 'the land where gods are invented', stamped its powerful imprint not only upon the customs but upon the ideas of the Greeks, in spite of the efforts of the rulers of Egypt to keep the

Egyptians in a subordinate state. <sup>21</sup>

But even once the non-Greek origin of much of Plotinus' work was recognized, historians still tried to ascribe his source to Iran or India – anywhere but Egypt. One might have thought that Plotinus being an Egyptian taught by an Egyptian in Egypt might have been a clue to the source of the non-Platonic parts of his philosophy.

More recently a dose of objectivity, not to say common sense, has been injected into this unnecessarily complicated subject. Karl W. Luckert, the German-born American

professor of the history of religion at South-west Minnesota State University, has strongly and persuasively argued that Plotinus' philosophy should not be called Neoplatonic at all, but 'neo-Egyptian'.<sup>[22](#)</sup>

Luckert shows that Plotinus derived his ideas from traditional Egyptian spirituality. For example, he taught that the human soul comprises of both the high soul and the low soul. Not only is there nothing that corresponds to this idea in the Greek religion, but Plotinus' description matches exactly the well-known Egyptian concept of the *ka* and *ba*. The *ka* is a kind of astral double, the life force that is born with the individual and which returns to the gods at death; the *ba* is the spiritual part of the personality, the *ka*'s manifestation in the physical world. The latter is more like the traditional Western concept of the spirit body, but in the Egyptian system both make up the human soul.

Luckert goes on to show that many of Plotinus' concepts – the nature of the god-head, the human soul and its relationship to the divine – are directly lifted from Ancient Egypt. While Plotinus did use Platonic ideas, he only did so to present Egyptian traditions in a way that was familiar to his scholarly readers:

Plotinus has given us Egyptian religion, theology in the linguistic garb of Hellenic philosophy. His philosophical and Greek linguistic cover and his scarce links with Platonic philosophy sufficed to hold the attention of a few Greek students of philosophy. <sup>23</sup>

Further evidence of the Egyptian origins of Neoplatonism can be seen in the career of the philosopher Antoninus, who died shortly before the suppression of the pagan cults in the 390s. Again, very little is known about him – the only source is a summary of his life written by Eunapius, an Athenian physician and philosopher, in a work dating from about a century later.

Like Ammonius Saccas and Plotinus, Antoninus was secretive and evasive about the religious element of his beliefs. Eunapius tells us that after teaching at the Serapeum in Alexandria, Antoninus went to the coastal town of Canopus in order to devote himself to its 'secret rites'. Eunapius also says that because of the growing imperial hostility to the religion, while Antoninus was in Alexandria he would only ever answer people's questions using Plato's philosophy, and would flatly refuse to discuss the divine or theurgy. This is enough to label him a Neoplatonist as far as history is concerned. But clearly Antoninus was something else, something Egyptian and secret – something that *was not incompatible* with Plato, but equally not necessarily actually Plato. As Eunapius writes of Antoninus:

Though he himself still appeared to be human and he associated with human beings, he foretold to all his followers that after his death the temple would cease to be, and even the great and holy temples of Serapis would pass into formless darkness and be transformed, and that

a fabulous and unseemly gloom would hold sway over the fairest things on earth. <sup>24</sup>



Eunapius tells us that Antoninus' followers understood this as an oracle, which came to pass very shortly after he died in the persecutions ordered by Emperor Theodosius. His prediction echoes the Lament, although Antoninus could not have been its author as it was being quoted by Christian writers from the start of the century. However, he could well have used the Lament as the basis for his own prediction. At the very least this shows that the 'Neoplatonist' Antoninus shared the attitudes and anxieties of the Hermetic writer of *Asclepius*.

As with Hermeticism, the Neoplatonic trail leads to native Egyptian traditions connected with the Serapis cult. In fact, Neoplatonism and Hermeticism were natural bedfellows – they are simply two sides of the same coin.

However, the Serapis cult itself was a relatively new innovation, created or adapted

for the all-conquering Hellenic world, just as Alexandria was a new city built by the Greeks. Therefore any traditions transmitted via the cult to the Hermetica must have originated with some other cult from some other place. But what was it and where did it come from?

Karl Luckert traces the origins of the wisdom tradition of which Plotinus was heir not just a few centuries back into Egypt's past, but all the way to its beginning. And, if Luckert is right, given that Neoplatonism is the twin of Hermeticism, then wherever one is found, inevitably the other will be also.

# THE SACRED CITY

After comparing Neoplatonic spirituality with the traditional Egyptian religious schools, Luckert identified its origin as the theology of the major cult centre of Heliopolis. This discovery leads us to another: that strangely evocative but mysterious city also holds the key to the wisdom of the Hermetica. Even the name 'Heliopolis' is tantalizing, being Greek for 'City of the Sun', which is probably why the term appealed so much to Renaissance Hermeticists such as Tommaso Campanella.

The golden city was the centre of the cult of the sun god Ra, or Re (associated with the Greek Helios). Even during the era of Greek domination the city still hosted a great annual ceremony in his honour. Sadly, this sacred place is now submerged beneath a largely

industrial suburb in the north of Cairo (although confusingly not the district called Heliopolis, which is in quite another part of the city), where a three-and-a-half-thousand-year-old temple was discovered beneath the market in 2006. The ancient Egyptians called it Iunu, which means ‘pillars’, a reference to the many obelisks that poked their phallic fingers at the sky, only one of which now remains – and it appears under the name of On in the Old Testament. The matching pair of red granite obelisks in New York and London (the anachronistically named Cleopatra’s Needle) also came from Heliopolis. It was the most renowned centre for the preservation of Egypt’s wisdom tradition, and the most ancient. Its reputation is attested by the fifth-century BCE Greek historian Herodotus, who visited the city ‘where the most learned of the Egyptians are said to be found’, and met its priests.<sup>25</sup>

In fact, Egypt has always exerted a powerful hold over the imagination, certainly because of its literal magic and mystery but also possibly simply because its civilization endured virtually unchanged for a staggering length of time, from the unification of the two kingdoms of the Nile, Upper and Lower Egypt, around 3100 BCE. Within a mere five hundred years it had reached the level of architectural and engineering genius embodied in the great pyramids of Giza, Saqqara and Dahshur. To put these staggering achievements in perspective, this was about four thousand years before the building of the great Gothic cathedrals in European cities such as York in the north of England and the French capital.

Yet the essential aspects of ancient Egyptian civilization – its political and social structure, culture, symbolism and religion – remained more or less the same for over two millennia. Although throughout its

remarkably long history there were periods of foreign occupation, the culture always rose again with its traditions basically intact. Indeed, when the Greeks took over in the fourth century BCE Egyptian culture was still recognisably the same as it always was. Even then, it continued beneath a veneer of Hellenization for another seven centuries until finally being wiped out by the Christians.

The earliest known religion of Egypt, the one that inspired the pyramid builders and other Egyptian geniuses, was that of Heliopolis. Over the course of the civilization's lengthy history other cults and religions came to the fore at different times. A particularly strong challenge was posed in the fifteenth century BCE by the religion centred in Memphis that featured Ptah as its creator-god. But the Heliopolitan theology influenced all those that came after it – it was 'the dominant strain of thought by which subsequent Egyptian religious notions and rites

were oriented'.<sup>26</sup> The Ptah cult, for example, assimilated the Heliopolitan tradition rather than attempting to displace it. Although, like every other system of belief, it went through changes and evolved, the essential ideas remained unchanged over time. Only one attempt was made to eradicate the Heliopolitan religion completely. In the fourteenth century BC the 'heretic pharaoh' Akhenaten tried to replace it with the cult of his single solar god Aten, who was in many respects his divine alter ego.

The great genius Imhotep was a priest of Heliopolis – his cult was still practised in Heliopolis in the second century BCE, an astonishing two and a half millennia after his earthly life – and so was Manetho some 2,300 years later. Both show that Heliopolis was both a religious centre and a place of learning and science. Both priests also have a connection, albeit indirect, with the Hermetica. The prominence in the texts of Asclepius,

a thinly-disguised Imhotep, suggests an association with Heliopolis. And given that Manetho was a priest of Heliopolis and instrumental in founding the Serapis cult – with which the Hermetic works seem connected – one can clearly see a bridge between the two cults.

Although the Heliopolitan religion was complex and sophisticated, nowhere in its lengthy history did its priests record its basic theology and practices. This was not the Egyptian way. Apart from an apparently engrained instinct to maintain secrecy – possibly because only the worthy could be initiated into the mysteries – priests preferred to express their religion through ritual and symbols and myths, the best-known of which is the story of Isis and Osiris.

The supreme expression of the Heliopolitan religion is the Pyramid Texts, which were inscribed in hieroglyphs on the walls of the burial chambers of the pyramids of seven



pharaohs and their queens, between about 2500 and 2200 BCE. The inscriptions consist of a series of several hundred incantations relating to the afterlife journey of the deceased. However, even though the inscriptions are the most ancient religious writings known from anywhere in the world, unquestionably they derive from even older texts, dating from the very beginning of the Egyptian civilization.<sup>[27](#)</sup>

Yet although even the Pyramid Texts fail to set out the beliefs of Heliopolis systematically, why should we expect them to? After all, the people who mattered – the priests and worshippers – were already familiar with their own religion. The Texts do, however, allow the core theology and cosmology behind them to be reconstructed. The most successful attempt is found in Karl Luckert's *Egyptian Light and Hebrew Fire* (1991), which isolates two related aspects: the overall understanding of the origins and

nature of the cosmos, and its relationship to human beings.<sup>[28](#)</sup>

The religion centres on nine major gods, who became known later in Greek as the Great Ennead (a group of nine). The Nine – along with a multitude of lesser gods – were all considered manifestations of the one great creator-god, Atum. The other god-forms are convenient symbols for his different aspects.

As might be expected of such a red-blooded ancient people, the Heliopolitan creation myth – or metaphor – is highly charged sexually. Luckert describes it as ‘pornographic theography’.<sup>[29](#)</sup> In the original version of the creation myth Atum masturbated, his explosive climax shooting the universe out into space. Later this description was modified in order to appease the easily offended, having him spit or merely cry out – an image of a god creating the world through

his divine word that was borrowed by both Old and New Testaments.

At first the image seems crudely school-boyish, but it actually possesses rather more sophistication than meets the eye. For a start, it is a pretty good analogy for the ultimate act of creation and certainly conjures up an irresistibly lasting image, unlike those contained in dry-as-dust astronomical textbooks. And like many belief systems, the Heliopolitan religion saw everything in creation in terms of a yin yang-like complementary polarity – positive-negative, light-dark and so on – which is most often experienced at a human level as the relationship between male and female. In the original myth of Atum, his phallus is the male principle and his hand the female, and the first things they make are the goddess Shu and god Tefnut, their embodiments.

Some authorities, such as German specialist on myth and symbols Manfred Lurker,

prefer the description that Atum ‘copulates with himself using his hand’.<sup>30</sup> Although to the uninitiated this may sound like much of a muchness, the essential difference is that masturbation doesn’t normally generate life. The point is that Atum contains within himself both male and female. And the metaphor of ejaculation encapsulates the notion of the universe as having a single point of origin in space and time, from which everything explodes outwards – a very modern image for the beginning of the universe. One Egyptologist even uses the word ‘singularity’ to describe this concept.<sup>31</sup>

So the universe expands outwards from Atum’s own big bang, becoming not just larger but ever more complex and multidimensional, each level being represented by new pairings of deities. The first new gods or forces that come into being are the female Shu (corresponding to life) and the male Tefnut (order), who are locked in ‘perpetual

sexual union'.<sup>32</sup> This produces their more visible manifestations – in sharper focus, as Luckert puts it – the Earth-god Geb and Sky-goddess Nut. They in turn give birth to two pairs of twin gods, Osiris and Isis, and Set and Nephthys. All together they make up the Great Ennead, arranged in four levels of being, beginning with Atum. As Luckert notes:

The entire theological system can be visualized as a flow of creative vitality, emanating outward from the godhead, thinning out as it flows farther from its source. Along its outer periphery this plethora of divine emanation becomes fragmented into what begins to appear as

the light and shadow realm of our material world. It becomes visible.<sup>33</sup>

This is by no means the end of the process, however, as the system is repeated on a lesser octave, beginning with the child of Isis and Osiris, the falcon-god Horus, who occupies a transitional space between the Great Ennead and the Lesser Ennead, the nine gods of this world (which includes Thoth). Horus' relationship with the material universe is the same as Atum's with all creation, making him therefore the god of the material

world (besides being ‘a son of God and savior of humankind’),<sup>34</sup> the equivalent of Plato’s Demiurge and the Hermetic second god, while (like his father Osiris, who died and rose again), also bearing some comparison with Jesus.

As we have seen, according to Heliopolitan beliefs, the material universe that we perceive through our senses is only part – the outer edge, as it were – of an unimaginably vaster creation, much of which is hidden from us. Again, there is an obvious parallel with Plato’s later concept of spiritual and physical worlds, which is why the last heirs of Heliopolis, the Neoplatonists, found his philosophy so suitable for their purposes.

In his book, Luckert makes a detailed comparison of the Heliopolitan theology in the Pyramid Texts and the principles of the Neoplatonists, particularly Plotinus, and finds the two are identical. Given the overwhelming evidence that Plotinus derived his

teachings from secretly-taught Egyptian traditions, there seems to be no other explanation than that the Heliopolitan system was transmitted down the ages until it reached Ammonius Saccas and other Egyptian sages.

Given the close relationship between the inaccurately named Neoplatonism and Hermeticism, the Heliopolitan system must therefore also underpin the latter. And an examination of the basic ideas of Hermeticism bear this theory out. The language may be different, but the fundamental principles are the same.

# VISIBLE AND INVISIBLE GODS

In the world accessible to human perception, the sun god Ra was deemed to take a role analogous to Atum's in relation to the universe as a whole, and was even known by the composite name Atum-Ra. (For the same reason Ra was also equated with Horus, as Ra-Horakhty.) It is not known when or how this conflation of the two great gods took place, only that they were being associated in the very earliest days of the Egyptian civilization. Atum was a hidden, invisible god; Ra, the golden royal sun, is his visible manifestation. This reveals a connection with the words of *Asclepius* quoted by Copernicus, namely that the sun is a 'visible god', which of course implies the existence of one that is invisible. This has an important implication:



if Atum is the centre of creation, then the sun is the centre of the cosmos that humans can perceive.

There is something else that Atum conceals but implies by his very presence. The gods of both Enneads, besides representing deities concerned with specific aspects of nature and human endeavour, are all really aspects of Atum. Moreover, not only is everything created by Atum, but it is also created *from* him, which makes his creative energies and forces present in everything. Effectively Atum is the universe, as well as possessing a part, or energy, which lies outside and transcends it.

Human beings, too, contain Atum's 'divine spark' within themselves, making them just as god-like as the likes of Horus and Thoth. The only difference is that humans are locked into the world of matter in a way that the gods are not. This echoes the origin of another vital Hermetic principle: that human

beings are potentially gods, and some even manage to actualize that potential. This was also, as we have seen, a central tenet of Hermeticism's philosophical partner, Neoplatonism, which focused on the journey of the soul to the divine in preparation for enlightenment, drawing on another crucial aspect of the Heliopolitan theology.

But there is something else that the myth of Atum has to tell us, something extraordinary. The creative flow from the god to the material universe is not just a one-way phenomenon. Just as it 'exhales' from Atum, it 'inhales' the life force of individuals, which then travels back to its source. Horus, therefore, also represents what Karl Luckert calls the 'turnaround realm', the point at which the life force can begin the journey back towards Atum. We might need Atum – but he also needs us.

The Pyramid Texts are concerned with those rituals that ensure the return of the

King to Atum after his death, projecting his soul into the stars. It is commonly assumed that this stellar existence and the ability to commune with the creator is a prerogative of the King alone, becoming his only after death. However, neither is necessarily the case. The Pyramid Texts are specifically concerned with the King because they happen to be in royal tombs, but nowhere do they say that this afterlife is reserved for him alone. Indeed, the logic of the Heliopolitan theology, in which every individual is a manifestation of Atum, suggests that it happens to everyone.

The 'return journey' described in the Pyramid Texts refers to the afterlife simply because, again, they are in a tomb. But as with most other cultures, it was also believed that certain special individuals – priests or shamans, for example – could undertake this journey in life (usually in an altered state of consciousness), gaining insight or

illumination.<sup>35</sup> This journey was also the aim of the Neoplatonists.

Remarkably, the cosmology of the Hermetica is, ultimately, also that of the first flourishing of the Ancient Egyptian culture. The belief of Renaissance Hermeticists such as Bruno and Newton that the Hermetic works represented the wisdom of that great civilization is absolutely vindicated. And Isaac Casaubon – whose work is still trotted out to trash the value of the Hermetica – was just plain *wrong*.

Other researchers have recognized the connection between the religion of Heliopolis and the Hermetica, as can be seen from the subtitle of Timothy Freke and Peter Gandy's 1997 translation of extracts from the Hermetica: *The Lost Wisdom of the Pharaohs* and their translation throughout of 'God' as 'Atum'.

Of course, the obvious big question is where did the priests of Heliopolis get their ideas from? Did they dream them up, getting lucky with material that just happened to be scientifically accurate? Or was their belief system based on a genuine understanding of the way the universe is organized?

Sadly, a definitive answer about the origins of the Atum religion remains impossible because of a lack of relevant historical information. Some would no doubt prefer to explain the mystery as a legacy from an earlier, advanced, but lost, civilization, which would only push the question back further, not answer it. And inevitably some would conjure up the lazy if sensational notion that we can lay all these wonders at the door of ancient astronauts (a desperately non-Hermetic idea that implies human beings are just too stupid ever to have built wonders like the pyramids). But we suggest the greatest clue lies in the religion itself.

A major component of the magical world-view hardwired into humanity is that specially trained individuals can enter into a state of communion with the gods in which they are given intensely *practical* information. This idea is also the basis of the Heliopolitan 'return journey', Neoplatonic theurgy, the Hermetic gnosis and the occult art of memory. Such communion is not to be understood as bestowing enlightenment in the Eastern sense of the ultimate goal being the achievement of a purely spiritual state – or at least not exclusively – but as providing an understanding of how the universe works in very practical ways. This practice can then be used to extend human knowledge and induce enlightenment in the western sense, as in the Age of Enlightenment.

To judge the results of this communion we have to look no further than the great names who found enlightenment in the Hermetica, itself the ultimate expression of the ancient

Heliopolitan system. Encouraging the belief that all things are possible means that the most ambitious dreams can actually be lived – and often for the greater good.

## Chapter Seven

[1](#) Fowden, pp. 68–74.

[2](#) See below, p. 185.

[3](#) Festugière, p. 102.

[4](#) Luckert, p. 55.

[5](#) Lurker, p. 121.

[6](#) *Ibid.*

[7](#) Ray, p. 65.

[8](#) *Ibid.*, p. 160.

[9](#) Fowden, p. 34.

[10](#) Lurker, pp. 69–70.

[11](#) Ray, p. 165.

[12](#) Fowden, p. 27.

[13](#) *Ibid.*, pp. 40–1.

[14](#) According to Plutarch (p. 161) the establishing of the Serapis cult was the work of Manetho and a member of the family that held the hereditary priesthood of the Greek mystery centre of Eleusis, which makes sense if it was to be a ‘hybrid’ cult for Egyptians and Greeks. Although some doubt Plutarch’s story, Manetho was certainly associated with the cult – see J. Gwyn Griffith’s notes to *ibid.*, pp. 387–8.

[15](#) Iamblichus, p. 5.

[16](#) Fowden, p. xxv.

[17](#) Churton, *The Gnostic Philosophy*, p. 120.

[18](#) Plotinus, p. 9.

[19](#) Luckert, p. 261.

[20](#) *Ibid.*, p. 262.

[21](#) Quoted in *ibid.*, p. 260.

[22](#) See *ibid.*, chapter 14.



[23](#) *Ibid.*, p. 257.

[24](#) Eunapius, 'Lives of the Philosophers', in Philostratus and Eunapius, pp. 419–25.

[25](#) Herodotus, p. 130.

[26](#) Luckert, p. 42.

[27](#) E.g. Lurker, p. 99.

[28](#) See Luckert, chapter 2.

[29](#) *Ibid.*, p. 52.

[30](#) Lurker, p. 31.

[31](#) Lehner, p. 34.

[32](#) Luckert, p. 52.

[33](#) *Ibid.*, p. 45.

[34](#) *Ibid.*, p. 57.

[35](#) Campbell and Musès, p. 138.

## CHAPTER EIGHT

# LAMENT HERMES

# FOR

Having looked beyond the historical clichés we see now that the scientific revolution, usually considered to have started with Copernicus and ended with Newton, was in fact the *Hermetic* revolution. Science emerged from the world of the occult in a very real and direct way. All the major players relied not just on the Hermetica's exhilarating image of humanity but also on its model of creation, which opened up their minds to the nature of the universe and its testable realities. Without Hermes Trismegistus we might never have reached the scientific age, or at

least we might only have done so much later in our history.

Hermeticism always encouraged a scientific mindset, even if that was, from a modern perspective, inseparable from a more esoteric worldview. By the end of the seventeenth century the scientific component had been brutally torn from its arcane twin and given an independent existence, but the fact remains that modern science *emerged from* Hermeticism.

Today most people accept the simplistic notion that chemistry emerged from alchemy, and astronomy from astrology, as a new generation realized the error of the old ways and ditched 'irrational' practices in favour of what could be weighed, measured and tested. And yet, as we have seen, most of the greatest movers and shakers of both Renaissance and even Enlightenment science did their best work because of their occult beliefs, not despite them. Their passion

for the esoteric went way beyond mere eccentricity or an occasional hobby but was a source of electric inspiration. This was especially so in the case of Isaac Newton, whose world-changing theories were a direct application of Hermetic magical principles to physical phenomena.

This book grew out of our desire to set the record straight, to bring the Hermetic tradition back out of the shadows to take its rightful place centre stage in the history of western civilization and culture. The *Hermetica* has had a greater impact on our civilization than any other collection of texts apart from the Bible, and a greater impact on modern history than any other collection of texts *including* the Bible. Even those who dismiss all things occult and Hermetic might at least have the grace to acknowledge that without them the world would be very different, and arguably much the poorer. Science as we know it may not ever have come into

existence. At the very least, the time to acknowledge our debt to the Hermeticists is long overdue.

And what achievers they were ... The Hermetic tradition directly or indirectly inspired giants such as Copernicus, Kepler, Gilbert, Galileo, Fludd, Leibniz and Newton. As well as these big names, the tradition included figures who should be remembered as their equals but who have been relegated to history's second or third divisions: Tommaso Campanella, John Dee and, above all, Giordano Bruno. Apart from the luminaries featured in our story, the tradition inspired much else in the artistic and literary realms, including the works and ideas of Leonardo da Vinci, Botticelli and William Shakespeare – a pretty impressive list, surely, by anyone's standards.

Hermes' books played a central role in the golden age of Arabic science, which preserved the knowledge of the classical world,

developed it and passed it back to Europe in the late Middle Ages. And the Hermetica was *the* mainspring of the Renaissance. Of course other ideas, attitudes and philosophies also contributed to that great flowering of the human mind and spirit – but the great tradition was what glued everything together.

Yet historians have long taught that other elements, such as the renewed interest in classical philosophy and learning, were at the core of the Renaissance. Hermeticism was grudgingly acknowledged, if at all, as a contributory factor, often hidden behind by the more familiar but off-puttingly dry ‘Neoplatonism’, or the slightly more interesting but vague label ‘humanism’.<sup>1</sup> But an objective examination of the motivations behind the great names of the period shows the opposite to be the case. The Hermetic philosophy was at the core of the Renaissance: it was the other factors, such as a renewed passion for the works of the ancient Greeks,

which were of secondary importance – and often a poor second at that.

Hermes' influence also continued as the Renaissance matured into the Age of Enlightenment, drawing to him as he did some of the new era's greatest intellects, including Newton and Leibniz.

Most of all, however, and with a fine flourish of irony, Hermeticism *was* the scientific revolution. This is no exaggeration. Just consider the following discoveries, which all owe an eternal debt to the Hermetica:

- The heliocentric theory
- The laws of planetary motion
- The concept of an infinite universe
- The idea of other solar systems containing habitable planets
- The theory of gravity
- The Newtonian laws of motion
- The circulation of the blood
- The Earth's magnetism



- The basic principles of information theory and the basic principles of computer science

The idea that mankind was of limitless potential and could do just about anything given the desire – the very *spirit* of science – also came from the teaching of Hermes. When the likes of Richard Dawkins declare that our achievements make him proud to be human, he is (presumably) unknowingly, speaking like an ancient occultist. *Magnum miraculum est homo!* The cosmic joke is not lost on Glenn Alexander Magee, who writes:

It is surely one the great ironies of history that the Hermetic ideal of man as magus, achieving total knowledge and wielding Godlike power to bring the world to perfection, was the 2 prototype of the modern scientist.

So why isn't the Hermetic tradition given due credit? Why is it the case, as Piyo Ratansi notes, that 'to grant Hermeticism any prominence in the history of sixteenth-and seventeenth-century science is tantamount,

apparently, to challenging the rationality of science'?<sup>3</sup>

A major reason for today's neglect is the well-established cultural bias that favours the classical world. Another is the lack of recognition, until recently, of the important contribution of Egypt's intellectual and philosophical traditions. However, this bias does not appear to be a cause but an *effect* of the neglect of the Hermetica. Until Isaac Casaubon's damning critique, even Hermes' enemies had accepted his works as the product of the most venerable period of the Egyptian civilization. Pouring cold water on the alleged wisdom texts, Casaubon tempted scholars in the opposite direction with his message that Egypt had nothing to teach us compared to the Greeks. Had Casaubon never put quill to paper, Egypt might well have remained a focus of scholarly respect, an equal of classical Greece and Rome. Had this been the case, twentieth-century academics

such as Garth Fowden and Karl Luckert would never have faced such an uphill struggle to persuade their colleagues that all of the extraordinarily powerful and inviting subjects that we have seen thus far had Egyptian rather than Greek roots. Casaubon was wrong anyway. As believed by Hermeticists all along, the *Hermetica* authentically preserved and transmitted the cosmology and philosophy of Egypt's pyramid age, which we believe has much to teach us – even in the digital age.

Another reason for the engrained prejudice against Hermeticism is that the study of the texts was essentially forbidden after the tradition's ambitions for religious and social reform suffered serious reversals during the seventeenth century. This came about because of a paradoxical collusion between the forces of science and religion. The Catholic Church condemned Hermeticism as demonic, both because it employed magic and its

perceived political threat. For their part, Protestant intellectuals backed off from the subject largely because Catholics had made it such a point of contention. One of the consequences of the power politics of the day was that it became expedient to be seen as an occult-denier, especially when the opposite could get you burnt at the stake. But the practical necessity to play it safe effectively sucked the lifeblood from the Hermetic tradition. Men of science were thus no longer men of God – or of the spirit – and soon it seemed that the two were mutually exclusive. Scientists not only denied the very existence of their predecessors' inspiration, but also had no choice but to denigrate its source.

We saw in the story of the origins of the Royal Society the struggle between the Rosicrucian attitude and the new impersonal mechanistic experimental philosophy. There were good reasons for minimizing the influence of magic, even in Restoration England.

A campaign to lose the esoteric gained favour in English academic circles, and this led those of an overt Rosicrucian or Hermetic bent to be branded sinister – and possibly satanic. In 1659 a work based on a hostile editing of John Dee's diaries, *A True and Faithful Relation of What Passed for Many Years Between Dr John Dee ... and Some Spirits*, was published. Written by Méric Casaubon – Isaac's son, so keeping up the family tradition – it uncompromisingly painted Dee as a necromancer in league with the Devil. While it is true that with a dodgy clairvoyant named Edward Kelley, Dee had experimented over a number of years with communications with discarnate entities, they were allegedly angels rather than demons or spirits of the dead. But Casaubon Junior's book effectively trashed Dee's reputation for centuries and also cast suspicion on those who respected and worse, *used*, the good doctor's mathematical works. This was

particularly unfortunate as, whatever one might think of Dee's esoteric studies, his was one of the greatest mathematical minds of all time.

The move from the Hermetic studies of the Renaissance to what we recognize today as science, the great intellectual flagship for rationalism and mechanism and all other resolutely non-magicalisms, was the result of the occult philosophy splitting into two parts: the magical view of the universe and its application to the phenomena of nature. Basically the theory was junked in favour of the practice.

It is often assumed that science emerged when thinking people began to question religion. This is not so: it was a specific reaction against Hermeticism – one that was actively encouraged by those members of the Catholic Church who backed Descartes' new method. What is perhaps odd given such a momentous schism, is that it was largely an

accident of history that science diverged from the ancient and much loved philosophy that inspired it.

Hermeticism as a system of thought survived the Enlightenment. But just as it diverged from science, the philosophy itself became firmly the province of the occult underground and the world of secret initiatory societies. Study of the *Corpus Hermeticum* as anything other than a historical curiosity came to be reserved for students of the esoteric and magic.

The first Rosicrucian secret societies proper, formed in emulation of the brotherhood described in the *Fama Fraternitatis* and *Confessio Fraternitatis R.C.*, appeared in Germany in the first decades of the eighteenth century, part of the burgeoning interest in Freemasonry and Masonic-style organizations. However, despite claiming to be inspired by the Rosicrucian ideal, these societies were actually the opposite, exploiting

the mystique around the original invisible society to add an elitist gloss to their own image while keeping their secrets, real or imagined, to themselves.

In Britain, these underground currents that flowed through Europe resulted in the influential esoteric society the Hermetic Order of the Golden Dawn. Founded in the 1880s, it not only attracted the usual suspects – famous occultists such as Aleister Crowley and Dion Fortune – but also the likes of Irish poet and patriot W.B. Yeats and, according to rumour, the originator of *Dracula*, Bram Stoker. To these and many others who dealt in the symbolic keys and the secret initiations that would open up both their psyches and their minds, Hermes was a god like no other, for to follow him was to become divine oneself. He has proven himself to be equally present in the lilt and lift of language and in the fire of the cosmos.



Hermeticism survived in other, less expected ways. For example, Romantic poets such as Percy Bysshe Shelley and John Keats breathed Hermetic fire into their works as well as into their remarkably colourful and short lives. And the influential philosopher Georg Wilhelm Friedrich Hegel (1770–1831) – whose thinking inspired Karl Marx among others – was an unashamed Hermeticist. His writings, both published and unpublished, are packed with references to masters such as Bruno – whose brilliance was the subject of Hegel’s lectures – and his library included books by Hermeticists and esotericists, including Agrippa and Paracelsus. Yet it took until 2001 for a study to acknowledge his Hermetic passion. Even then Glenn Alexander Magee’s *Hegel and the Hermetic Tradition* was regarded as a radical new view.

Many might think that although it a shame that the old Hermetic influence on certain important historic names is neither properly

nor widely recognized, surely the big split between magic and science turned out to be a good thing. After all, it allowed science to develop without the constraints of a metaphysical framework, leading to the explosion of discoveries and world-changing technologies such as steam trains, spinning jennys and telegraphy. Indeed, one could argue that Hermeticism was not necessary to make sense of this kind of scientific progress.

Up to the first half of the twentieth century, that argument might have worked. But since then science has shifted into a completely new phase, a considerably less certain world than that of Victorian nuts and bolts. And, we argue, Hermeticism is once again relevant, this time to the realm of quarks, M-theory and DNA.

As science itself becomes more magical, Hermeticism's time has truly come.

# Chapter Eight

[1](#) ‘Humanism’ is a fluid term, coined in the mid-nineteenth century and applied not just to contemporary ideas but also retrospectively to earlier philosophers and social reformers. It is applied to any philosophy that places human beings at the centre of things, asserting not only their fundamental right to control their own destiny but also stressing their *ability* to do so. But beyond that, the precise definition varies depending on the era in question: the values and ideals of a twenty-first century humanist are very different from a fifteenth-century one. The biggest difference is that today’s humanism tends to eschew the metaphysical and religious. Under this definition, the likes of Pico, Ficino and Bruno qualify as humanists, but they would never have recognized the term.

[2](#) Magee, p. 7.

[3](#) P. M. Rattansi, ‘Some Evaluations of Reason in Sixteenth- and Seventeenth-Century Natural Philosophy’, in Teich and Young (eds.), p. 149.

# PART TWO

# **The Search for the Mind of God**

## CHAPTER NINE

# THE DESIGNER UNIVERSE

The most fundamental element of the Hermetic worldview is, as we have seen, that the cosmos is not meaningless, inert or random, but is in its tiniest manifestation not only alive but also purposeful.

Unlike believers in the biblical version of creation, where God merely creates life and the universe on what appears to be a whim, to the Hermeticists as well as their ancient predecessors, the priests of Heliopolis, the material universe is nothing less than an emanation of God. In some majestically transcendent but also ultimately practical manner, the cosmos also represents his thought.

Obviously this isn't the way that the vast majority of modern scientists – as exemplified by Richard Dawkins and Stephen Hawking – see it. But we argue that it should be. Scientific cosmology has amassed a great deal of evidence about the nature of the universe that has seriously jolted the complacency of determined rationalists. The new data reveals a universe that is not merely the result of the blind workings of the immutable laws of physics. This universe emerges as being deliberately designed for a purpose in which intelligent life plays a crucial, if not *the* crucial, role.

The road to this point began back in the late 1970s when a paper appeared in the respected journal *Nature*, sending strong ripples through the scientific community worldwide. This was entitled 'The Anthropic Principle and the Structure of the Physical World' and was written by British physicists Bernard Carr and Martin Rees. Based on the



evidence of seven decades, the authors reflected on an unsettling pattern that was emerging from the accumulated discoveries of science: to an uncanny degree, the laws of physics seem to have been ‘fine tuned’ to allow the development of intelligent life.

Carr is now Professor of Mathematics and Astronomy at the University of London and, unusually for today, a member – and former president – of the Society for Psychical Research. Rees is the Astronomer Royal, Baron Lees of Ludlow, and since 2005, President of the Royal Society. The passage of time has done nothing to sway the authors of the paper from their original conclusions. Carr was still saying in 2008 that judging by the fine tuning, ‘the universe is designed for intelligence’.<sup>1</sup> He is not alone. Leading cosmologists John D. Barrow and Frank J. Tipler similarly declared that:

there exist a number of unlikely coincidences between numbers of enormous magnitude that are, superficially, completely independent; moreover, these coincidences appear essential to

the existence of carbon-based observers in the Universe.<sup>2</sup>

Carr and Rees adopted British cosmologist Brandon Carter's term, first used in the 1960s of 'anthropic [man-centred] principle' to define the situation their paper examines. Carter mused about what the universe would be like if the laws of physics were different, and realized that for almost every variation, the universe they produced would be incapable of supporting life. But he later regretted 'anthropic', which refers only to humans; he had meant that the universe seems fine-tuned for intelligent life in general.

Of course, the notion that the universe was 'designed' for anything, let alone us, is unconscionable to the vast majority of scientists, since it contradicts the very basis of their discipline. Not only does it reintroduce the notion of a creator god but also the idea that the human species has some special relationship with Him/Her/It. As leading

theoretical physicist Leonard Susskind remarked:

This idea is of course anathema to physicists, who see the existence of themselves as an accidental property of a universe determined by mathematical principles, to be discovered by disinterested analysis.<sup>3</sup>

One can hardly imagine a more nihilistic worldview than that expressed by another theoretical physicist and Nobel Prize-winner, Steven Weinberg: ‘The more the universe seems comprehensible, the more it also seems pointless.’<sup>4</sup>

Of course, Carr and Rees were emphatically not claiming that they had found scientific evidence for the existence of God. They were highlighting a question that science had largely avoided, having only been explored by a handful such as Carter, and then only tentatively. The anthropic principle merely makes the observation that life could never have arisen except under very specific conditions, and does not necessarily

propose that they were put in place *in order* to produce life. The assumption behind Carr and Rees' paper was that what looks like design is really an illusion based on our human-centred perception of the cosmos: if the laws of physics were any different there would be no life to ponder this question in the first place. After all, just because we live on a habitable planet, it doesn't mean that the planet was created especially for us.

But they admitted that the odds were far too high to dismiss all the examples of apparent fine-tuning as coincidence. Some other, unknown, factor had to explain the illusion. As they concluded after surveying the many conditions that seemed so convincingly contrived:

One day, we may have a more physical explanation for some of the relationships discussed here that now seem genuine coincidences ... However, even if all apparently anthropic coincidences could be explained ... it would still be remarkable that the relationships dictated by

physical theory happened also to be those propitious for life. <sup>5</sup>

Perhaps this situation can be explained using the analogy of a lottery: if we win, we might ascribe our success to our skill in picking the numbers or believe we were somehow 'meant' to win, but in fact our triumph would be entirely due to chance. Much the same, the anthropic principle shows that the odds seem to have been stacked in life's favour, as if after scooping the jackpot we found that only our own numbers had been put into the machine.

Although the overwhelming majority of scientists believe that the rigging of the universal lottery machine can be explained purely in terms of an illusion – the 'weak anthropic principle' – there are some who ascribe to the 'strong anthropic principle', which stipulates that the universe is the way it is specifically to give rise to intelligent life. Among them is Freeman Dyson, the British-born American theoretical physicist, who wrote in 1979:

The more I examine the universe and study the details of its architecture, the more evidence I find that the universe in some way must have known we were coming. There are striking examples in the laws of nuclear physics of numerical accidents that seem to conspire to make

the universe habitable. [6](#)

# **‘A MONSTROUS SEQUENCE OF ACCIDENTS’**

In fact, the apparent fine-tuning of the universe involves so many factors that it is not merely the equivalent of winning the lottery once. This is scooping the jackpot week after week for several years.

The classic example of the fine-tuning is the origin of carbon, one of the most abundant elements in the universe, which is essential for the existence of life (as in the familiar phrase ‘carbon-based life forms’), at least as far as we can conceive it. Like all but the very simplest elements, carbon is manufactured in the centre of stars, the only places hot enough, at several million degrees, to allow the nuclear fusion that, in a literal

transmutation, builds the atoms of one element from those of others. At the beginning of the 1950s, scientists understood the principle behind the formation of carbon, but not precisely how the process worked, as there seemed to be an insurmountable obstacle. Although an atom of carbon is made from the fusion of the nuclei of three atoms of helium, this should be an extraordinarily rare event, as first *two* helium nuclei had to fuse, and the resultant structure (an atom of beryllium) is so unstable it should be impossible for it to survive long enough for a third nuclei to join the act. If carbon managed to exist at all, it should be a very rare element, whereas of course the universe is actually overflowing with it. Clearly, some kind of special condition exists that increases the chances of the three helium nuclei coming together.

In 1951 a British astronomer, the celebrated – and to some, notorious – scientific



maverick Fred (later Sir Fred) Hoyle, speculated that the solution to the mystery surrounding carbon was that the nucleic energy is enormously amplified by an aspect of quantum theory called resonance. This would prolong the life of the beryllium and so greatly increase the chances of the third helium nuclei joining the party. From this premise, Hoyle was then able to calculate what the energy of the resonance ought to be. An American team at the California Institute of Technology (Caltech), the only place at that time where such experiments could be carried out, tested Hoyle's prediction and found he had been precisely correct. This was a watershed event in the modern history of science, marking an enormous leap in the understanding of the way all elements are created. But while the American team were honoured with a Nobel prize for the discovery, blunt Yorkshireman Hoyle was overlooked (as we will see in the next chapter),

almost certainly because by the time the prizes were awarded in the mid-1980s, Hoyle had championed two controversies too far – the theory of panspermia, the idea that life came to Earth from space, and that of the ‘intelligent universe’.

What really intrigued Hoyle was the precision of the energy ‘spike’ produced by the resonance, known as the triple-alpha process. If it were just one per cent higher or lower the reaction would fail, ultimately leaving only tiny amounts of carbon in the universe, and therefore no life. As there seemed no reason for the resonance to be so precise *except* to make the process work, Hoyle went so far as to describe it as a ‘put-up job’.<sup>7</sup>

The significance of the triple-alpha process goes beyond the creation of carbon, since all other elements necessary for life depend on it. Stellar ‘factories’ work by adding nuclei to the atoms of one element to make a new,

more complex, one. Just as beryllium atoms have to form before carbon can be made, so carbon atoms are needed to make oxygen, oxygen atoms to make neon, and so on. All these reactions are more straightforward than the triple-alpha process as they don't require the convenient energy spike, so the obstacle Hoyle faced isn't present. But if carbon did not exist, then neither would any of the elements above it on the periodic table. Literally everything depends on the triple-alpha process. Without it there would only be four elements in the entire universe.

Such apparent contrivances prompted Hoyle to declare in a lecture at University Church, Cambridge, in 1957:

If this were a purely scientific problem and not one that touched on the religious problem, I do not believe that any scientist who examined the evidence would fail to draw the inference that the laws of nuclear physics have been deliberately designed with regard to the consequences they produce inside the stars. If this is so, then my apparently random quirks have become part of a deep-laid scheme. If not then we are back again at a monstrous sequence of

8  
accidents.

Since Hoyle made that statement, the more science has discovered about the origins and evolution of the universe the more ‘monstrous’ the ‘sequence of accidents’ has become.

One of the first to be intrigued by Brandon Carter’s anthropic principle was British cosmologist Paul Davies – that rare animal, both a highly regarded academic and a successful popular science writer. He has continued to explore the implications and mysteries of the anthropic principle, most famously in *God and the New Physics* (1983) and *The Mind of God* (1992), and most recently in *The Goldilocks Enigma* (2006) – the title referring to the conditions in the universe that are, like Goldilocks’ porridge, ‘just right’ for life.

Davies points out that life has three main requisites: ‘stable complex structures’ in the universe (galaxies, stars and planets rather than clouds of gas or vast numbers of black

holes); certain chemical elements (for example carbon, oxygen); and a place where the components can come together (for example the surface of a planet). Of course our universe has all of these components, but each requires such fortuitous circumstances to exist that ours is, as Davies puts it, apparently a 'designer universe'.<sup>9</sup>

The universe as it is today is, of course, the result of how it was in the beginning. Had conditions been different then, it would be different now – and almost certainly hostile to the development of life. According to today's thinking, the universe began 13.7 billion years ago with the 'big bang'. (Ironically the term was invented by the sceptical Hoyle, but only as a put-down. Then to compound the irony, his team found some of the best supporting evidence for it.) The big bang had to be within a certain range of size and explosive potential to produce our universe. If it had been bigger or bangier, it would have

expanded too quickly for galaxies to form. If it had been smaller or less bangy, gravity would have pulled the universe back into itself well before life could have evolved.

For a time after the big bang the expanding universe was too hot to be anything other than a dense, incandescent plasma composed of subatomic particles like protons, neutrons and electrons. As it expanded further it cooled, so that – an estimated 380,000 years after the big bang – the particles could fuse to form the simplest elements, hydrogen and helium. Those two elements make up about 99 per cent of matter in the universe. But if the relative masses of protons, electrons and neutrons were only minutely different, not a single hydrogen atom could form. It seems we must boldly go well beyond the frontiers of coincidence to begin to understand the way our universe was created, and how it continues to work.

Attracted by the gravity of individual atoms, clouds of hydrogen and helium clump together, clumping faster and faster as they grow. The smaller the clumps, the hotter they become, until they are hot enough to kick-start nuclear reactions – and it is at this stage that a star is born, whose deadly beauty masks its true self, a gigantic fusion reactor. Acting as unimaginably massive factories that manufacture more complex elements from hydrogen and helium, stars then disperse these into space where they explode as supernovae. Every atom in every molecule, including those that make us up, was born in a star light years away, millions or billions of years ago, making even the tiniest newborn in some respects old beyond imagining. As the legendary American theoretical physicist Richard P. Feynman observed, ‘the stars are of the same stuff as ourselves’.<sup>10</sup> And as Paul Davies comments:

The life cycle of stars provide just one example of the ingenious and seemingly contrived way in which the large-scale and small-scale aspects of physics are closely intertwined to produce

complex variety in nature. [11](#)

But contrived and intertwined by whom or what?

There are also many instances where a combination of factors has to work together to produce a bio-friendly outcome – almost as if knobs are being twiddled until the balance is exactly right. In his 1999 book *Just Six Numbers*, Bernard Carr explored six of the fundamental forces, or relationships between forces, on which the universe is built. He found that all of them are very finely balanced, and if they were just slightly smaller or larger they wouldn't produce a life-friendly universe. As he pointed out in 2008:

Known physics does not explain these fine tunings. It seems indisputable that these relationships are required in order that life can arise, and they're really quite precise – they don't determine constants uniquely, but they do determine constants to, say, within something like

10 per cent, and there simply is no explanation. [12](#)



## Stephen Hawking also acknowledges this remarkable phenomenon:

The laws of nature form a system that is extremely fine-tuned, and very little in physical law can be altered without destroying the possibility of the development of life as we know it. Were it not for a series of startling coincidences in the precise details of physical law, it

seems, humans and similar life forms would never have come into being. <sup>13</sup>

Perhaps the most astonishing example of fine-tuning comes from the most recent to be discovered. In order to describe it, we need to start from the apparently bizarre premise that there is no such thing as ‘empty’ space; even the interstellar vacuum is filled with ‘virtual particles’ that nevertheless possess energy. This has an effect on the rest of the universe, specifically on the rate at which it is expanding. In theory at least, the ‘vacuum energy’ has huge potential significance in terms of the anthropic principle. Some of the particles will be positive, some negative. If the sum total of all the vacuum energy were positive, then the expansion of the universe

would be accelerating, and if it were above a certain value then the universe would have expanded too fast for galaxies to form – matter would fly apart faster than gravity could pull it together. On the other hand, if the vacuum energy were negative, the life cycle of the universe – from big bang to big crunch – would be too short for life to evolve.

In practice, however, the presence of vacuum energy was not considered too important – at least until the 1990s. The rate of expansion was believed to be constant, neither accelerating nor decelerating, which meant that the vacuum energy played no part in the process. This, in turn, meant it must have a net value of zero – that is, all the energy in the vacuum was neatly balanced, the positive and negative particles cancelling each other out. Cosmologists had no idea why, but that's what the data suggested.

But in the mid-1990s this happy state of affairs suffered a major jolt, as independent

research based on new, more accurate data from sources such as the Hubble Space Telescope showed that the rate of expansion is, in fact, speeding up. This means that the vacuum energy has a slight positive value, not all of which is cancelled out by the negative. It is only a tiny imbalance: calculations showed that the positive energy value is  $10^{120}$  times (that's 119 zeroes after the decimal point before you even get to the 1) less than the total positive energy in the vacuum. In other words, the negative energy cancels out all the positive – apart from a minute portion.

Learned jaws were on the ground yet again when it was realized that if that value was just one decimal place shorter – that is, the actual positive energy was  $10^{119}$  times less than the total (or 118 zeroes after the decimal point and before the 1) – then the universe as we know it simply couldn't exist. It would expand too quickly for galaxies, stars or planets

to form. Referring to this as the ‘biggest fix in the universe’, Davies points out that this tiny difference – a point between the 119<sup>th</sup> and 120<sup>th</sup> decimal place – is the thinness of the knife edge on which all life is balanced.<sup>14</sup> In answer to the dilemma posed by this ‘staggeringly precise’ balancing of the vacuum energy, Leonard Susskind writes: ‘This seems like an absurd accident and we have no idea why it should happen. There is no fine-tuning quite like this in the rest of physics.’<sup>15</sup>

However, while acknowledging that there is no viable alternative to an ‘anthropic explanation’,<sup>16</sup> Susskind does not imply the existence of a ‘grand designer’. For him this phenomenon can only be explained by whatever is behind the anthropic effect as a whole, which to the conventional scientist means the *illusion* of design. For Susskind, however, as for many scientists, there is only one solution to the conundrum: the

marvellous and all-encompassing notion of ‘multiverse’.

# INSIDE THE MULTIVERSE

According to the fans of this fashionable hypothesis, there are millions or billions, perhaps an infinite number, of universes, co-existing invisibly alongside our own, each governed by its own laws of physics. We just happen to live in one that happens to be bio-friendly. It may appear to have been custom-made for us, but as this universe is, by definition, one that will sustain our sort of life and the only one we can perceive, this is the only one we know about.

The multiverse is a concept that turns the virtually impossible into the almost inevitable. To use the lottery analogy again, if your ticket automatically entered you into several million draws simultaneously, it would hardly be surprising if your numbers came up somewhere. The same logic dictates that

by positing millions upon millions of universes, the odds that at least one would boast the right conditions for life are drastically shortened.

The multiverse theory is the only alternative to design that remains within the bounds of scientific credibility and allows the anthropic conundrum to be debated without professional anxiety. Bernard Carr explains that physicists regard the multiverse hypothesis as the ‘legitimization’<sup>17</sup> of the anthropic principle.

However, unfortunately for its many scientific fans, there are major problems with the multiverse. First, and surely the most damning, is that it is purely a theory with not a shred of solid data to back it up. There are three basic, competing models of the genesis of multiple universes that may keep physicists agog with debate and busy formulating mathematical models of how they might work, but this seems a hollow exercise as

none of the models have the remotest hope of ever being proved. In fact, it is impossible to gather evidence because interaction between universes is by definition also impossible.

On the other hand, multiverse theory can be used to predict certain features of *this* universe. But as American theoretical physicist Lee Smolin, founder of the Perimeter Institute for Theoretical Physics in Ontario, Canada, notes:

Within the standard model of elementary-particle physics, there are constants that simply don't have the values we would expect them to have if they were chosen by random distribution among a population of possibly true universes ... In fact, I know of no successful predictions that have been made by reasoning from a multiverse with a random distribution of

[18](#)  
laws.

The theory also violates another highly-prized scientific principle, Occam's razor, expressed by the great twentieth-century physicist Sir James Jeans as, 'We must not assume the existence of any entity until we are compelled to do so', [19](#) or, in other words, the



simplest explanation is usually the best. As Paul Davies wryly comments: ‘To invoke an infinity of other universes to explain one is surely carrying excess baggage to cosmic extremes.’<sup>20</sup>

The complete absence of evidence does not justify the extraordinary confidence with which the multiverse is promoted as a solution to the anthropic conundrum. In a 2008 radio discussion, British theoretical physicist Fay Dowker stated that ‘the existence of the multiverse, if we can establish it, would eliminate the question of why the laws of nature are the way we see them’.<sup>21</sup> *If we can establish it ...*

In the introduction to *Universe or Multiverse?* (2007), Carr acknowledges that the multiverse hypothesis:

... is highly speculative and, from both a cosmological and a particle physics perspective, the reality of a multiverse is currently untestable. Indeed, it may always remain so, in the sense that astronomers may never be able to observe the other universes with telescopes and particle physicists may never be able to observe the extra dimensions with their accelerat-

## He goes on:

For these reasons, some physicists do not regard these ideas as coming under the purview of science at all. Since our confidence in them is based on faith and aesthetic considerations (for example mathematical beauty) rather than experimental data, they regard them as having

more in common with religion than science. [23](#)

In recent years the multiverse theory has become inextricably bound up with two others: string theory and the related M-theory. These are now locked in a symbiotic – indeed circular – relationship. To put it baldly, one is taken as proof of the other. Unfortunately, however, both the string and M-theories suffer from the same problems as the multiverse. And a growing chorus of physicists are volubly expressing doubts about their validity and whether, despite all the time, effort and often almost hysterical enthusiasm devoted to them, they are nothing more than a complete dead end. One of the most withering attacks on string theory came

in 2006 from Lee Smolin in his book *The Trouble with Physics*.

String theory – often called ‘superstring theory’ in a rather pitiable attempt to make it sound sexier – posits that instead of being single points, subatomic particles are all manifestations of a single type of vibrating one-dimensional string-like entity that expand and contract as they gain or lose energy. As they are beyond tiny, one-trillionth of a trillionth the size of an atom, obviously no one has ever seen one. They only definitely exist within the realm of mathematical formulae.

String theory was formulated in the mid-1980s and was quickly recognized as the best hope for the physicists’ dream of a theory that would unify relativity and quantum theories, the grand unified theory or theory of everything. However, it rapidly moved in the opposite direction. As it failed to explain certain things, variations were suggested to

account for them, and so every attempt to fix the initial problem ended with another variant of the basic theory – adding new excrescences to the equations. As the number of variations multiplied exponentially, creating new sub-theories, each with its own problems, attempts to fix those led to more variations. And so on.

Physicists belonging to the old guard reacted with alarm. Richard Feynman declared: ‘I don’t like that for anything that disagrees with an experiment, they cook up an explanation – a fix-up to say, “Well, it still might be true.”’<sup>24</sup>

The numbers involved are literally beyond imagining. Based on the currently-understood value of certain cosmological parameters, when all the different variables are taken into account, there are around  $10^{500}$  possible versions of string theory. That’s 1 followed by 500 zeroes – difficult enough to

write down, let alone imagine – about six times the number of atoms calculated to exist in the observable universe. As Smolin points out:

Even if we limit ourselves to theories that agree with observation, there appear to be so many of those that some of them will almost certainly give you the outcome you want. Why not just take this situation as a *reductio ad absurdum*? That sounds better in Latin, but it's more honest in English, so let's say it: if an attempt to construct a unique theory of nature leads in-

stead to 10<sup>500</sup> theories, that approach has been reduced to absurdity. <sup>25</sup>

In 1995 the term ‘M-theory’ was coined in an attempt to bring order to the chaos. M-theory simply means the single theory that is assumed to lie behind all the variations of string theory and which, once established, will reconcile them all. Although ‘M’ was chosen randomly – like labelling an unknown quantity ‘X’ – those to whom it is the ultimate answer have happily tied themselves in knots trying to work out what it means, suggesting it might stand for ‘magic’, ‘mystery’ or ‘mother’. Those who are undecided about its value suggest maybe it stands for ‘maybe’. Sceptics prefer ‘myth’. Despite

the fact that M-theory is simply shorthand for a desperately needed, but currently non-existent solution to the complex problems posed by string theory, many physicists now solemnly make statements like ‘according to M-theory ...’

In a seminal paper in 2002, Leonard Susskind, the ‘father of string theory’, one of those who originally formulated it in the late 1960s, proposed a unification of the string and multiverse theories that made a virtue of the vagueness of M-theory. He was compelled in that direction by the astonishing precision of the near-cancellation of vacuum energy that we discussed earlier, which he realized could only point to an anthropic explanation. Susskind proposed that every variation of string theory was as correct as any other – each simply defines the laws of physics for a different universe. In what he termed a ‘landscape’ of string theories, he proposed that rather than one theory of

everything, there are really lots of 'everything's', each with its own theory.

So, although the term 'M-theory' was originally invented as an umbrella term for the  $10^{500}$  competing variations of string theory, its advocates, most prominently Susskind and Stephen Hawking, have turned it into a single theory in its own right. This 'proves' that there are  $10^{500}$  different string theories defining the laws of physics for  $10^{500}$  different universes, and is therefore taken as proof that the multiverse is real.

This may be an ingenious exercise in explaining one unknown by another, but that's all it is. As we have seen, the multiverse theory is after all by definition untestable, and M-theory unproven to say the least. As Jim Al-Khalili, theoretical physicist at the University of Surrey, commented:

The connection between this multiverse idea and M-theory is ... tentative. Advocates of M-theory ... would have us believe that it is done and dusted. But its critics have been sharpening their knives for a few years now, arguing that M-theory is not even a proper scientific

The situation thus becomes very much like the argument between those who insist their chosen god is bigger and better than any other, a line that so rouses Richard Dawkins' ire. To him, this is ludicrous even to begin to debate, as *no* gods exist. Yet here we have a very similar attitude. The arguments about multiverses and string theory are basically *theological* debates without a god or gods.

Clearly the multiverse explanation of why we live in a bio-friendly universe is (to put it as kindly as we can) at best speculative. As Smolin comments, because the multiverse hypothesis can't be confirmed by direct observation, it can't be used as an explanation and conversely, 'the fact that we are in a biofriendly universe cannot be used as a confirmation of a theory that there is a vast population of universes.'[27](#) The late John



Archibald Wheeler, who took on Einstein's mantle in the 1950s, discovered black holes and is widely regarded as the greatest theoretical physicist of modern times, considered the multiverse as unscientific speculation that carried 'too much metaphysical baggage'.<sup>[28](#)</sup>

Paul Davies explores an ironic and amusing twist to the multiverse theory, one that takes the story into rather unsettling *Matrix*-esque territory. This invokes another sci-fi idea that is nevertheless taken seriously by many scientists, that of simulated universes. Building on the ideas of British philosopher Nick Bostrom, Davies explored the implications presented by the simulated universes concept in the 'design vs. multiverse' debate.

As Davies pointed out in an article in 2003, since multiverse theories posit an infinite number of universes, anything anyone can think of will inevitably happen in one or more of them. Although only rarely will one

universe possess the right conditions for life, there will still be masses of inhabited universes. (After all, what's a small percentage of infinity?) In some of them, civilizations will have arisen that are so technologically advanced they will have developed their own computer-simulated, *Matrix*-style universes. For all we know, we might be living in one. (But how would we ever know if there were no red pills?) After all, a civilization that can simulate one universe can simulate many. As Bostrom points out, the ability to run such simulations wouldn't remain confined to a civilization's scientists, but would eventually filter down to students, schoolchildren, artists and even hobbyists. Programmers might even create universes where the inhabitants are advanced enough to simulate their own universes. The logical outcome would be that the *majority* of universes would be artificially designed.<sup>[29](#)</sup>

This provocative scenario does, of course, depend on the multiverse theory being correct in the first place, and Davies is far from convinced of this. The point of his paper is that *if* one accepts the multiverse, then one also has to accept that the odds are in favour of our universe actually being simulated. So, pushed to its logical conclusion, even the multiverse theory supports the idea of design!

What surprised Davies was the enthusiasm with which proponents of the multiverse such as Lord Rees took to their idea.<sup>[30](#)</sup> They are much more willing to accept that our universe is designed by a computer programmer than that it was designed by a God or gods – even though the distinction is of course, essentially merely semantic. To humanity the Great Programmer(s) would *be* divine and omnipotent – so they might as well be gods.

# A DESIGN FOR LIFE

Even with such prestigious opponents as Wheeler, most physicists and cosmologists accept the multiverse theory. But are so many of the best modern scientific minds simply clinging to it just because they're afraid of facing the very unwelcome implications of the anthropic principle?

The evidence underpinning the anthropic principle suggests one of two scenarios: either the cosmos was intelligently designed, specifically to produce intelligent life, or there is something about it that makes it *seem* like this is the case. The only suggestion that has been made about what that 'something' might be is the multiverse. This presents us with a straight choice between one or the other. And if the multiverse is

wrong then science itself proves that the universe is designed for life.

This choice is recognized by most leading physicists such as Stephen Hawking, who writes that the anthropic principle ‘suggests either intelligent design or, if there are trillions of universes as M-theory proposes, that luck and probability are enough to make our existence feasible’.<sup>31</sup> In his 2010 *The Grand Design*, co-written with Leonard Mlodinow, he comes down firmly on the side of the multiverse and M-theory, which led to his well-publicized pronouncement that God did not create the universe, while acknowledging that M-theory hasn’t yet been proven. Jim Al-Khalili, however, points out that this is essentially the same logic as those used by religionists. While they use fine-tuning, along with their faith, as evidence for the existence of God, Hawking and his fellow advocates of M-theory seize on it – together with the

assumption that there is no God – as evidence for their own hypothesis.<sup>32</sup>

No less a figure than Steven Weinberg, the eminent American Nobel prize-winning theoretical physicist, when discussing the enigma of the vacuum energy, writes that if further research confirms this seemingly miraculous balancing act ‘it will be reasonable to infer that our own existence plays an important part in explaining why the universe is the way it is’.<sup>33</sup> Susskind calls Weinberg’s statement ‘the unthinkable, possibly the most shocking admission that a modern scientist could make: man’s place in the universe may indeed be at the centre’.<sup>34</sup> Of course, despite those words, Weinberg, champion of the ‘pointless universe’, will not agree for a moment that man is at the centre of things. He goes on:

For what it is worth, I hope that this is not the case ... I hope that string theory really turns out to have enough predictive power to be able to prescribe values for all the constants of

nature ... <sup>35</sup>

But if string theory finally and comprehensively falls, as it shows every sign of doing, then we will be left with Weinberg's reasonable inference that the presence of intelligent life is fundamental to explaining the universe. This would mean that science itself effectively provides overwhelming evidence for the designer universe, which of course means there must be a Grand Designer.

We are often told that science is an evolving, self-correcting process, in which its laws and theories are never fixed but merely contingent, the best conclusions that can be drawn from the available data. It is also implicitly understood that future discoveries may completely overturn current thinking and lead to a revision of the theories. But when it comes to the anthropic principle this reasoning suddenly falls by the wayside.

The best available data from physics – the hard facts it has amassed, which can then be tested experimentally and empirically – points unequivocally to a universe fine-tuned for intelligent life. However, the majority of scientists argue that one day we will have better data that will show this to be an illusion. But all their supporting ‘evidence’ is theoretical, speculative and untestable. We can imagine what would happen in any other field of human endeavour if someone admitted they had factual evidence pointing in one direction, but then declared they can think of a hypothetical reason why the opposite, which unfortunately is impossible to test, is true.

Why should this be? Why should the normal rules of science change when it comes to the anthropic principle? The justification for making it a special case is that a designed universe violates one of the most fundamental principles on which the scientific



worldview and method is based. The scientific revolution, we are told, came about when thinkers realized that physical phenomena could best be explained in terms of mechanical processes and laws that are purely a consequence of the way the universe is – without presupposing the existence of a designing and guiding intelligence.

However, as we saw in Part One, this is *not* the way the scientific revolution happened. *All* of its great figures – Copernicus, Kepler, Galileo, Newton, Leibniz – based their work on the understanding that the universe *was* intelligently created and that human intelligence plays a key part in its design and purpose. Bruno even anticipated the existence of other, more advanced extraterrestrial intelligences, which fits the strong anthropic principle even more neatly. None of them would have had any problems with the implications of the anthropic principle; they would have taken it for granted. And they certainly

wouldn't have tied themselves in theoretical knots to evade the evidence staring them in the face.

Opponents of design point out that the hypothesis is just as untestable as the multiverse theory. That is not the case. The hypothesis of creation by deity or deities unknown does allow for the formulation of testable predictions. What predictions? Simply, if the universe is designed for intelligent life then the more our understanding of physics advances, the more we will uncover evidence of such design. Which is, of course, exactly what has happened. The design hypothesis passes that test.

A few scientists have at least been open to the notion of some form of design. Fred Hoyle proposed that the 'intelligent universe' (the title of his 1983 book) is a purposeful, creative entity evolving towards some specific end. Hoyle also scathingly dismissed the usual scientific response to the anthropic

principle, calling it ‘a modern attempt to evade all implications of purpose in the Universe, no matter how remarkable our environment turns out to be’.<sup>36</sup>

The most high-profile scientific advocate of the design idea now is Paul Davies, who summed his position up in *The Mind of God* (1992):

Through my scientific work I have come to believe more and more strongly that the physical universe is put together with an ingenuity so astonishing that I cannot accept it merely as a brute fact. There must, it seems to me, be a deeper level of explanation. Whether one wishes to call that deeper level ‘God’ is a matter of taste and definition. Furthermore, I have come to the point of view that mind – i.e. conscious awareness of the world – is not a meaningless and accidental quirk of nature, but an absolutely fundamental facet of reality. That is not to say that *we* are the purpose for which the universe exists. Far from it. I do, however, believe

that we human beings are built into the scheme of things in a very basic way.<sup>37</sup>

However, perhaps oddly, the theory that suffers the most from the design interpretation of the anthropic principle is the traditional idea of God as creator, because it exposes the limitations of his divine power.

The God of the Judeo-Christian religion, for example, created worlds from his will and

word alone, and fashioned Adam out of clay and Eve from a rib bone. This was not metaphorical, but *literal*. After that particular tour de force tweaking the resonance of the helium nuclei or making a minor adjustment to the strength of the weak nuclear force in order to produce a man and woman millions of years later is something of an anticlimax.

This is not properly understood (or perhaps it is, but evaded) by those representatives of organized religions who use the evidence for design in support of their own doctrines. We find ourselves in the unusual position of agreeing with a pope, John Paul II, in his 1985 statement that to dismiss the scientific evidence for design in the universe as being a simple coincidence 'would be to abdicate human intelligence'.<sup>38</sup> But we profoundly disagree that such evidence supports the existence of the God of the Bible, and therefore of the Catholic Church.

The Cardinal Archbishop of Vienna, Christoph Schönborn, supremely missed the point when he declared in 'Finding Design in Nature', published in the *New York Times* in 2005, that by refusing to accept chance explanations for the way the universe works, the Catholic Church is 'standing in firm defence of reason' and that it will again defend human nature by proclaiming that the immanent design evident in nature is real'.<sup>39</sup> He is quite wrong: the evidence of design *disproves* the Catholic teachings about God, and it is disingenuous to pretend otherwise.

However, the 'designer universe' concept does support the cosmology of the Hermetic tradition, as well as the Neoplatonists' and the Heliopolitan theology that we argue lay behind them. Paul Davies notes that the kind of designer suggested by the strong anthropic principle fits the model of the Demiurge – the lesser or, in the words of the *Hermetica*, 'second god', whose creative power is

constrained by matter – rather than the omnipotent God of Judeo-Christian tradition.<sup>[40](#)</sup> So in this respect at least, science supports the Hermetic tradition.

At this point the exact nature of the designer isn't the most important consideration. If we have to use a term that doesn't commit us to any specific image, we suggest Grand Universal Designer – or the good GUD almighty.

In this chapter we have only explored the *conditions* that made the universe ripe for life. If GUD exists, we should be able to see evidence of his or her hand elsewhere in nature, particularly in the emergence and development of intelligent life. On the other hand, other branches of science may utterly demolish poor old GUD by demonstrating conclusively that certain phenomena could only happen through the workings of pure chance and blind forces. But which way does it go?

# Chapter Nine

- 1 In the radio programme 'The Multiverse', part of the *In Our Time* series, broadcast on BBC Radio 4 on 21 February 2008.
- 2 Barrow and Tipler, p. 5.
- 3 Susskind, 'A Universe Like No Other', p. 38.
- 4 Weinberg, *The First Three Minutes*, p. 154.
- 5 Carr and Rees, p. 612.
- 6 Dyson, p. 44.
- 7 Quoted in Davies, *The Mind of God*, p. 199.
- 8 Stockwood (ed.), p. 64.
- 9 Davies, *The Mind of God*, Chapter 8.
- 10 Feynman, p. 12.
- 11 Davies, *The Mind of God*, p. 197.
- 12 In the BBC Radio 4 programme 'The Multiverse' (see note 1 above).

- [13](#) Hawking and Mlodinow, p. 161.
- [14](#) Davies, *The Goldilocks Enigma*, pp. 166–70.
- [15](#) Susskind, ‘A Universe Like No Other’, p. 37.
- [16](#) *Ibid.*, p. 39.
- [17](#) In the BBC Radio 4 programme ‘The Multiverse’ (see note 1 above).
- [18](#) Smolin, *The Trouble with Physics*, pp. 166–7.
- [19](#) Jeans, p. 96.
- [20](#) Davies, *The Mind of God*, p. 173.
- [21](#) In the BBC Radio 4 programme ‘The Multiverse’ (see note 1 above).
- [22](#) Carr, p. 14.
- [23](#) *Ibid.*
- [24](#) Quoted in Smolin, *The Trouble With Physics*, p. 125.
- [25](#) *Ibid.*, pp. 158–9.
- [26](#) Al-Khalili, p. 23.
- [27](#) Smolin, *The Trouble with Physics*, p. 163.



- [28](#) Quoted in Malone, p. 191.
- [29](#) See Nick Bostrom, 'Are We Living in *The Matrix*? The Simulation Argument', in Yeffeth (ed.).
- [30](#) Davies, *The Goldilocks Enigma*, pp. 213–4.
- [31](#) Hawking, 'The Grand Designer', p. 25.
- [32](#) Al-Khalili, p. 23.
- [33](#) Weinberg, *Dreams of a Final Theory*, p. 182.
- [34](#) Susskind, 'A Universe Like No Other', p. 36.
- [35](#) Weinberg, *Dreams of a Final Theory*, p 182.
- [36](#) Hoyle, pp. 217–8.
- [37](#) Davies, *The Mind of God*, p. 16.
- [38](#) Quoted in Schönborn.
- [39](#) *Ibid.*
- [40](#) Davies, *The Goldilocks Enigma*, pp. 228–30.

## CHAPTER TEN

# STARDUST IS EVERYTHING

In the last chapter we saw that advances in cosmological understanding point firmly in the direction of the design interpretation of the anthropic principle, suggesting that the universe was intentionally fine-tuned – by whom or what we have no way of knowing – specifically to make it suitable for intelligent life. But this only concerns the physics, the manufacture of the elements necessary for life and the planets where it can dig in and thrive. What about the next step? How are living things actually made? And do the processes that create life support the designer universe hypothesis?

After all, if life itself turns out to be an incredible fluke, the whole idea of a designer universe would be undermined. On the other hand, if the laws of physics have been rigged to produce a universe agog for life, we would expect the rules of biochemistry to be similarly primed to ensure life develops wherever and whenever it can.

Frustratingly, however, matters are not as cut and dried as they are with the physics, since there are enormous gaps in the available data. Charles Darwin wrote to his great friend, the botanist Joseph Dalton Hooker, in 1863, four years after the publication of *On the Origin of Species*, saying: 'It is mere rubbish, thinking at present of the origin of life; one might as well think of the origin of matter.'<sup>1</sup> Although 150 years later we know considerably more about the origin of matter itself, our information on the origin of life is still largely 'rubbish'. Darwin's foremost modern apostle, Richard Dawkins, writes in

*The Greatest Show on Earth: The Evidence for Evolution* (2009) that ‘we have no evidence bearing upon the momentous event that was the start of evolution on this planet’.<sup>2</sup> ‘No evidence ...’ None whatsoever.

Since Darwin took the discussion of the evolution of life to a new level in the mid-nineteenth century, biologists’ growing understanding of the conditions necessary for complex life forms could be extrapolated in two diametrically opposite directions. Some still consider that the chain of events that led to life on Earth was so dependent on chance that organic life must be an extremely rare phenomenon, cosmically speaking. Some even argue that the odds are so stacked against the development of life that Earth may be unique in the universe. Yes, they claim, we are alone – get used to it. On the other hand, some believe the processes that produce life unfold according to rigid laws. What happened here will happen anywhere

given approximately the same conditions. And given the vastness of the universe, even if those conditions were rarer than multiverses with life, there will still be millions of suitable locations for it to exist.

Once upon a time most biologists believed that life was an exceedingly rare phenomenon at best. But new discoveries in the last two or three decades prompted specialists to see it as a common, even inevitable, feature of the universe. A phrase that is often bandied around is that life is a 'cosmic imperative': the ordering of the universe means that wherever conditions are such that life *can* evolve, it *will*, just as weeds will seize on the tiniest nooks and crannies to grow and thrive. Life just can't stop itself.

One of the foremost exponents of this school is Christian de Duve, the Belgian biochemist and cytologist who won a Nobel Prize in 1974 for his work on cells. In 1995 he published *Vital Dust: Life as a Cosmic*

*Imperative*, a detailed survey of the origin and development of life on Earth, from the first organic molecules to human beings. He writes:

... life is the product of deterministic forces. Life was bound to arise under the prevailing conditions, and it will arise similarly wherever and whenever the same conditions obtain. There is hardly any room for 'lucky accidents' in the gradual, multistep process whereby life origin-

ated.<sup>3</sup>

It may be early days yet, and the evidence may be nowhere near as conclusive as that for the fine-tuning that led to the formulation of the anthropic principle, but the very fact that the study of the origins of life, or abiogenesis, is moving in this direction is implicitly designer-universe friendly. This also fits in with the Hermetic principle that the universe is teeming with life – or at least the potential for life. Giordano Bruno took this line of thinking to its logical conclusion, arguing for the existence of other inhabited worlds.

The modern trend towards seeing life as a cosmic inevitability arose largely from the growing recognition that the universe is brimming with the building blocks of life – not just on planets but even in deepest space.



# ALIEN SEEDS

The spring of 1953 was a big time for abiogenesis: two seminal scientific papers appeared within just three weeks, fuelling great excitement in the subject. The first was published in the 23 April edition of the British scientific journal *Nature*, by James D. Watson (a somewhat maverick American biologist) and Francis Crick (British physicist-turned-biologist), announcing their discovery of DNA's double helix. Then on 15 May the American *Science* carried a paper by Stanley L. Miller on his and Harold Urey's re-creation at the University of Chicago of some of the fundamental chemical building blocks of life – most significantly certain amino acids – under simulated 'primitive Earth' conditions.

At the time, it was Miller who made the bigger splash. Watson and Crick's paper was about what was then considered a very uninteresting nucleic acid, only hinting cautiously, in its very last sentence, that it might actually be the long-sought medium of genetic inheritance: 'It has not escaped our notice that the specific pairing we have postulated immediately suggests a copying mechanism for the genetic material.'<sup>4</sup> But despite their laid-back comment, the discovery of DNA made the scientific landscape richer, more colourful and intoxicatingly alive with promise.

Miller's paper, on the other hand, offered much more hope for unlocking the origins of life. It seemed to confirm the prevailing theory that it began in the Earth's 'primordial soup' of biochemicals. The implication was that further research would reveal how the more complicated parts of the system came

into being through similar processes – all of them essentially blind.

As we now know, by unravelling the genetic mystery, Watson and Crick's discovery has had by far the greater impact, not just on science, but on our daily lives – witness, for example, the DNA 'fingerprinting' used to catch criminals. Urey and Miller haven't fared nearly so well, partly because although their experiments showed amino acids and certain other biogenic chemicals could be produced easily in the lab, taking it further and putting the building blocks together in any more complex way remained out of reach. Since 1953 it has also been discovered that creating, for example, amino acids doesn't require terrestrial conditions at all. Many of the building blocks of life have been found literally floating around in space.

For a long time it was assumed that however life on Earth originated it happened *on* Earth. Even over a century ago this was

not without its challengers, however. Great names of the Victorian age such as German physicist Hermann von Helmholtz and British physicist and engineer Lord Kelvin advocated that the seeds of life could be carried between planets by meteors and comets, a theory that was termed ‘panspermia’ in 1907 by the Nobel-prizewinning Swedish chemist Svante Arrhenius. He actually took the term from Athanasius Kircher who wrote of *panspermia rerum*, ‘the universal seed of things’. In turn, he had developed the concept from Bruno’s *spermia rerum*, meaning the basic unit of which everything is made – essentially atoms.<sup>5</sup>

Panspermia’s most (in)famous recent champions were Sir Fred Hoyle and his long-time collaborator Chandra Wickramasinghe. With a typically robust side-swipe at his peers, Hoyle likened their view that life originated exclusively on Earth to the geocentric ideas that prevailed before

Copernicus.<sup>6</sup> In a way he was right, their ideas effectively make our planet the *biological* centre of the universe.

And the increasingly exciting discoveries of the comparatively new field of astrobiology – developed in the late 1950s – reveal that there is no doubt whatsoever that many of the building blocks of life do have an extraterrestrial origin. The only real controversy is how far they were assembled before they arrived on Earth.

Certainly the chemical ingredients for life exist in space. Even the most remote regions of interstellar space are pervaded with gas and a much, much smaller amount of solid material in the form of extremely fine-grained ‘dust’. These cosmic grains are enormously significant. Until the beginning of the 1960s the consensus was that they were simply frozen clumps of gas molecules, but improved technology has revealed that

some were too close to stars to be frozen. So what could they be?

Enter the ever-energetic Hoyle and his newly arrived research student from Sri Lanka, Chandra Wickramasinghe. Their time working at Newton's alma mater Trinity College, Cambridge marked the beginning of one of the most enduring scientific collaborations, one that continued after Wickramasinghe's own glittering scientific career took off and only ended with Hoyle's death in 2001.

It was Wickramasinghe who developed the idea that organic carbon-based chemicals form the major components of cosmic dust. When he and Hoyle first proposed this in 1962 it was, unsurprisingly, highly controversial. But research in the 1960s and 1970s vindicated it, and these days it is simply a given.

Formaldehyde, one of the simplest organic compounds, was detected in interstellar clouds in 1969, and since then a whole host of organic chemicals has been added to the list. By the end of the next decade over thirty complex molecules had been found in interstellar dust, including water vapour, carbon monoxide and ammonia. Organic molecules including methane, acids, alcohols and sugars have now been found. Even molecules of vinegar have been detected in a gas cloud in Sagittarius. Around 20 per cent of interstellar dust is now thought to be made up of organic chemicals. The discovery of so many prompted Hoyle and Wickramasinghe to propose, in the mid-1970s, that even more complex organic molecules could be lurking in the interstellar clouds, and that this was a better candidate for the origin of life than the terrestrial 'primordial soup'.

One of the most significant discoveries in this field came in 2005 from a NASA team

from the Ames Research Center in California, using data from the Spitzer Space Telescope. The team was studying a type of complex organic molecule with the uncatchy name of polycyclic aromatic hydrocarbons (PAHs), a very common family of chemicals which, in the words of the team's leader Douglas Hudgins, are found 'in every nook and cranny' of the universe. The fact that PAHs are abundant in space had been known for a long time, and few thought they were worth much of a second look. But the NASA team discovered to their great astonishment that the PAHs they were looking at – in a distant galaxy designated M81, 12 million light years away – were rich in nitrogen. This is considerably more significant than it might appear.

Without nitrogen PAHs tend to be hostile to the biochemistry of life. On Earth they are largely the result of the breakdown of organic material, for example the burning of fossil



fuels, making them pollutants and in some cases carcinogenic. But with nitrogen it's a different story. Without nitrogen-containing PAHs, amino acids, DNA and RNA, as well as a host of other vital molecules (for example haemoglobin, chlorophyll – and even essentials such as chocolate) could not exist. Indeed, one of the theories of how life originated on Earth puts nitrogen-rich PAHs right at the centre. But the big question is how they developed in the first place.

The discovery that nitrogen-bearing PAHs are present in space provides a major piece of the puzzle. The current understanding, based on the NASA Ames team's work, is that they are formed and ejected into space by the death of stars. As Douglas Hudgins puts it:

There was a time that the assumption was that the origin of life, everything from building simple compounds up to complex life had to happen here on Earth ...

This stuff contains the building blocks of life, and now we can say they're abundant in space.

7

Hudgins points out that discovering nitrogen-containing PAHs in interstellar space does not prove that life on Earth came from the stars but that as it is the simplest theory, according to Occam's Razor, this is the one that science should prefer.

# ON THE TAIL OF COMETS

Another way that building blocks can be seeded on planets is via comets. Not through scoring a direct hit on Earth – which would incinerate any ‘seeds’ – but by drifting down with the ‘rain’ that floats into the atmosphere as the planet passes through the debris from the tail of comets.

Most comets are believed to be left over material from the gas and dust clouds that coalesced at the birth of the solar system, now roaming its highways and byways under the influence of the gravity of the heavenly bodies, generally orbiting the sun. The endless process of heating and freezing as the comet approaches and recedes from the sun causes reactions in its basic chemicals, which creates new compounds.

The idea that seeding occurs via comets has received strong support from the study of meteorites, especially fragments from the famous specimen believed to be from the nucleus of a comet that exploded over the small Australian town of Murchison, Victoria, in 1969. Analysis of the Murchison meteorite has continued ever since – the latest batch of test results, after examination with cutting-edge techniques, was released in February 2010. One of the first things to be discovered was that it was made up of organic carbon chemicals – it even smelt of petrol. It contains 70 different amino acids, including two, glycine and alanine, which are fundamental to life on earth – the very same, in fact, as those that emerged from the Urey-Miller experiments with the primordial soup that so excited scientists back in 1953.

There is an even more direct connection between comets and glycine, which is chemically the simplest amino acid. In 1999 NASA

launched the probe Stardust to collect material from the comet Wild-2, which orbits the sun once every six years. In January 2004 Stardust swept up dust from the comet's coma – the cloudy halo around the nucleus – returning it to Earth in a sealed container nearly two years later. Analysis revealed the presence of glycine. As Dr Carl Pilcher, head of NASA's Astrobiology Institute announced:

The discovery of glycine in a comet supports the idea that the fundamental building blocks of life are prevalent in space, and strengthens the argument that life in the universe may be

common rather than rare. [8](#)

# LIFE IN THE LAB

Some of the greatest revelations have come from attempts to reproduce interstellar conditions in the laboratory, in what is effectively a cosmic version of the Urey-Miller experiments. At the forefront of this quest is the NASA Ames Research Center in the 1990s with a team led by Louis Allamandola.

Allamandola set out to study how dust grains in gas clouds interact with the gases by replicating the conditions. He and his team placed methane, water vapour, ammonia, carbon monoxide and so on in extremely thin densities and very, very cold temperatures and bathed them in ultraviolet radiation. Under these conditions, chemical reactions occur that would be impossible under normal earthly conditions. The radiation breaks apart the molecules, and the icy cold

puts them back together in very unusual and complex ways. But although many of these structures had never been seen before, some were eerily familiar to biochemists ...

The first surprises related to the PAHs. Interstellar conditions, particularly exposure to ultraviolet, transform the PAHs' carbon into useful forms for life such as alkaloids – 'ubiquitous in the plant world'<sup>9</sup> – and quinones, essential for photosynthesis and the functioning of muscle and brain cells. These vital substances simply wouldn't exist without the gas and dust clouds in deep space. But there were even more ground breaking discoveries.

Back in 1985, American biologist David Deamer had discovered something very odd in the Murchison meteorite: small 'bubbles' closely resembling biological structures known as vesicles – basically membrane sacs containing liquid biochemicals that constitute part of cells. But were they really vesicles? It was only in the late 1990s that

Deamer realized the potential of Allamandola's work: Could it be that similar bubbles had appeared in his simulated interstellar environment? Indeed they had. They found identical vesicles, about the size of red blood cells. They called in biochemist Jason Dworkin – a former collaborator with Stanley Miller – who identified them as lipids, a class of macromolecule that includes fats and waxes.

Lipids perform several vital functions, but most tantalizingly they make up the membranes of cell walls, which may be small but are in fact big operators. They separate biochemicals into packets – capsules, basically. Without them many of the processes and reactions vital to the development of life could never happen because the biochemicals would be too dilute. As geneticist Pascale Ehrenfreund, specifically commenting on the Ames discovery, pointed out, 'membrane



formation is a crucial step to the first forms of life.’<sup>[10](#)</sup>

Researchers trying to replicate the origins of life had never previously had any success in replicating lipids under terrestrial conditions. But here, in interstellar gas clouds, they appeared utterly spontaneously – in fact, the Ames team never noticed they had made lipids until David Deamer asked them to look. The similarity with the vesicles found in the Murchison meteorite shows that this process really does happen in space. It isn’t just the Frankenstein child of jiggery pokery in a lab.

In other experiments, Allamandola’s team demonstrated that not merely cell membranes, but some of their internal biogenic chemicals – ammonia, formaldehyde and even amino acids – can also be made in the interstellar clouds. Allamandola speculates that the first cells could have come from inside comets – all the ingredients are there,

and so is the membrane to neatly wrap them up – although he admits that this theory is untestable.<sup>[11](#)</sup> At least at the moment.

In fact, Allamanodola wasn't the first to make such a suggestion. With so many of life's building blocks being found in space, Fred Hoyle and Chandra Wickramasinghe had suggested back in the 1970s that they could be developed into what they termed 'protocells' that could then be seeded onto planets by comets. The Ames discovery shows this is almost certainly correct.

To us the important thing, though, was the implications of creating lipids in the lab. As Louis Allamandola pointed out:

The most amazing thing is that we start with something really simple. And then suddenly we're making this enormous range of complex molecules. When I see this kind of complexity forming under these extreme conditions, I begin to really believe that life is a cosmic imper-

ative.<sup>[12](#)</sup>

# THE LIVING, BREATHING EARTH

The universe – not just planets, but space itself – is bursting with the potential and materials for life, created and transported around like ocean currents carrying seeds between remote islands. However, it is still only on planets that these can develop into something more complex than bacteria at best, which brings us to another even more controversial idea that fits very neatly into the ‘universe designed for life’ vision.

The Gaia hypothesis was proposed in the 1970s by British scientist James Lovelock, who is even more of a maverick or independent thinker, depending on your point of view, than Sir Fred. Similarly, Lovelock’s brilliance is acknowledged even by his critics (even if

they believe his imagination sometimes gets the better of him), as are his very real contributions to science.

Lovelock describes himself as an ‘independent scientist’, neatly encompassing both his attitude to the freedom of thought he believes is essential for a scientist and his avoidance throughout his long career of being tempted by commercial or even academic institutions – although he has occasionally been successfully headhunted as a consultant. With his broad knowledge of all the sciences, and contempt for the increasing specialization that blinkers scientific thinking, he would have been at home in the Renaissance.

Lovelock is that rare being, someone whose brilliance has actually changed the face of the world. Most significantly, this occurred in the early 1970s through his discovery that human-made chlorofluorocarbons (CFCs) had penetrated the environment to

such a degree they were present in places as remote from human industry as Antarctica. It was because of Lovelock that today's world is CFC-free.

The concept behind Gaia was a spin-off of Lovelock's work for NASA in the 1960s, when he was devising ways to detect life on Mars. He reckoned analysis of the Martian atmosphere might reveal the characteristic changes caused by the presence of living organisms. Looking further into this question and the impact life has on the Earth's atmosphere led him to certain striking observations. It isn't just that the presence of plants and animals – the biosphere – changes the atmosphere, but they appear to be regulating it, actively keeping the Earth habitable. Life itself keeps the planet in a condition suitable for life.

From such phenomena Lovelock developed the idea that the Earth is a 'self-regulating entity', where living things are not

passive guests but ensemble players with integral parts in shaping conditions on the planet.

A prime example of this self-regulation, besides a host of others, relates to the Earth's response to changes in the sun's output. Living organisms can only survive within a narrow range of temperatures, about 10 to 20 degrees centigrade. However, although astrophysicists agree that since life first appeared on Earth at least 3.5 billion years ago the sun's heat has increased by about 30 per cent, the Earth has obviously remained at a temperature suitable for life. Somehow the increasing heat has been balanced to keep the average global temperature steady.

As a 2 per cent drop in the heat reaching Earth from the sun is enough to trigger an ice age, imagine what the Earth would be like with 30 per cent less heat. When life originated, something – probably a high level of greenhouse gases in the atmosphere – made

the Earth significantly warmer than it would have been otherwise. But as the sun grew hotter, some other factor must have altered conditions – the mix of gases in the atmosphere, for example – as compensation. And that unknown factor had to keep step with the steady increase in solar heat.

As Lovelock pointed out, any of the processes that have been proposed to explain this compensation would have had to be staggeringly precise. Even small variations in, say, the mix of atmospheric gases would result in runaway reactions that would either seriously overheat the Earth (the oceans would literally boil away), or reduce it to a frozen ball. Yet that clearly didn't happen; the process seems somehow to have been controlled.

... the Earth's living matter, air, oceans, and land surface form a complex system which can be seen as a single organism and which has the capacity to keep our planet a fit place for

life.<sup>13</sup>

Following the suggestion of his neighbour, the novelist William Golding, Lovelock called this the 'Gaia hypothesis', after the ancient Greek Earth goddess. In 1979 he produced *Gaia: A New Look at Life on Earth*.

When *Gaia* was first published there were howls of outrage from the scientific world, led predictably by Richard Dawkins. (Lovelock declared that he 'hated Gaia as much as he hates God'.)<sup>14</sup> Dawkins condemned Lovelock's system because to him it could never have evolved by natural selection, while Lovelock maintains that it fits natural selection perfectly. However, as a 2010 BBC documentary on Lovelock's work showed, much of the thinking behind the once-controversial Gaia hypothesis has now become mainstream, while some still regard Lovelock's idea as oddball and over-imaginative, others, including the philosopher John Gray, consider the idea as revolutionary as Charles Darwin's.<sup>15</sup>



Despite widespread belief to the contrary, what the Gaia hypothesis does *not* proclaim is that the world is alive in the same way that an animal is alive, or that it is somehow self-aware, with some higher planetary consciousness controlling and ordering the individual parts to benefit the whole. In fact, Lovelock is scathing about the New Age, which took (or most probably, hugged) his book to its heart, believing in some way that it was scientific proof of the reality of the Mother Goddess. To Lovelock such concepts exist outside the realm of science, since they can't be tested by scientific methods.<sup>16</sup> Lovelock uses the term 'alive' metaphorically, taking pains to explain - with superbchutzpah, but pinpoint precision - 'the planet is alive in the same way that a gene is selfish'.<sup>17</sup> To him the Earth fits the definition of a 'super-organism': 'bounded systems made up partly from living organisms and partly from non-living structural material'<sup>18</sup> - as, for

example, a beehive. Literal interpretations of the word 'alive' also seems to be behind Dawkins' hostility, which implies a certain lack of sophistication in his understanding or perhaps an unwillingness to confront the theory properly.

The Gaia theory is what one would expect, indeed predict, from the designer universe hypothesis. If the universe is fine-tuned to support life, and life is a cosmic imperative arising wherever conditions are conducive, then one would expect that once complex life did take hold on a planet, some kind of mechanism would be in place to ensure its survival. We should recall here the concept of the *anima mundi*, the 'world soul', which animates and also controls the world so dear to the Hermeticists' hearts.

But however exciting the Gaia hypothesis might seem, we should remember it has yet to be conclusively proven. Nor does it prove the designer universe theory correct. But, as

with the existence of biochemical evidence in support of the cosmic imperative, it nevertheless fits and supports the concept of a designer universe.

# THE IMPERATIVE

# COSMIC

It no longer seems a question of whether panspermia happens – it does, quite clearly – but rather of how far the building blocks of life can be fused in space before they need a planet to really get going. Christian de Duve sums up the current state of our knowledge:

There is ... ample evidence that a number of biogenic compounds can form spontaneously under primitive Earth conditions, in interstellar space, and on comets and meteorites. Most likely, such compounds provided the first seeds of life. How much was made locally, how

much was brought in from outer space, is still widely debated. [19](#)

The latest scientific thinking about the origin of life in the universe is very compatible with the concept of a designer universe. Rather than life being a billion-to-one fluke, it seems to be a common – even a *universal* – phenomenon. And the different parts of the

universe play vital roles in the creation and dissemination of life.

We must be careful, however, not to put words into the mouths of the likes of Louis Allamandola and Christian de Duve. When they use the expression 'life is a cosmic imperative', they are saying that conditions in the universe mean that wherever life can evolve, it inevitably will. This is emphatically not the same as saying that the 'purpose' of the universe is to produce living organisms. Scientific objectivity and a strict adherence to current evidence could never allow them to draw such a conclusion. But if the universe is designed for life, would we be able to tell the difference between that more Hermetic kind of cosmic imperative and de Duve and Allamandola's version?

It is unlikely. If the universe is fine-tuned to produce the chemical elements and right physical conditions for intelligent life, then that same delicate balancing act would have

to also include the imperative that biochemistry is now beginning to recognize. It would be pretty pointless otherwise.

Unlike many in the biological sciences, de Duve does give houseroom to the more metaphysical interpretations of the cosmic imperative. In *Vital Dust* he discusses the ideas of Pierre Teilhard de Chardin, the French Jesuit priest and palaeontologist who put forward a theory of cosmic evolution very similar to the designer universe, albeit with a Christian gloss. To Teilhard, creation evolves from simple matter, to life and on to consciousness as part of a divine plan, which de Duve considers a valid possibility.<sup>[20](#)</sup>

As biochemistry has become increasingly sophisticated, it has found nothing to contradict the idea of intelligent design. Quite the reverse. However – and to many this will be a very big caveat indeed – the evidence for life as a cosmic imperative is, like that for the fine-tuning of the big bang, hard to square

with the image of the biblical God. This is far too limiting for that kind of personal entity, with his alleged omniscience but all-too-human emotions.

An alternative to scientific atheism, which also fits this evidence, is the Hermetic interpretation, in which the cosmos was specifically built for life. Some of de Duve's statements even read like an expression of Hermetic cosmology – a belief in the living universe – albeit in biochemical terms:

The universe is not the inert cosmos of the physicists, with a little life added for good measure. The universe *is* life, with the necessary infrastructure around; it consists foremost of trillions of biospheres generated and sustained by the rest of the universe ...

The entire cloud of vital dust forms a huge cosmic laboratory in which life has been experimenting for billions of years. <sup>21</sup>

To de Duve most of the universe exists simply to provide the scaffolding to support life. In his view, the universe is effectively a super-organism in much the same way that, according to James Lovelock, the Earth is. Just on an unimaginably vaster scale.

But what about evolution? Surely the current understanding points in the opposite direction to ideas about life as a universal imperative, or inevitability, good as they may sound. The development of life, especially into anything more complex than a bacterium, is, we are told purely down to chance. If the evolution of life is dependent on random factors, then the idea of design in the universe as a whole is instantly and completely undermined.

Evolution is so often presented to the general public by the Dawkins school as the final coup de grâce, not merely to religious creationism, but also to any idea of design behind the universe, that it may seem perverse even to begin to challenge it. But what happens if you dare do just that? The results are rather surprising, although they won't turn you into a creationist. In fact, quite the reverse. As we are about to see, the theory of



evolution so beloved of Dawkins et al., by no means proves atheism to be right.

## Chapter Ten

- [1](#) Quoted in Dawkins, *The Greatest Show on Earth*, p. 417.
- [2](#) *Ibid.*, p. 416.
- [3](#) De Duve, *Vital Dust*, p. xv.
- [4](#) Watson and Crick, p. 738.
- [5](#) See Ingrid D. Rowland, ‘Athanasius Kircher, Giordano Bruno, and the *Panspermia* of the Infinite Universe’, in Findlen (ed.).
- [6](#) Hoyle and Wickramasinghe, *Evolution from Space*, p. xiii–xv.
- [7](#) Quoted in Carey.
- [8](#) Quoted on BBC News website, “‘Life Chemical’ Detected in Comet’, 18 August 2009: [news.bbc.co.uk/1/hi/sci/tech/8208307.stm](http://news.bbc.co.uk/1/hi/sci/tech/8208307.stm).
- [9](#) Schueller, p. 34.

[10](#) Quoted in *ibid.*, p. 31.

[11](#) *Ibid.*, p. 34.

[12](#) Quoted in *ibid.*, p. 35

[13](#) Lovelock, *Gaia* (1979 edition), p. vii.

[14](#) In the documentary, 'Life, the Universe and *Everything*: James Lovelock' in the *Beautiful Minds* series, produced and directed by Paul Bernays, ARC Productions for BBC Four, 2010.

[15](#) Interviewed in the above documentary.

[16](#) Lovelock, *Gaia* (2000 edition), p. xv.

[17](#) *Ibid.*, p. ix.

[18](#) Lovelock, *The Ages of Gaia*, p. 15.

[19](#) De Duve, *Vital Dust*, p. 20.

[20](#) *Ibid*, pp. 286–9.

[21](#) *Ibid.*, pp. 292–3.

## CHAPTER ELEVEN

# DARWIN'S CLOTHES

# NEW

Today's only accepted and acceptable scientific theories about the origins and development of living things reject even the slightest degree of design. Instead, the whole process that has fashioned the dazzling display of animals, plants and micro-organisms that cover the Earth is, we are told, driven ultimately by blind chance.

Evolution has become the really big battleground for the righteous – or perhaps, more accurately, the self-righteous – in the conflict between science and religion, particularly between militant atheists and Christian fundamentalists.

For those who take Genesis literally, evolutionary theory has not only to be rejected but also actively anathematized. The first book of the Bible states that God made all plants, sea creatures, birds and land animals (in that order) ‘according to their kinds’ – as individual, and by implication, fixed species. If, as science now understands it, different species developed one from another, then the biblical account is basically just wrong. Even worse to the Genesis literalists is the notion that humans – to whose creation God is supposed to have devoted special care and attention, making us ‘in his own image’, no less – are part of that scheme, that we have evolved from lower animals.

But scientists have made evolution a battleground too, seeing it as their greatest victory over the forces of superstition and irrationality, and raising the fear that undermining it will see the end of their intellectual triumph. In the last couple of decades there

have been good reasons for scientists to be anxious, as the recent political controversy in the USA over intelligent design (ID) has shown. The well-organized and generously-funded ID movement aims to undermine evolutionary theory by picking on its flaws, but it does so as part of a Christian fundamentalist – creationist – agenda. So, if biologists admit that the theory is anything less than cast-iron, their opponents will pounce and, particularly in America, use such admissions for political ends, their immediate objective being control of the education system.

The ID movement emerged as the result of a series of reversals that Christian fundamentalists have suffered since the 1980s, in which attempts to have creationism taught compulsorily in state school science classes were successfully challenged in the Supreme Court. These were ruled unconstitutional because the United States' constitution – its

First Amendment, which dates back to 1791 – explicitly separates Church and State.

Creationists then began to recast their argument in more scientific-sounding terms, basically crossing out ‘God’ and ‘creation’ and replacing them with ‘designer’ and ‘intelligent design’. The phrase ‘intelligent design’ was carefully chosen, as it has occasionally cropped up in the scientific literature over the years. Charles Darwin himself used it.

The ID movement’s strategy is to highlight apparent gaps in Darwinian theory and biological phenomena that are either hard to explain in Darwinian terms or which seem to actively contradict it. It goes for the weak spots and then offer, intelligent design as an alternative. Of course, it is possible to believe in intelligent design without being a Christian fundamentalist; it’s just that virtually all ID-ers are.

But – and this is an important point – many of the ID movement's claims about Darwinism's weaknesses aren't its own, but are lifted from the works of bona fide scientists. The notion that some creative, guiding and purposeful factor influences biological evolution has been proposed by dispassionate and objective thinkers with no religious axe to grind. Indeed, that great proponent of the 'intelligent universe', Sir Fred Hoyle, could have given Richard Dawkins a run for his money in the anti-organized religion stakes. The ID movement is cynically twisting such challenges to serve its own agenda.

Given such resolute opponents, small wonder that the scientific community sees any attempt to challenge Darwinian orthodoxy as dangerous and religiously motivated. Anyone who argues against it is assumed to be hiding a creationist agenda. This makes the whole subject a minefield for those who



fully intend to get to the bottom of the subject, no matter where it might lead.

Of course there will be many who disapprove of non-specialists investigating the subject in the first place. But often those who devote decades to one aspect of a complex discipline end up simply not being able to see the wood for the trees. We, on the other hand, can stand back and see the wider picture. One way of doing so – especially where academic sacred cows like evolution are concerned – is to revert to childhood. One specific, fictional childhood in particular will provide some much-needed perspective. We are assuming the role of the little boy in Hans Christian Andersen's fable, the lone critic of the Emperor's 'new clothes'. In this tale, everyone agreed they were magnificent – except for the young outsider who saw that they were, in fact, completely non-existent. Following his lead, we also find ourselves standing towards the back of the crowd,

ignoring the cheering to see what is really there.

# CHANCE WOULD BE A FINE THING

Famously, the cornerstone of evolutionary theory is natural selection, or survival of the fittest, as proposed by Charles Darwin (1809–82), most prominently in his *On the Origin of Species by Means of Natural Selection* in 1859, which has since become the bible of modern biology. Actually ‘survival of the fittest’ was coined by the philosopher Herbert Spencer as a way of avoiding the implication of design in the phrase ‘natural selection’. Even back then people were cautious about giving ammunition to creationists.

In Richard Dawkins’ hands, natural selection has been moulded into a quasi-religious revelation. To him natural selection also

achieves a very rare thing: it proves a negative by showing that God does not exist. Natural selection provided Dawkins with his atheist epiphany, as well as being the catalyst that ‘raised his consciousness’,<sup>1</sup> to use one of his favourite phrases.

To Darwin’s natural selection, modern biology has added genetics, the mechanism of heredity first proposed by – another irony – a Catholic monk, Gregor Mendel, in 1865 (although the term ‘gene’ was only coined in the first decade of the twentieth century), and since the discoveries of Francis Crick and James D. Watson in the 1950s known to operate through DNA. ‘Neo-Darwinian theory’ or ‘Neo-Darwinian synthesis’ is basically natural selection plus genetics.

The basic principles of natural selection are familiar and straightforward enough. If a new trait appears in an individual animal or plant that gives it an edge in the survival game – helping it be more efficient at finding

food, dodging predators or attracting a mate – it will out-perform the rest of its species. It will live longer and produce more offspring that, inheriting the new trait, will also be one step ahead in the survival stakes. Eventually, after many generations, only those with the new feature will remain, the species having evolved into something new and better. Many more generations later, it will have become a new species entirely, unable to breed with members of the ‘parent’ species. Conversely any new traits that hamper an organism’s ability to survive or reproduce will be self-evidently eliminated.

As to what causes the changes on which natural selection works, it is all down to changes in DNA. When this miraculous molecule replicates during cell division, it nearly always reproduces itself perfectly. Extremely rarely a change is introduced, and when this happens it changes something in the

organism's physical form or in one of the biochemical processes that sustains it.

Changes in even a single gene can have the most profound effect. One mutation, for example, results in a mammal's hind legs remaining vestigial within the body. Although an animal with such a handicap wouldn't last very long, there are rare situations in which the mutation can actually be useful: it would help streamline semi-aquatic mammals, for example. In fact, fully aquatic whales and dolphins have been shown to have exactly that mutation.

Natural selection is not the driving force of evolution; genetic mutation is. Natural selection is more of a steering force, either gifting a change to the rest of a species or simply eliminating it. But what causes the mutations? According to the consensus, they arise from random and unpredictable copying errors that occur during replication. So, although the genetic system is beautifully

elegant, life in all its myriad forms owes its existence to the imperfections in this system.

The process of random genetic mutation and natural selection, we are told, accounts for all of the enormous diversity of life on Earth. Everything that lives – microbial, animal or vegetable – has evolved over the course of billions of years from a single original organism, the ‘cenancestor’. (Otherwise known as the more zappy LUCA, ‘Last Universal Common Ancestor’, a term presumably chosen because the more apt ‘First Universal Common Ancestor’ would result in a somewhat inappropriate acronym.)

As Francis Crick wrote (his emphasis), *‘Chance is the only true source of novelty’*.<sup>2</sup> Similarly, in the 1960s the Nobel-prize winning biologist Jacques Monod uncompromisingly put man in his place. With typically French existential angst he wrote:

The ancient covenant is in pieces; man at last knows that he is alone in the unfeeling immensity of the universe, out of which he emerged only by chance. Neither his destiny nor his

Put so depressingly, it's hard to see much of a choice.

But does chance alone really explain everything in the natural world? Copying errors do happen – as is proven by genetic disorders – but if every individual tweak to the genetic code is random, can they alone explain all the vast number of changes needed to transform LUCA into human beings, *E. coli*, broccoli, whales and duck-billed platypuses?

Introducing errors into any system isn't usually a clever idea. Fred Hoyle and Chandra Wickramasinghe astutely observed that ascribing all the variety of the animal, vegetable and microbial worlds to random mutations is like saying a computer program can be improved by introducing random



mistakes.<sup>4</sup> And Paul Davies writes in *The Cosmic Blueprint* (1988) that logically:

one would suppose that random mutations in biology would tend to downgrade, rather than enhance, the complex and intricate adaptedness of an organism. This is indeed the case, as

direct experiment has shown: most mutations are harmful.<sup>5</sup>

The standard response to this is that the vast majority of DNA mutations are indeed harmful, but natural selection weeds them out by killing off the afflicted animal or plant. The number of mutations that just happen to be beneficial might be minuscule, but they are enough, we're told confidently, to account for everything that ever evolved. But this is by no means solid fact: it is actually just an assumption.

The problem for evolutionary scientists is that the factors involved are impossible to quantify. Mutations during DNA replication are extremely rare. According to John Maynard Smith, one of the late twentieth century's foremost geneticists, each time

DNA replicates, the chance of a change in a base pair is one in a thousand million.<sup>6</sup> Most mutations have no effect anyway because the genetic system has a clever error-correcting mechanism. And the vast majority of mutations that do have an effect on the individual organism make no difference in evolutionary terms. The only changes passed on to the next generation are those which happen in the 'germ line' cells – sperm and eggs and the cells from which they develop. Only a minute percentage of *those* produce a beneficial change in the organism; most do damage. It is impossible to put precise figures on any of this.

The other side of the equation involves the speed of evolution, or how long it takes one particular species to evolve from another, which entails identifying the genetic changes responsible. As evolutionists can rarely, if ever, determine either of these with anything approaching certainty, there is ultimately no

way they can prove that chance and chance alone was responsible.

Evolution is dependent on so many factors – the appearance of ‘good’ mutations, the size of a species’ population, competition from other animals, its environment and the speed of environmental changes to which it has to adapt or die. The origin of each species, every branch in the evolutionary tree, is a special, unique case, as Francis Crick asserts:

Strictly speaking, we can form no firmer estimate about the time needed for evolution than we can for the chance of any particular step ... There is no detailed theory of evolution so quantitative that we can calculate just how long any particular stage is likely to require.<sup>7</sup>

Ever since Darwin, the physical changes on which natural selection works have been *assumed* to be purely random. The reason is obvious: if these changes aren’t the result of chance alone, then some other factor or factors are responsible, and there is no

conceivable way to account for such factors without invoking the supernatural.

In order to make this assumption work, evolutionary theory relies on an egregiously circular argument, which basically goes as follows: No matter how unlikely it seems that a particular characteristic should evolve through random mutations, it must have done, because it now exists – and only random mutations can make things evolve. Frankly, this is outrageous. If non-Darwinists used similar (non) logic, we would be hammered – and quite right, too.

To be fair to evolutionary biologists, their inability to prove the quintessential importance of chance does not necessarily mean the theory is wrong. There are, however, many events in evolutionary history that are not merely difficult, but impossible to explain in neo-Darwinian terms. In fact, astoundingly, *most* of the major steps in the advancement of life, from the primeval to the complex, fall

into this category. Even mainstream biology acknowledges that processes outside the normal neo-Darwinian mechanism are required for these steps, or else pronounces itself completely baffled.

# THE GREAT DNA MYSTERY

The first big mystery is how DNA itself came into being. After all, the entire variation of life on earth is essentially the result of the shuffling and reshuffling of its basic code. As one researcher put it recently, DNA 'has multiplied itself into an incalculable number of species, while remaining exactly the same'.<sup>8</sup>

There is a fundamental Catch-22 situation about the origin of life. In order to replicate, DNA requires certain proteins in the form of enzymes to act as a catalyst, but no protein can be produced without DNA in the first place. At present, there are only theories that seek to explain how this came about, which because of their very nature are untestable.

In the mid-1980s a suggestion by British molecular biologist Graham Cairns-Smith that the earliest 'genes' evolved from clays attracted considerable interest. A current favourite is the 'RNA world' theory, which proposes that in the early stages, when only primitive single-celled organisms existed, life was based on RNA rather than DNA. We also mentioned earlier the PAH world hypothesis, according to which polycyclic aromatic hydrocarbons once predominated, leading to the development of RNA. However, although it makes sense that PAHs came first, were followed by RNA and then DNA, this theory is also rather vague.

All of these hypotheses, naturally, assume that the process of development from ordinary chemicals to the fully-fledged genetic system was entirely due to blind chemical reactions and chance. But that's just an assumption, and it gets worse: there are only the vaguest ideas about exactly how this

happened. As Christian de Duve comments in *Life Evolving* (2002):

... we are mostly left with speculative hypotheses to explain the manner in which the basic building blocks provided by cosmic chemistry might have combined into larger molecules, such as proteins and, especially, nucleic acids, not counting the more complex assemblages from which the first biological structures arose. One may well wonder, therefore, whether we will ever succeed in explaining the origin of life naturally or, even, whether this phenomenon

is naturally explainable. <sup>9</sup>

Life, and therefore DNA, appears to have been here almost as soon as the planet had reached the right conditions. There seems suspiciously little time for it to have evolved through random events.

And there is a further twist: DNA seems to have evolved twice. Until the 1970s it was thought that all life could be divided into two ‘domains’, depending on their type of cell. These were bacteria and the more complex ‘eukaryotes’ – everything that isn’t bacteria, including all the really complex stuff such as animals and plants. Basically the eukaryotic cell has a nucleus, whereas the bacterial cell doesn’t.



Then in 1977 American microbiologist Carl Woese made an apple-cart-upsetting discovery at the University of Illinois. It turned out that some 'bacteria' were actually something else entirely. Although these organisms were, like bacteria, single-cell microbes without nuclei, they are as genetically distinct from bacteria as bacteria are from eukaryotes. Woese named this new, third type of organism archaea, from the Greek meaning 'beginning' or 'primeval'.

Unexpectedly, molecular biologists discovered that bacteria use different enzymes to replicate their DNA from those used by eukaryotes and archaea – revealing that there are two entirely different systems of DNA replication.<sup>10</sup> Since DNA controls its own replication, this means there are two quite separate and independent types of DNA. Basically, as geneticist Anthony Poole of Stockholm University noted: 'What it really looks like is that DNA has evolved

twice.’<sup>11</sup> There was therefore not one but two LUCAs, one the ancestor of bacteria, and the other of everything else. Assuming it is all due to chance, something with extraordinarily long odds actually happened twice – both times very early in the Earth’s history – and never happened again.

Some scientists, such as Carl Woese, now acknowledge that it is impossible to explain the evolution of the genetic code in purely Darwinian terms, and are exploring alternative mechanisms for the origin of DNA.<sup>12</sup>

So nobody knows. Not even a little bit. All the ideas put forward are still too clunky to count. Leading palaeontologist Simon Conway Morris laments that scientists’ inability to discover the origin of life is ‘one of the great scientific failures of the last fifty years’.<sup>13</sup> Even Dawkins stays out of the mix, but only, he is keen to point out, because the search for the origin of life, being a question

of chemistry, is outside his field of expertise.<sup>14</sup> It's frustrating and sobering to realize that although we know what must have happened for life to get started, we haven't the faintest idea how. It certainly suggests that evolutionists who declare dogmatically that the origin of life owes nothing to non-random factors are vaingloriously jumping the gun. They just can't be sure.

# THE BIG ANAL BREAKTHROUGH

Although DNA is the prerequisite for life, there are other key milestones in the journey from single-celled microbes to today's complex life forms. And without these, no further progress up the evolutionary tree could ever have been possible.

Many of these landmark events are obvious, such as the development of vertebrae, but some are more unexpected, including the appearance of the anus, sometimes called somewhat eye-wateringly the 'anal breakthrough', which apparently occurred some 550 million years ago. Without an anus, mouths couldn't evolve – or if they did without benefit of a rectum, animals would explode after a couple of meals – and

without mouths heads couldn't evolve, and without heads we couldn't have sizeable brains. This prompted one of our favourite quotes in evolutionary literature, from Oxford zoologist Thomas Cavalier-Smith: 'The anus was a prerequisite for intelligence.'<sup>15</sup> (Given the pronouncements of certain dogmatists, we always suspected as much.)

Another of life's most vital developments was the appearance, some two billion years ago, of a revolutionary new type of cell, the complex and large eukaryotic cell that we mentioned above. Before its appearance there was only the more primitive bacterial, 'prokaryotic' cell.<sup>16</sup> The crucial difference between the two is that the eukaryotic cell has a DNA-filled nucleus, whereas in the prokaryotic the DNA is diffused throughout the cell. Eukaryotic cells have up to one thousand times more DNA. And the nucleus arrangement means that only eukaryotic cells can develop into large, more complex

organisms – all animals and plants are eukaryotes. Without this type of cell, the Earth would still be populated exclusively by microbes. Microbes without anuses, that is.

Logically the eukaryotic cell must have evolved from the simpler prokaryotic. As Cavalier-Smith notes, this process ‘involved the most radical changes in cell structure and division mechanism in the history of life’.<sup>17</sup> He adds that the leap required ‘dramatically accelerated evolutionary rates for many genes and, more importantly, massive novel gene creation’.<sup>18</sup> But as leading cell biologist Lynn Margulis, writing with her son Dorion Sagan, acknowledges:

The biological transition between bacteria and nucleated cell, that is between prokaryotes and eukaryotes, is so sudden it cannot effectively be explained by gradual changes over time. The division between bacteria and the new cells is, in fact, the most dramatic in all bio-

logy.<sup>19</sup>

They go on to explain:

All cells either have a nucleus or do not. No intermediates exist. The abruptness of their appearance in the fossil record, the total discontinuity between living forms with and without nuclei, and the puzzling complexity of internal self-reproducing organelles suggest that the

In other words, this vital leap simply *cannot* be explained by the usual neo-Darwinian chance mutation and natural selection. There has to be a completely different process involved.

Lynn Margulis' ground breaking solution to the conundrum, proposed in the mid-1960s and which revolutionized the understanding of cells (after the usual years of disparagement and dismissal from her peers), was that it was an act of symbiosis: some kinds of prokaryotic cells entered others, feeding off their waste products and leaving their own detritus as food.

But even this only explains *what* happened. We are no nearer to knowing how or why. Evolutionary scientists fully accept that such a transition required a special,

unique – and resolutely non neo-Darwinian – set of processes, without which multi-celled life could never have existed.



# THE IMPOSSIBILITY OF SEX

Another milestone after eukaryotes was the development of sexual reproduction, without which no complex life would be possible – and something else about which evolutionary biologists tie themselves in knots. Metaphorically, at least.

The simplest micro-organisms reproduce asexually, by splitting into two, each half containing the same DNA. From the first appearance of life around 3.5 to 4 billion years ago until, according to the evidence of microfossils, between a billion and a billion and a half years ago, that was the only kind of reproduction there was. As each new cell is essentially a clone of its ‘parent’ – they are genetically identical, the DNA being passed on

unchanged – it doesn't allow for much genetic diversity, making evolution very slow, which is why not much happened for some three billion years.

Sex is by far the better option for the evolution of more complex and intelligent organisms. Genes are packaged in chromosomes, and during reproduction those from each parent are split up and then recombined. No new genes are created – that's still down to mutation – but new *combinations* of genes are thrown up. The process of recombination creates genetic diversity in a way that asexual reproduction never can. Natural selection has more options to try out. It also allows beneficial mutations to spread throughout a species more easily – basically speeding up evolution.

The evolution of sex is another of biology's great unsolved riddles. It is easy enough to see *why* it happened, but it has proved impossible to work out *how*. The leading

American evolutionary biologist George C. Williams wrote that sex is ‘the outstanding puzzle in evolutionary biology’.<sup>21</sup> His *Sex and Evolution* (1975) opens with the sentence: ‘This book is written from a conviction that the prevalence of sexual reproduction in higher plants and animals is inconsistent with current evolutionary theory.’<sup>22</sup> His conclusions have a somewhat forlorn tone:

I am sure that many readers have already concluded that I really do not understand the role of sex in either organic or biotic evolution. At least I can claim, on the basis of the conflicting

views in the recent literature, the consolation of abundant company.<sup>23</sup>

John Maynard Smith also devoted a volume, *The Evolution of Sex* (1978), to the various theories on the subject. He, too, concluded forlornly: ‘I fear that the reader may find these models insubstantial and unsatisfactory. But they are the best we have.’<sup>24</sup> In a later essay entitled ‘Why Sex?’, Smith says that even though he has devoted twenty years to the problem of sexual evolution, ‘I

am not sure I know the answer.’<sup>25</sup> (Even so, he still received the Royal Society’s Darwin Award for his contributions to research on the evolution of sex. It seems a little unfair on the rest of us who also don’t know the answer.)

Little progress has been made since the 1970s. In *Evolution* (2004), zoologist and science writer Matt Ridley examines all the most popular theories about how sex evolved, and after finding major problems with the lot, concludes that ‘the existence of sex is the profoundest puzzle of all’.<sup>26</sup>

The prevailing theory is that sex began with the chance fusing of cells infected with different but very similar viruses. When the cells divided, differences between the viruses resulted in a replication of the DNA that prefigured the workings of chromosomes. If this theory is correct, this vitally important change without which nothing bigger than a

virus could exist, didn't even involve a genetic mutation. Even more than other evolutionary changes, it was pure fluke. Like the all-important appearance of eukaryotes discussed above, sex is another thing that owes nothing to the usual neo-Darwinian mechanism.

John Maynard Smith makes the major point that although we think of sex and reproduction as the same, genetically speaking they're the exact opposite. Reproduction turns one cell into two, while sex fuses two to make one. He goes on:

Darwin has taught us to expect organisms to have properties that ensure successful survival and reproduction. Why, then, should they bother with sex, which interrupts reproduction? ...

It is not merely that sex seems pointless: it is actually costly. [27](#)

The big cost is the necessity of producing and maintaining males. Compared to asexual reproduction, sex takes twice as many organisms to produce the same number of offspring. Fewer offspring are produced and

more slowly. These weren't obstacles once sex caught on, but would have been severely restricting in the very earliest stages, when the primitive sexual organisms were in competition with the asexuals, which should have out-bred them. As Williams comments: 'This immediate advantage of asexual reproduction is generally conceded by those who have seriously concerned themselves with the problem.'<sup>28</sup>

As everyone knows from experience, sexually reproducing animals have to devote time and energy to finding mates that could be better used ensuring their survival. And we see the palaver it causes just in the animal and bird world when even after all that strutting and rutting and preening, it is still possible to get rejected – or eaten. As Lynn Margulis and Dorion Sagan acknowledge: 'Biologically, sexual reproduction is still a waste of energy and time.'<sup>29</sup> Many would agree.

For the individual organism, asexual breeding is much better, requiring less energy and biochemical complication. And, according to neo-Darwinism, the individual level is all that matters. The fact that doing things differently might be better for the species as a whole, or for the progress of life in general, is irrelevant. New systems are only adopted if they help the individual; helping the species is just an accidental by-product. As John Maynard Smith acknowledges, both obvious advantages – genetic recombination and speedier evolution – ascribe foresight to evolution, which would cause any self-respecting neo-Darwinist to have apoplexy.<sup>30</sup>

In fact, theoretically, sex shouldn't exist, as Williams admits: 'The impossibility of sex being an immediate reproductive adaptation in higher organisms would seem to be as firmly established a conclusion as can be found in current evolutionary thought.'<sup>31</sup>

But the impossible *did* happen. That was lucky.

And as for the big puzzle of why sex was invented, although of course it would be facetious to suggest it is because it's more fun than cell division, frankly that's as good an idea as any other at the present time.



# SEX AND DEATH

A similar situation applies to the phenomenon of ageing, common to everything above the simplest organisms in the animal world. In nature, ageing *is* death: in the wild individuals rarely have the luxury of dying of old age, as the inability to run away, fight or even chew food properly carries its own death warrant. Without ageing and death the evolution of ever more complex organisms would be impossible. And yet it is far from clear how ageing evolved.

Strange as it may seem, rather than simply being the result of the body wearing out, the build-up of toxins or accumulated oxidation from free radicals, ageing is due to a genetic switch that halts the repair and regeneration processes at a cellular level. Once the repair mechanisms stop, we start to age.

While some individual problems of old age, such as cataracts, are due to the length of time an individual has lived, that's not the same as the general condition of ageing, or senescence. Old age is basically a pre-programmed phase of life, just like puberty. But whereas puberty has an obvious biological function, what on earth is the purpose of ageing?

If ageing is genetic, self-evidently it must have evolved. Indeed, in the 1990s studies of the genomes of different species found it was due to specific genes that are shared throughout the evolutionary tree, from yeast to mammals. <sup>32</sup> An irreversible decline seems to be a common feature of eukaryotes, and emerged at around the same time as sexual reproduction.

The genetic basis of ageing presents something of a problem for natural selection, in which survival is allegedly paramount. To put it kindly, it is a paradox. After all, what's

the survival value of something that kills you?

And there's another problem: how did the ageing genes get passed on in the first place? There must have been a point, very early in the life of eukaryotes, when the genes didn't exist. Therefore mutations must have created them. For most of the organism's life, and especially during its most fertile time, those genes would be irrelevant: they only have an effect when the switch is thrown. So why, then, would natural selection favour them? Why would individuals with the mutations be more successful, producing ever more offspring?

Evolutionary biologists are unable to answer these questions. There aren't even many theories. The most popular hypothesis, that of 'antagonistic pleiotropy' put forward in the 1950s by George C. Williams, was blown out of the water in the 1990s by laboratory experiments. Briefly, Williams' theory

was based on the idea that the ageing genes must also have *beneficial* effects, especially early in life, and although they may have a deleterious effect later this doesn't matter, since the majority of organisms in the wild seldom live to old age anyway. (Live fast, die young, in other words.) Natural selection favoured individuals with the genes because it gave them early advantages, any later drawbacks being irrelevant. Although this may be the only hypothesis that could explain senescence while remaining dutifully neo-Darwinist, *it doesn't work*. New discoveries have highlighted its drawbacks, such as the ageing genes in yeast, for example. And laboratory experiments have not only failed to prove the theory's predictions but have come up with diametrically opposite results – selectively breeding fruit flies to live longer, for example, has shown them to be fitter in early life, too.<sup>33</sup>

The very few other theories all raise more questions than answers. How ageing evolved is literally another one of life's unsolved mysteries.

There is only one known species that is, quite literally, immortal, barring accidents and disease. This is a tiny, 5 mm hydrozoan, *Turritopsis nutricula* – a sort of jellyfish native to the Caribbean – whose special biological talent was only discovered in 2009. *T. nutricula*'s trick is to revert to its sexually immature stage after reproducing, going through an endless cycle of infancy and adulthood. Although apparently unique, it does demonstrate that immortality can evolve. But why isn't it more common, especially since obviously the ultimate in natural selection would not be mere survival but actual immortality?

As with sex, it's easy to see the advantages that ageing has for a *species*, and for the progress of life in general. It avoids over-

population and therefore competition for resources. Just imagine what would happen if a species were both immortal and fertile! It also retains the all-important genetic diversity by renewing the entire population periodically. If older generations didn't die, and were able to mate with younger generations, then a species would never be able to eradicate its old genes. No new, improved genes would ever get a chance to catch on.

It is tempting to speculate that senescence developed specifically in response to the evolution of sex, in order to avoid these problems. Without death, after all, the benefits of sex for the faster spreading of life-improving genes throughout a species would be lost. The only drawback to this neat explanation is that Darwinian theory doesn't allow for it.

The avoidance of overpopulation and the clearing out of the gene pool was, around the turn of the twentieth century, the most

popular explanation even among Darwinists for the development of ageing. But it then dawned that this explanation actually contradicts Darwinism as it assumes that the species as a whole somehow knows what is good for it in the long run. Getting rid of the older generations is advantageous to a species as a whole, but can hardly be said to be much good for an individual, and it is changes in the individual that drive evolution. It's another one of those awkward catch-22 situations that make us feel as though we're missing something vital, somewhere.

# **CREEPS AND JERKS**

Almost incredibly, neo-Darwinism also has difficulty in explaining – of all things – the origin of species ...

The theory of speciation says that when a beneficial mutation occurs, over the course of many generations natural selection carries the new trait to the rest of the species. Eventually so many changes from enough mutant individuals accumulate that a new species comes into being. The new species is genetically distinct from the original, to the point that it cannot breed with any members of the original species that might still be around. Given the air of confidence with which such matters are discussed in the public domain it's surprising that evolutionary biologists can't agree about how the processes governing speciation occur.



Different schools of evolutionary biology have proposed different models of exactly how speciation happens, but none of them can prove theirs to be correct. This is not surprising: it's another of those areas where it is virtually impossible to acquire hard data. Evolution is such a long, slow process. After all, you can't watch it happening in a lab, at least not where the likes of elephants, rubber plants or quantum physicists are concerned. There have been some instructive experiments on the micro level, with bacteria – particularly a long-running series with *E. coli* at Michigan State University – where different strains have changed their genetic make up over many generations. However, such experiments involve primitive species in glorious isolation facing limited survival problems, which hardly mimics the conditions of the real world.

All the scientists really have to work with is observation of the natural world and

analysis of fossils, both of which are severely restricted. Contrary to expectations, the fossil record is not much help for the neo-Darwinian model, since many things that the theory would predict to be there are conspicuous by their absence. Darwin himself, in the words of palaeontologist Stephen Jay Gould, 'viewed palaeontology more as an embarrassment than as an aid to his theory'.<sup>34</sup> And eminent neo-Darwinian Ernst Mayr acknowledged in the late 1980s that following the fossil record:

... seemed to reveal only minimal gradual changes but no clear evidence for any change of a species into a different genus or for the gradual evolution of an evolutionary novelty.

Anything truly novel always seemed to appear quite abruptly in the fossil record.<sup>35</sup>

More recently, Steve Jones, Professor of Genetics at University College, London, has stated that the fossil record 'can look anti-Darwinian', meaning many of the things that should be there just aren't.<sup>36</sup>

These absences are contrary to all Darwinian expectations. After all, the most dramatic changes should take the most time to manifest, and should leave more fossil traces. Yet they are not there. It has to be assumed that this anomaly is due to the fragmentary nature of the fossil record.

True, we are left with the remains of a tiny fraction of all the animals and plants that ever lived, representing a tiny fraction of all the species that have ever evolved. Only about a quarter of a million different species have been found in fossils, whereas the number of species ever to live is probably up there in the billions. The fossil record is really a random sampling of evolutionary history. How random and how big a sample nobody really knows – palaeontologists are left floundering in a statistical gloom.

Others are not so sure that the leaps in the fossil record can be brushed aside quite so easily. Although most biologists maintain

Darwin's original view that evolution is a slow and gradual process, for a minority in the field – mostly palaeontologists – it happens in short, sharp bursts. These are the theories of quantum evolution proposed by George C. Simpson in the 1940s (which still has supporters such as Thomas Cavalier-Smith), and punctuated equilibrium put forward by Stephen Jay Gould and Niles Eldredge in the early 1970s. While the two theories agree the story of each species consists of long periods of stasis with short bursts of rapid evolution, their proposed mechanisms are quite different.

Critics of punctuated equilibrium have called it a theory of evolution by jerks; Gould responded that theirs is a theory of evolution by creeps. But punctuated equilibrium and quantum evolution do at least suggest why the fossil record offers scant evidence of the gradual metamorphosis of one species into another.

The major objection to quantum evolution and punctuated equilibrium is that they require a mechanism over and above ‘classic’ neo-Darwinism, implying that the current theory is incomplete – hardly music to the ears of most evolutionists.<sup>37</sup> Other biologists argue the theory is missing something vital. British biologist Brian Goodwin declares:

... despite the power of molecular genetics to reveal the hereditary essences of organisms, the large-scale aspects of evolution remain unexplained, including the origin of species ... So Darwin's assumption that the tree of life is a consequence of the gradual accumulation of small hereditary differences appears to be without significant support. Some other process is responsible for the emergent properties of life, those distinctive features that separate one group of organisms from another – fishes and amphibians, worms and insects, horsetails and grasses. Clearly something is missing from biology.<sup>38</sup>

# **‘LOOK AT THE KING! LOOK AT THE KING!’**

Most of the problems highlighted above would be solved if there were some way that natural selection could operate at the level of species, or even higher. Something that could see the big picture, in other words. But there is no place in neo-Darwinian theory for this. A species evolves because the individuals within it evolve. Natural selection does not work at the level of the species, or the gene, but the individual.<sup>[39](#)</sup>

We are told, with confidence edging into arrogance, that neo-Darwinism can explain everything in the biological world, and there's no need to invoke anything else. However, as we have seen, it totally fails to explain:

- The origin of life itself, specifically the origin of DNA.
- The appearance of the nucleated, eukaryotic cell without which multicelled life would be impossible. (A 'special case', the result of a process outside the usual neo-Darwinian model.)
- The origin of sexual reproduction, another thing without which complex organisms couldn't evolve. (Another special case that required a non-Darwinian process.) Not to mention how sex caught on, given all its disadvantages.
- How ageing, the clearing out of the gene pool without which evolution couldn't advance, came into being.
- And – irony of ironies – Darwinism can't really explain exactly how species originate.

Frankly, the Emperor is just plain naked. Nude. His only suit is the one he received on his birth day.

No doubt Richard Dawkins will be sighing as – or rather if – he reads this, ‘Here we have yet more non-scientists picking holes in Darwinism just because it can’t explain everything ... so far at least ...’ But there is an elephant in the room that is particularly difficult to miss. In fact, there are so many glaring flaws in the logic of neo-Darwinism that there is a whole herd of deliberately unnoticed pachyderms crammed into that one little space.

Darwinism performs a neat sleight of hand by using observations as explanations. Although perhaps an oversimplification, there is nevertheless some truth in the way that the great iconoclast of scientific theorizing and collector of strange phenomena, Charles Fort sums up the evolutionary message: ‘survivors survive’.<sup>[40](#)</sup> It’s not so very different



from the logic behind the quip: 'Statistically, people who have the most birthdays live longest.'

Neo-Darwinians do have a penchant for seeking to explain all biological phenomena simply by describing them. Take for example convergent evolution – perhaps 'parallel' might be a more apt term – where two species widely separated on the evolutionary tree have independently developed exactly the same anatomical solutions to the same survival problems, without having inherited them from a common ancestor.

There is a plethora of impressive examples across the animal and plant kingdoms where organisms that look virtually identical are in fact completely unrelated genetically. Many of the most obvious are found in Australia, which because it has been cut off from the other continents for around 50 million years, has developed its own idiosyncratic flora and fauna. In particular, marsupials rule,

whereas in the rest of the world mammals with placentas have won the day. This has resulted in many Australian creatures that, fitting the same ecological niche as placental mammals, have evolved a very similar anatomy. There are marsupial moles, which look like moles from elsewhere, marsupial mice that look like non-Australian mice, and even an equivalent of the flying squirrel, the flying phalanger. Since marsupial and placental mammals diverged far back down the evolutionary tree, all of these have evolved completely independently.

But evolutionary theory also recognizes divergent evolution, where different species facing the same survival problems in similar environments come up with *different* solutions. Yet both types of evolution – convergent and divergent – are regularly cited as definitive proof of Darwinism. For example, *New Scientist's* biology features editor Michael Le Page, wrote of divergent evolution

in a 2008 article intended to counter the claims of the intelligent design movement, that ‘there is no reason why a “designer” would not have mixed up these features’.<sup>41</sup> Dawkins also argues that the absence of shared features in distantly related species is evidence against intelligent design – no mammal has feathers, even though they would be useful to flying mammals such as bats.<sup>42</sup> But convergent evolution is just the kind of mixing up that Le Page says never happens; according to his and Dawkins’ logic, convergent evolution must be evidence for design.

Even more bizarrely, Dawkins uses convergent evolution as an argument against intelligent design. He counters creationist claims that complex organs such as the camera eye of mammals – which is made up of separate components that individually do nothing but work perfectly together – could not have evolved by chance. Dawkins points

out that the camera eye has actually evolved independently at least seven times (and eyes of any kind, based on other principles, at least forty).<sup>43</sup> Not only is this a non sequitur – surely it just makes the problem seven times worse – but it also contradicts his argument that the lack of shared features in distantly related species disproves the existence of a designer.

Besides divergent and convergent evolution adapting a species to its environment, there is a third option: no evolution at all, or stasis. Judging by the fossil record, some species – ‘living fossils’ as Darwin put it – have hardly changed over vast tracts of time, including sharks, crocodiles, horseshoe crabs and horsetail plants. According to Ernst Mayr: ‘Some species are extraordinarily young, having originated only 2,000 to 10,000 years ago, while others have not changed visibly in 10 to 50 million years.’<sup>44</sup>

But *why* didn't the living fossils change even a tiny bit over such an extraordinary length of time, when most species quite clearly have? Unsurprisingly nobody knows for sure. It is often said that the non-changers are just perfectly adapted to their environment ('evolutionary complacent', as British comedian David Mitchell puts it), but this is simply putting too positive a gloss on it. The evolutionary explanation is rather that it is because these smug species are so finely attuned to their environment that the slightest change means they can't survive in their own little niche, so no changes ever get a chance to get going. They are trapped in an evolutionary dead end they can never break out of.

But many of these animals and plants are found in different habitats and live alongside other species that *have* continued to evolve. Sharks live in all the oceans of the world alongside a host of fishy creatures that have

evolved way beyond them, and horsetails grow alongside other much more advanced plants. To say chance mutation has never thrown up genetic improvements for these species begs the question of why it obliged for most others.

And there's no question that the conditions in which some of these living fossil species exist have changed dramatically during their existence. Fossil dragonflies from 325 million years ago look exactly the same as today's. Dragonflies are considered to be among the first, if not *the* first, insects – indeed, the first creatures – to develop flight. And they have carried on happily unchanged, seeing the rise and fall of the dinosaurs 230 to 65 million years ago, the appearance of mammals 190 million years ago and birds 150 million years ago.

Today's dragonflies have to survive against predators, chiefly birds and web-spinning spiders, but as the first creatures to take to

the air, they didn't have to contend with them originally. There were simply no birds, flying dinosaurs or mammals. Spiders with the ability to spin suspended webs to catch flying prey only appeared 200 million years ago. But dragonflies nevertheless survived throughout that time, and the appearance of those predators, *without ever adapting*. In other words, dragonflies 325 million years ago were fully adapted to life in the twenty-first century. This flatly contradicts the conventional notion of evolution being an 'arms race' between predators and prey.

All these examples demonstrate that evolutionary theory is so flexible that it, too, has the ability to adapt itself to any given situation. If two species in similar environments are different, that's divergent evolution; if they are the same, that's convergent evolution; if a species hasn't changed at all, that's stasis. It's all OK. It all fits. Actually it doesn't, but it will have to. Nothing is as

evolutionarily complacent as evolutionary theory itself.

There are more examples of this reasoning. Many species have adapted so specifically to a particular habitat that they can survive there and there only. The evolutionary explanation is that the species has carved out its own unique niche and it alone is capable of exploiting it. This means that the species has no competition, and so it thrives. On the other hand, there are animal and plant species living in a wide variety of environments. In these cases, we're told, evolution has favoured flexibility because that increases the chances of survival, as adaptation that is too specific puts all the species' eggs in one basket.

So which is it to be: evolution tending towards increasing specialization or greater versatility? Naturally, the standard answer is that different things work for different species, so each case has to be judged on its own



merits. It's here that we begin to see the infamous circularity at work. Survivors survive. The eminent philosopher of science, Karl Popper, noted scathingly (his emphasis):

Take 'adaptation.' At first sight natural selection appears to explain it, and in a way it does; but hardly in a scientific way. To say that a species now living is adapted to its environment is, in fact, almost a tautology ... Adaptation or fitness is *defined* by modern evolutionists as survival value, and can be measured by actual success in survival: there is hardly any possib-

ility of testing a theory as feeble as this. <sup>45</sup>

This sloppy reasoning also prompted Popper to say (his italics), 'I have come to the conclusion that Darwinism is not a testable scientific theory, but a *metaphysical research programme* – a possible framework for testable scientific theories'.<sup>46</sup> He argued that Darwinism became universally accepted because:

Its theory of adaptation was the first nontheistic one that was convincing; and theism was worse than an open admission of failure, for it created the impression that an ultimate explanation had been reached.

Now to the degree that Darwinism creates the same impression, it is not so very much better than the theistic view of adaptation; it is therefore important to show that Darwinism is

not a scientific theory, but metaphysical. <sup>47</sup>

Labelling natural selection metaphysical is, of course, an exquisite irony.

Even John Maynard Smith, a self-confessed ‘unrepentant neo-Darwinist’,<sup>48</sup> declared his own distaste for the ‘belief that if some characteristic can be seen as benefiting a species, then all is explained’.<sup>49</sup> But sadly that’s all we get from his peers.

Even being generous, the neo-Darwinian theory of evolution is nowhere near as solid as its proselytizers pretend. It has many more gaps and areas of astounding vagueness than they would ever admit to the public. It is, in fact, a startlingly anaemic theory, manifestly failing to explain *any* of the really major events in the development of life on Earth. Its ‘explanation’ of much of the rest is no more than a description, backed up with circular reasoning that assumes the correctness of the theory in the first place. This is

analogous to physicists claiming to have a theory of everything that was absolutely complete – except for its failure to explain gravity or the behaviour of subatomic particles.

Evolutionary biology is, surely, unique among the sciences in that it uses gaps in its knowledge to *support* its fundamental theory, arguing that, since nobody can prove it wrong, the theory must stand. It is less a theory than a default position.

Undeniably, molecular biology has made huge strides in understanding what makes living things tick, particularly the workings of DNA and genes. Although a multitude of mysteries still remain unsolved, the essential laws of genetics – how genes determine the form of an organism and govern its survival, and its role in heredity – have been thoroughly tested scientifically.

What has not been proven, and it is hard to see how it ever could be, is the proposition that random mutations in genes, and random mutations alone, drive evolution. Ever since Darwin, the basic argument has been that chance is responsible for evolutionary change because it *must* be, since self-evidently no non-chance factors can possibly exist. If some other factor was involved it would have to be, by definition, supernatural and everything must be explicable in mechanistic terms? There can be no suggestion of purpose, let alone design. From his ivory tower of certainty, Richard Dawkins writes in the final chapter of *The Blind Watchmaker* (1986) (his emphasis):

My argument [in this chapter] will be that Darwinism is the only known theory that is in principle *capable* of explaining certain aspects of life. If I am right it means that, even if there were no actual evidence in favour of the Darwinian theory (there is, of course) we should still

be justified in preferring it over all rival theories. [50](#)

The reason for this public show of certainty and the unwillingness to admit that there are

gaps in current knowledge are understandable – at least to some extent. Evolutionary biologists would probably be more candid about the weak spots in their discipline if it was not for the fact that vested religious interests are ready to pounce at any sign of wavering.

One consequence of this approach is that evolutionary theory has become the bedrock of scientism – science as an ideology as opposed to a method of investigating the world. As Simon Conway Morris observes, ‘More than one commentator has noted that ultra-Darwinism has pretensions to a secular religion.’<sup>51</sup> Backsliding and expressing honest doubts about the completeness of the theory is simply not tolerated in biology in the same way it is, for example, in physics.

There is no question, however, that the biblical model should be rejected. God did not make all species complete as they are today in a week. Evolution, in its widest

sense, is an established fact, even though many of the details about the precise mechanisms and forces that drive it remain highly debatable. The evidence for natural selection alone deals a deathblow to creationism, although mutation does potentially allow God to slip back in to decide what changes will (or might) work so that tweaks can then be made to DNA. But it is surely something of a demotion, and rather demeaning to an allegedly all-powerful deity. Why should the God of Judeo-Christian tradition be restricted to working in this way?

Rejecting the God of Genesis does not preclude some form of 'soft' design, an active but *limited* creative force at work. In fact, certain aspects of evolutionary history would be easier to explain if such a force existed. Everything considered, neo-Darwinism is neither the coup de grâce to all design theories, nor the atheist epiphany it is supposed to be.

According to Dawkins, once you properly understand neo-Darwinian theory, you *know* there is neither God nor any kind of supernatural force at work in the universe. However, the man who originally formulated the neo-Darwinian synthesis – of which Dawkins is the eager acolyte – saw it very differently. In fact, this largely unacknowledged genius would have had no problem with the thrust of this part of the book ...

# THE GOD GIVER

It comes as something of a shock to discover that Darwin's ideas were far from the overnight success most people believe them to be. As science is no exception to the rule that history is written by the victors, today we have the impression that the publication of Darwin's *On the Origin of Species* changed everything at a stroke. In fact it took almost a century for his ideas to become the scientific givens that they are now. Until as late as the mid-1930s *most* biologists and palaeontologists considered that Darwin was, at best, half right, and factors other than natural selection played a part in evolution. Although a number of influential biologists quickly embraced natural selection, many either rejected it or regarded it as an interesting but unproven hypothesis.



Palaeontologists in particular refused to accept Darwin's theory because it failed to fit the fossil record.<sup>52</sup>

A great irony is that the rise of genetics in the first decades of the twentieth century was originally thought to demolish Darwinism. The whole basis of genetics was that genes are fixed and unchangeable units of heredity – the biological equivalent of atoms – while Darwinism required them to vary. The neo-Darwinian synthesis was the result of reconciling genetics with Darwinism, laying the foundation for everything that has come after. It was the recognition that genetic mutation was the cause of the small, individual variations that natural selection seized on and honed.

Ernst Mayr and science historian William B. Provine sum up the rapidity of the change in attitude in their introduction to *The Evolutionary Synthesis* (1980):

629/900  
In the early 1930s, despite all that had been learned in the preceding seventy years, the level of disagreement among the different camps of biology seemed almost as great as in Darwin's day. And yet, within the short span of twelve years (1936–47), the disagreements were almost suddenly cleared away and a seemingly new theory of evolution was synthesized from

the valid components of the previously feuding theories.

53

The momentum has carried on ever since. But just what happened over those dozen years, and why did Darwinism come out on top after nearly a century in the wilderness?

The surprising difficulty in answering this question is shown by the number of conferences called to discuss the events of those years. *The Evolutionary Synthesis* was a collection of the papers delivered at one such event organized by the American Academy of Arts and Sciences in 1974. A similar gathering was held in 1981 at Bad Homburg in Germany to discuss the rather syntactically tortured question: 'How complete and how stable is, and has been, the evolutionary synthesis, or "neo-Darwinism"?'

It was there that Stephen Jay Gould delivered his paper on 'The Hardening of the Modern Synthesis', which covered the crucial 1936–47 period. After surveying the process of theory-hardening, he came to the more important, but problematic, question of *why* it had happened in the first place, admitting: 'I now arrive at the point where I should give a conclusive and erudite explanation of why the synthesis hardened. Yet truly, I do not know.'<sup>54</sup>

He offered two possible explanations. The first he called the 'heroic' version, which is that evolutionary biologists came up with the right answers through an objective evaluation of the evidence. The second, the 'cynical' version, is that the advocates of natural selection were themselves guilty of selection, by picking only the evidence that fitted the emerging consensus and dismissing the rest:

Since the world is so full of a number of things, cases of both adaptation and nonadaptation abound, and enough examples exist for an impressive catalogue of partisans of either

If the cynical version is right, Gould pointed out, it might be preventing a proper understanding of evolution by ignoring factors other than natural selection. But which is right? Gould concluded once again: 'The only honest answer at the moment is that we do not know.'<sup>56</sup> That was 1981, but the situation is still pretty much the same. Neo-Darwinism still dominates, but perhaps that's because it refuses to look too closely at potentially hostile data.

The 'hardening' of the theory was almost entirely due to one man. Theodosius Dobzhansky (1900–75) was a Russian-born, naturalized American biologist and it was his 1937 landmark book *Genetics and the Origin of Species* that showed the way to reconcile natural selection and genetics.

Flick through the pages of any academic book on neo-Darwinian theory and Dobzhansky is a star, acknowledged for his revolutionary insight that laid the foundation for everything that came after. But look in any more popular account and you'll be lucky to find him so much as mentioned. He doesn't rate a single reference in Dawkins' *The Blind Watchmaker* or *The Greatest Show on Earth* (although he is mentioned in passing in *The Ancestor's Tale*, as 'the great evolutionary geneticist').<sup>57</sup> There may be a good reason for the difference between the way specialists talk about him among themselves and the relative silence in their public pronouncements. It's quite simple. Dobzhansky is something of an embarrassment because he was unashamedly a devout Christian. (Neatly, Theodosius means 'God-giver'.)

Not only was he an active member of the Eastern Orthodox Church, but he saw no

incompatibility between his faith and his belief in evolution. He even saw evolution as God's way of expressing and achieving his purpose, writing in 1970 that, 'man was and is being created in God's image by means of evolutionary developments'.<sup>58</sup>

Dobzhansky regarded evolution as a 'creative process'.<sup>59</sup> To him this did not compromise the essential blindness of natural selection: chance was an important part of the process. He thought that the putative universal designer – to him the Christian God – had set in motion a system that enabled life to develop and find its own way. He preferred to talk of natural selection as *groping* its way forward, having 'tried out an immense number of possibilities and ... discovered many wonderful ones. Among which, to date, the most wonderful is man'.<sup>60</sup>

Even this was part of his much wider vision. In the words of Greek geneticist Costas

R. Krimbas, one of Dobzhansky's research students in the late 1950s, he:

... recognized that organic evolution was part of a cosmic process that comprised the birth and evolution of matter and stellar bodies, the appearance and evolution of life, and finally the genesis of humankind. Every time the process passes from one stage of complexity to the next, it transcended itself, first in the transition from matter to life, and then in the genesis of

humans, the transition from material life to cultural life. <sup>61</sup>

Dobzhansky took the image of groping forward from Pierre Teilhard de Chardin, the French Jesuit palaeontologist who we mentioned briefly earlier, writing: 'This is a splendid, though somewhat impressionistic, characterization of evolution moulded by natural selection.'<sup>62</sup>

Teilhard de Chardin (1881–1955) was a paradoxical combination of Jesuit priest and evolutionary theorist. His eagerness to combine evolution with Catholicism was not shared by his fellow Jesuits, who wasted no time in posting him to China to prevent him lecturing on the subject. There he was part of the team that discovered Peking Man, fossil

remains of *Homo erectus* over half a million years old. Teilhard was forbidden to publish any philosophical works or, on his return to Europe twenty years later, to apply for academic posts. As a result, he went into self-imposed exile in New York. His classic work, *The Phenomenon of Man* (*Le phénomène humain*) was published shortly after his death in 1955, when the ban expired with him.

Teilhard saw the universe as absolutely purposeful, the aim of matter being to engender life and the goal of life being to attain consciousness. He argued that human consciousness would eventually create a planetary spiritual entity that he called the noosphere, which would eventually link with extraterrestrial intelligences; life and mind would then permeate and take control of the universe. The goal of the entire process was the 'Omega point', at which creation reunites with its creator. To Teilhard this meant



reunification with the Christian God. He declared that ‘evolution is an ascent towards consciousness – therefore it should culminate forwards in some sort of supreme consciousness’.<sup>63</sup>

Although most of his concepts had already been around for thousands of years, Teilhard’s contribution was to link them with twentieth-century ideas, particularly those from the biological sciences. The idea that the divine is present in everything and that creation is unfolding and moving determinedly towards a specific end underpins many ancient mystical systems – ironically for Catholic Teilhard, most of them Gnostic. It also very much underpins Hermeticism – especially the prime role of mind in the evolution of the cosmos – and the all-important arcane school of Heliopolis from which it developed.

Tantalizingly, there is even a specific connection, albeit an indirect one, between

Teilhard's ideas and the great Egyptian school. The same underground stream sweeps certain luminaries along throughout the millennia. Teilhard's formative influence was the philosophy of Henri Bergson, particularly his *Creative Evolution* (*L'Évolution Créatrice*), which Teilhard read just before his ordination in 1912. Bergson (1859–1941), in turn, was heavily influenced by the works of Plotinus,<sup>64</sup> the 'Neoplatonic' philosopher who we argue was more accurately neo-Egyptian given that he ultimately drew his inspiration from the religion of Heliopolis. Bergson also gave a series of lectures on the 'numerous and impressive' parallels between Plotinus' system and Leibniz' theory of monads.<sup>65</sup>

Teilhard's ideas on purposeful evolution were surprisingly influential, particularly in the French-speaking world, and remain cautiously debated by scientists such as Christian de Duve, John Barrow and Frank Tipler.

The latter two wrote in *The Anthropic Cosmological Principle* that ‘the basic framework of his theory is really the only framework wherein the evolving cosmos of modern science can be combined with an ultimate meaningfulness to reality.’<sup>66</sup>

Teilhard de Chardin obviously represents the polar opposite to Richard Dawkins, which is deeply ironic given that Dobzhansky, founder of Dawkins’ discipline, embraced Teilhard’s creative evolution. Not only did Dobzhansky greatly respect Teilhard’s philosophy, in the 1960s he even became President of the American Teilhard de Chardin Society. Significantly, however, he did not begin as a ‘Teilhardist’ and tailor his work in evolutionary biology to fit. Quite the reverse. It was his work on the neo-Darwinian synthesis – especially the implications of a creative element in evolution – which led him to Teilhard. To Dobzhansky the genetic system was fully compatible both with the

idea of a creative, intelligent universal power *and* a universe evolving towards an ultimate goal.

However, the mysteries discussed in this chapter suggest that even this fails to present the complete picture. As Dobzhansky saw it, God made DNA and left it to get on by itself, confident it would eventually reach its destination. But perhaps Dobzhansky stopped short of a full answer. It does appear that other events, elements of 'luck' with no connection to the genetic system, were contrived to get life past particular blocks on the evolutionary road ... perhaps with GUD's helping hand.

The belief that a purely mechanistic explanation must lie behind the processes that shape evolution might hold up if the sciences generally had found no evidence of design in the rest of creation. But they have. Physics, in particular, has moved on since the

mechanistic Victorian science in which Darwin advanced his theory. Biology hasn't.

To us, towering above all the other tantalizing hints about *true* intelligent design is the uncanny suitability of DNA and its mysterious origins. There does seem to be something scarily made to order about it. It is not just that a molecule with all the right, miraculous properties for life should have come into being. Whatever process produced DNA did not necessarily have to make something that was also able to adapt to changing conditions. LUCA might have turned out to be an organism that could happily survive and thrive in the conditions of a four-billion-year old Earth, but would die off as soon as those conditions changed.

Similarly, the single-celled life forms that developed from LUCA and populated the planet for the first two or three billion years had limited potential for evolution. Something else had to enter the equation in

order to create the revolutionary new type of nucleated cell that enabled more complex organisms to evolve. The standard theory can only ascribe this to sheer fluke. Another fluke started sexual reproduction, speeding up evolution and allowing even more complex forms of life to develop. But sex, too, faced an obstacle that would have limited the genetic diversity that it otherwise allowed had that obstacle not been removed by the appearance of the genes for ageing and, ultimately, death. Is it just us, or does that seem rather contrived?

Such 'luck' suggests that a proper understanding of evolution *does* require some ongoing creative factor, something somehow capable of comprehending the bigger picture. This, of course, fits elegantly into the designer universe scenario, and supports the evidence from cosmology that the universe was fine-tuned for intelligent life. It also implies, however, that evolution is working

towards a specific end, and that the development of ever-more complex life forms is at the core of that process. This in turn implies that humanity represents its cutting edge.

But is there any evidence that human faculties such as intelligence and consciousness are more than just freak products of a blind universe? And could they be in some way actually fundamental to the cosmos?

## Chapter Eleven

[1](#) E.g. Dawkins, *The God Delusion*, p. 173.

[2](#) Crick, p. 58.

[3](#) Monod, p. 167.

[4](#) Hoyle and Wickramasinghe, *Evolution from Space*, p. 119.

[5](#) Davies, *The Cosmic Blueprint*, p. 109.

[6](#) Smith, *Did Darwin Get It Right?*, p. 167.

[7](#) Crick, p. 113.

[8](#) Narby, *The Cosmic Serpent*, p. 92.

[9](#) De Duve, *Life Evolving*, p. 51.

[10](#) See Leipe, Aravina and Koonin.

[11](#) Hamilton, p. 29.

[12](#) *Ibid.*

[13](#) In his Gifford Lecture ‘Life’s Solution: The Predictability of Evolution Across the Galaxy (and Beyond)’, given at the University of Edinburgh on 19 Feb 2007. Audio file available at the University of Edinburgh’s Humanities and Social Science’s website: [www.hss.ed.ac.uk/gifford-exemp/2000/details/ProfessorSimonConwayMorris.html](http://www.hss.ed.ac.uk/gifford-exemp/2000/details/ProfessorSimonConwayMorris.html).

[14](#) Dawkins, *The God Delusion*, pp. 164–5.

[15](#) Cavalier-Smith, p. 998.

[16](#) Prokaryotes have, since Carl Woese’s discovery in 1977, been divided between bacteria and archaea, as described above, but neither this nor the evolution of the apparent independent DNA of bacteria, affects our point here.

[17](#) Cavalier-Smith, p. 978.



[18](#) *Ibid.*

[19](#) Margulis and Sagan, pp. 115–6.

[20](#) *Ibid.*, p. 118.

[21](#) Quoted in Ridley, p. 315.

[22](#) Williams, p. v.

[23](#) *Ibid.*, p. 11.

[24](#) Smith, *The Evolution of Sex*, p. 10.

[25](#) Smith, *Did Darwin Get It Right?*, p. 165.

[26](#) Ridley, p. xxii.

[27](#) Smith, *Did Darwin Get It Right?*, p. 165.

[28](#) Williams, p. 8.

[29](#) Margulis and Sagan, p. 157.

[30](#) Smith, *Did Darwin Get It Right?*, pp. 166–7.

[31](#) Williams, p. 11.

[32](#) See Guarente and Kenyon.

- 33 A. M. Leroi, A. K. Chippindale and M. R. Rose, 'Long-Term Laboratory Evolution of a Genetic Life-History Trade-Off in *Drosophila Melanogaster*', in Rose, Passananti and Matos (eds.). (This is a reproduction of a paper that first appeared in the journal *Evolution* in 1994.)
- 34 Stephen Jay Gould, 'G. G. Simpson, Paleontology, and the Modern Synthesis', in Mayr and Provine, pp. 153–4.
- 35 Mayr, pp. 529–30. A genus is the next step up from a species in biological classification, a group of distinct species that are closely related genetically, sharing a close common ancestor. Examples are the genera *Canis*, to which dogs, wolves, jackals, coyotes and dingoes belong, and *Equus*, which includes horses, donkeys and zebras.
- 36 In the radio show 'The Whale – A History', in the *In Our Time* series presented by Melvyn Bragg, broadcast on BBC Radio 4 on 21 May 2009.
- 37 See, for example, Smith, *Did Darwin Get It Right?*, pp. 148–9.
- 38 Goodwin, pp. xii–xiii.

39 Many have the impression from the title of his book *The Selfish Gene* that Richard Dawkins proposes that natural selection acts at the level of the gene. But he doesn't: he argues that evolution should be *viewed* from the level of genes, because animals and plants are basically big bags of genes. Natural selection acts on the individual, but its ultimate effect is on the gene pool of the species, determining what genes are in it and how many of each gene there are. Although offering a potentially useful new perspective for evolutionists to look at certain questions, this theory ultimately only describes the same things in different words.

40 Fort, p. 38.

41 Le Page, p. 26.

42 Dawkins, *The Greatest Show on Earth*, pp. 297–8.

43 See Dawkins, *Climbing Mount Improbable*, chapter 5.

44 Mayr, p. 541.

45 Popper, p. 171.

46 *Ibid.*, p. 168.

- [47](#) *Ibid.*, p. 172.
- [48](#) Smith, *Did Darwin Get It Right?*, p. 180.
- [49](#) Smith, *The Evolution of Sex*, p. ix.
- [50](#) Dawkins, *The Blind Watchmaker*, p. 287.
- [51](#) Conway Morris, *Life's Solution*, pp. 315–6.
- [52](#) See Mayr's preface to Mayr and Provine, pp. ix–x.
- [53](#) Mayr and Provine, p. xv.
- [54](#) Stephen Jay Gould, 'The Hardening of the Modern Synthesis', in Grene (ed.), p. 88.
- [55](#) *Ibid.*, p. 90.
- [56](#) *Ibid.*, p. 91.
- [57](#) Dawkins, *The Ancestor's Tale*, p. 262.
- [58](#) Quoted in Costas R. Krimbas, 'The Evolutionary World-view of Theodosius Dobzhansky', in Adams (ed.), p. 188.
- [59](#) Dobzhansky, *Genetics of the Evolutionary Process*, p. 430.
- [60](#) *Ibid.*, p. 431.

- [61](#) Costas R. Krimbas, 'The Evolutionary Worldview of Theodosius Dobzhansky', in Adams (ed.), p. 189.
- [62](#) Dobzhansky, *Genetics of the Evolutionary Process*, p. 391.
- [63](#) Teilhard de Chardin, p. 258.
- [64](#) See Curtis L. Hancock, 'The Influence of Plotinus on Bergson's Critique of Empirical Science', in Harris, vol. I.
- [65](#) Bergson, p. 384.
- [66](#) Barrow and Tipler, p. 204.

## CHAPTER TWELVE

# MIND MATTERS

Despite the bravado and bluster of atheist proselytizers, the very least that can be said is that evolutionary theory is by no means the final proof of their dogma. But there are also certain biological phenomena that apparently provide real evidence for a creative force at work. And this takes modern science ever closer to the core belief and central message of the Hermetica: that every human being is potentially a god, that the universe is alive and that we are all part of its divine long-term plan and destiny.

It seems self-evident when we look at life on Earth and the evolutionary history that science has reconstructed, that life developed from the simple to the complex: from

bacteria, through multi-celled micro-organisms, flatworms and insects to mammals, specifically humans. The impression is one of irrepressible progress, creatures becoming gradually more complicated and more able to interact with and modify their environment, besides becoming increasingly intelligent. From this perspective, the human being is the pinnacle of evolution on Earth, the ‘most wonderful’ result of natural selection to date, as Dobzhansky commented. Karl Popper noted that the ostensibly accidental mutations that drive evolution also uncanonically push a species forward – never a step back. Species seem to change by ‘sequences of evolutionary changes in the same “direction”’.<sup>1</sup>

Ultra-Darwinists such as Dawkins reject this evolutionary directionality as simple ‘species-ism’. We humans think we’re the best evolution has produced because we would, wouldn’t we, being us. We imagine



evolution has progressively produced more impressive species until it made us, its best work to date. But Dawkins argues this attitude is a mistake, if a forgivable one. Objectively, a bacterium or jellyfish is just as 'perfect' a piece of evolutionary design as Professor Dawkins himself (if far more silent). To him, the whole notion of 'higher' and 'lower' forms of life extrapolates too much from simple classifications. And human-like intelligence is by no means an inevitability; the planet got by without it for long enough, after all.

The most that hard-line evolutionists will admit is that natural selection moves a species towards ever more suitable adaptation to its specific environment, but that's not the same as achieving progress for life on Earth in general. Some evolutionists – Dobzhansky being the prime example – do accept the idea of directionality. To them there is no doubt that evolution does tend to produce

increasingly complex and more self-aware creatures. Human beings *are* the pinnacle of evolution (so far), although undeniably there is still considerable room for improvement.

# PIOUS ATHEISTS AND METAPHYSICAL EVOLUTION

Another scientific champion of directionality who believes that it reveals that there are evolutionary rules and principles yet to be recognized, is Simon Conway Morris, Professor of Evolutionary Palaeontology at Cambridge University's Department of Earth Science, and Fellow of the Royal Society. Despite being a committed Anglican, Conway Morris is equally critical of intelligent design and 'ultra-Darwinists' such as Dawkins, who he describes as 'arguably England's most pious atheist',<sup>2</sup> and being 'angry with God'.<sup>3</sup> As for his own position, Conway Morris sums it up in these words:

Evolution is true, it happens, it is the way the world is, and we too are one of its products. This does not mean that evolution does not have metaphysical implications; I remain

Conway Morris' special area of interest, convergent evolution – 'the recurrent tendency of a biological organization to arrive at the same "solution" to a particular "need"'<sup>5</sup> – has led him to the conclusion that it happens far more than neo-Darwinian theory dictates.

Conventional evolutionists believe that if we could restart life on Earth, because the evolutionary paths it took were shaped by random factors, the outcome would be very different. In this scenario animals and plants that are nothing like those we are familiar with would populate the world. Human-like creatures may not exist, since nothing would be inevitable. Conway Morris disagrees, arguing that convergent evolution shows that the number of evolutionary pathways is

limited and that therefore outcomes are largely predetermined. As he said in a 2007 lecture:

In fact, evolution shows an eerie predictability, leading to the direct contradiction of the currently accepted wisdom that insists on evolution being governed by the contingencies of circumstance.<sup>6</sup>

There are vast numbers of ‘macro’ examples of convergent evolution, such as the many in Australia discussed earlier, where the similarity between two species is immediately apparent from their appearance. However, Conway Morris demonstrates that many more similarities are not so obvious, since they relate to individual features, the anatomy and workings of a particular organ, say, or even an internal biochemical process. Backed up by a landslide of examples, it is clear that convergent evolution is, if anything, the norm. Evolution plays the same themes over and over again.

Conway Morris takes the prime example of two creatures that are to all intents and purposes as different as two creatures could be: the human being and the octopus (or more generally, mammals and cephalopods, which also include squid and cuttlefish). He observes the two species are so different that octopuses were frequently used in early science fiction – such as H.G. Wells’s *The War of the Worlds* – as the model for aliens. Humans and octopuses are the product of two entirely different evolutionary lineages. One a vertebrate, the other an invertebrate, they reflect one of the most fundamental and ancient branchings of the evolutionary tree. It is a very, very long time since we shared a common ancestor; cephalopods are, in fact, a class of mollusc, closely related to mussels and slugs. As we live in an entirely different environment and have gone through completely separate adaptations, it is hardly surprising we should look so different from

octopuses. First impressions do seem to confirm conventional wisdom: the further back in time two species shared a common ancestor, the more different they will be now.

The underlying reality is very different. There is startlingly more convergence than one might think. Most obviously, cephalopods have evolved eyes that work, like those of mammals, on the camera principle, with precisely analogous structures performing the same functions. But there are other, equally astonishing similarities. The blood and circulatory system – especially the aorta – of cephalopods is very much the same as that belonging to mammals, and nothing like that of other molluscs. The most intelligent of the invertebrates, the octopus, has evolved a completely different type of brain from mammals', but parts of it are precisely analogous to the mammalian hippocampus and cerebellum. Even the male octopus' sex organ, although positioned at the tip of a

tentacle, is structurally very like the mammalian penis, and bears no comparison with other molluscs'. So despite the overall differences in anatomy and its very separate evolutionary path, the octopus is far more 'mammalian' than we imagine.

There are many other examples which demonstrate that convergence is too widespread to be pure chance. The camera eye evolved no fewer than seven times, quite independently. The compound eye of insects has evolved at least four times. Trichromatic colour vision has evolved separately many times, as is the case with New World monkeys and the Australian marsupial honey possums.

That's the big stuff. Convergent evolution also happens to molecules. The biochemical processes that sustain organisms are often complex, and yet distantly-related species have independently developed exactly the same systems. One of the most striking



examples involves photosynthesis in plants, which uses sunlight to transform carbon dioxide into oxygen. It's not just important for plants, of course: as Conway Morris points out, photosynthesis literally underpins the whole biosphere. Most plants use a chemical process known as  $C_3$  photosynthesis, but many use an alternative, the much more complex  $C_4$ . This is an adaptation to an environmental change: over the last ten million years or so there has been a dramatic drop in carbon dioxide in the atmosphere, making life difficult for many plants. But not all the plant species that use  $C_4$  photosynthesis have, as we might expect, evolved from the first one to hit on it. Despite its complexity, this system has evolved quite independently *at least 31 times*.<sup>7</sup>

Conway Morris' *Life's Solution* (2003) is packed with the most extraordinary examples of convergent evolution among animals, insects, plants and bacteria. He argues

that the unexpected prevalence of convergence shows that, rather than evolution picking paths from a limitless number of possibilities, it continually finds and follows the same well-worn grooves. To him the evidence overwhelmingly suggests that the phenomenon reveals the existence of some factor that neo-Darwinism has yet to recognize. Conway Morris concludes by saying: 'It seems to me that evolution very much has directionalities, and in that sense it has destinations.'<sup>8</sup>

Restricted options imply that certain biological phenomena will *inevitably* evolve. Re-running the history of life on Earth would end up with creatures and plants pretty similar to modern ones. Conway Morris argues one of the outcomes is not only intelligence but that 'the constraints of evolution and ubiquity of convergence make the emergence of something like ourselves a near-inevitability'.<sup>9</sup> According to the conventional view,

humanity is just an insignificant accident, lucky to be here. But Conway Morris makes humans the focus of the universe, the very reason it exists. If evolution was always a journey towards humankind, then we are very special indeed.

## **‘SMALL BUT NOT STUPID’**

Further evidence that the evolutionary trajectory is aiming for creatures like ourselves comes from recent discoveries about intelligence and human-like behaviour throughout the animal kingdom. These revelations are finally overcoming humanity's belief that our species is set apart from the rest of nature, the only creature able to properly think and feel. Now we know that intelligence – the ability to solve problems and react creatively to changing circumstances – is widespread in nature. We may or may not be alone in the universe, but we are not alone on our home planet.

Swiss anthropologist Jeremy Narby's book *Intelligence in Nature* (2005) relates how intelligent, problem-solving behaviour is not just found in higher animals such as

primates and birds, but even among butterflies and such lowly life forms as slime moulds, which can negotiate mazes to find food. Recognizable intelligence is a feature of even the most primitive organisms. Amoebae engage in coordinated, cooperative behaviour to hunt their prey in packs. In 2007 James A. Shapiro, a bacterial geneticist at the University of Chicago, wrote a landmark paper entitled 'Bacteria are Small but not Stupid', a plea for the recognition that bacteria are sentient beings because they 'continually monitor their external and internal environments and compute functional outputs based on information provided by their sensory apparatus'.<sup>[10](#)</sup>

Other research, including that of Jonathan Balcombe, as set out in his book *Second Nature* (2010), has shown that not just intelligence, but other characteristics we usually think of as exclusively human, such as awareness of death, a sense of grief, even a

sense of fun, are an intrinsic part of animal lives. Although elephants' capacity to grieve over the death of a herd member is well known, recently they have been recorded as using ritual – such as passing around sticks – at the death of a loved one. Chimpanzees in a zoo, meanwhile, have been observed to stand silently in a circle and cry as a deceased friend was carried past. As Balcombe repeatedly emphasizes, animals are not just living, *they have lives*. And the complex and often touching nature of their lives reveals their innate intelligence and an awareness of more than simply the mundane and the present.

Many evolutionary biologists, such as Ernst Mayr, believe that while life might be common in the universe, *intelligent* life is so improbable it is virtually exclusive to Earth. Others, such as Christian de Duve take issue:

Conscious thought belongs to the cosmological picture, not as some freak epiphenomenon peculiar to our own biosphere, but as a fundamental manifestation of matter. Thought is

Simon Conway Morris is of the same mind. In 2007 he gave the annual Gifford Lectures at the University of Edinburgh – the series was instigated in 1887 by Lord Gifford to explore the theological implications of scientific advances – under the banner title of ‘Darwin’s Compass: How Evolution Discovers the Song of Creation’. The second of the six lectures was tellingly subtitled ‘The Inevitable Evolution of Intelligence’, while in another he declared the emergence of life, and of intelligence and of intelligent beings ‘really is set into the entire fabric of the cosmos’.<sup>[12](#)</sup>

The universality of intelligence and other mental and emotional phenomena once thought to be exclusively human supports the idea that nature – and indeed the universe – *wants* to produce self-aware organisms able to take control of their

environment. But even given our ever-closer kinship with other species, there does seem a real gulf – a quantum leap – that separates us from even our nearest evolutionary relatives. We wear clothes, tell stories, glory in language, explore our own planet and even deep space with increasingly sophisticated science.

The Dawkins school of thought doesn't deny that we humans are in a unique position, especially when controlling our evolutionary destiny, but contend that it is all just an accident, and there's nothing inherently special about our abilities. Others disagree. Michael Polanyi, Hungarian philosopher of science declares:

It is the height of intellectual perversion to renounce, in the name of scientific objectivity, our position as the highest form of life on earth, and our own advent by a process of evolution as

the most important problem of evolution. <sup>13</sup>

And Simon Conway Morris once again defends human greatness:



... incipient 'human-ness' is clearly visible in a wide variety of animals, be it expressed in terms of toolmaking, singing or even awareness of death. Yet in no case has it 'crystallized'.

We stand alone, feet on the ground, head towards the stars.

But is it intelligence that the universe seems compelled to seek – or is it consciousness? At its most basic, intelligence is the ability to adapt behaviour in response to data received by the senses, the type of intelligence exhibited by bacteria and slime moulds. That kind of problem-solving intelligence doesn't necessarily require self-awareness or the ability to reflect. Slime moulds can learn to negotiate mazes, but still demonstrate nothing like human consciousness. Slime mould philosophers are very rare – as far as we know.

If the universe is designed for life then there must be a reason – something that life is needed for. Cosmic evolutionary theorists such as Teilhard de Chardin argue that consciousness is what life – and even matter – is ultimately striving for. Carl Sagan famously

declared that ‘we are a way for the Cosmos to know itself’.<sup>15</sup> Are we really? Does the universe for some reason *need* conscious entities? And if so, why?

Very bizarrely, there is real scientific evidence that the purpose of the universe is indeed to produce conscious, thinking entities – for a very good reason. It needs us to bring the universe itself into being ...

We are now entering a very strange world indeed.

# GLOBAL EXCITEMENT

We know what we mean by ‘consciousness’ because we all have it and never stop using it until the day we die – and perhaps not even then. But can this elusive invisible thing that shapes our personalities and all of our utterances be defined and explained scientifically? Where does it reside, how does it work, and how does it relate to the world around us? Unlike DNA, which creates and maintains our bodies, it is impossible to locate or analyse consciousness under a microscope, although it is assumed to be connected with the brain.

Since the late 1980s there have been many attempts to explain consciousness in terms of quantum processes. One of the first was by Oxford University mathematician Roger Penrose – author of *The Emperor’s New*

*Mind* (1989) – who went on to collaborate with Stephen Hawking. Penrose said: ‘There is a certain sense in which I would say the universe has a purpose. It’s not there just somehow by chance.’<sup>16</sup>

However, most attempts to link consciousness and quantum theory tend to be fuzzy and speculative, which is not totally surprising as they seek to explain one nebulous issue in terms of another. Basically, although there is a groundswell of feeling that consciousness will prove to be explicable in terms of quantum processes rather than as a chemical product of the brain or similar phenomenon in the ‘macro’ world, it is still very early days. But if the quantum route does turn out to be fruitful, the implications are enormous. It will mean human consciousness is intimately connected with the physical world at a very fundamental level, an astonishing – even apparently magical – scenario, with which the old Hermeticists would

be totally at home. And this fits with accumulating evidence from the physical sciences that the very existence of consciousness can and does have a tangible, measurable effect on the world of matter.

One of the physicists drawn into the study of consciousness was Dick J. Bierman of the University of Amsterdam. From physics he moved into artificial intelligence, which naturally involved a study of cognition – how the mind picks up and processes information about the external world. This led him into the study of consciousness and its relationship with quantum physics. In fact, he got drawn even further into the physicists' forbidden realm of parapsychology, the study of alleged weird abilities and events, known collectively as *psi*. He reasoned that psychic abilities could be a possible manifestation of the interface between consciousness and the quantum world.

But was Bierman brave or just foolhardy to enter the world of parapsychology? Even the word itself is a turn-off to self-confessed rationalists. Ever since attempts began to scientifically test claims of psychic abilities – telepathy, precognition and psychokinesis, or mind over matter – the scientific world has opposed not just the claims, but even the idea of testing them (unless the tests disprove the claims, of course). But why the prejudice?

The fundamental objection is that such phenomena just *can't* exist since they violate the most basic, common sense principles that underpin our understanding of the material world. Telepathy upsets the rule that there must be a physical link between two objects for them to transmit information to each other. Precognition stands the concept of cause and effect on its head. Psychokinesis, or the alleged effect of mind over matter, is the ultimate horror, since it violates pretty

much all the basic principles, including the laws of energy conservation. If real, psychokinesis would mean that it is possible to conjure energy out of nowhere. Unsurprisingly the scientific community at large has a problem with the paranormal. Such things can't possibly *be*.

However, these rules only apply to the macro world of the atom and above. As understanding of the subatomic, quantum world has grown over the last century, it has become increasingly obvious that the common-sense principles with which we judge the world have no jurisdiction down there. There, effects sometimes precede causes ('backward causation'). Particles can jump from one energy state to another without apparently getting the energy from anywhere. Experiments have shown that two particles created by the same event – a collision in a particle accelerator, for example – remain in some weird way connected,

continuing to influence each other even when far apart and no longer linked in any way. And they can do so instantaneously, even seeming to breach the ultimate barrier of the speed of light.

Of all these violations of common sense, the most relevant to this discussion are the ones that relate to time. It may seem odd to most of us, but the fact that time usually flows in just one direction is a real puzzle to physicists, since there is no discernable reason for this according to the laws of physics. In theory many physical processes should be able to work in either direction. Whole conferences have been devoted to the problem of ‘time asymmetry’, such as one organized – somewhat unexpectedly – by NATO in 1991 in Magazan, Spain where celebrity scientists such as Stephen Hawking and John Archibald Wheeler delivered papers.<sup>17</sup>

In his 1988 paper, ‘A World with Retroactive Causation’, Bierman argues that even in



the macro world, 'there is empirical evidence that effects can precede causes'.<sup>18</sup> He argues that no paradox is involved, and that his findings fit the discoveries of quantum physics. Describing its implications 'far-reaching' is something of an understatement.

Given that subatomic particles have been demonstrated to act fast and loose with supposedly inviolable physical principles, it seems almost unsophisticated to insist that they have to be obeyed everywhere else – with no exceptions. The ever-perceptive Paul Davies makes the point that whereas scientists are quite happy to explore ideas of backward causation and instantaneous communication between unconnected particles, 'it is only when the end state involves life and mind that most scientists take fright and bale out'.<sup>19</sup> In other words, it is fine for a subatomic particle to 'see' into the future, but not for a human being.

A handful of physicists – most prominently British professor Brian Josephson, joint winner of the Nobel Prize for Physics in 1973 for his work on superconductivity – has openly accepted the reality of psi and is actively seeking a quantum explanation. As a result he is now head of the Mind-Matter Unification Project at Cambridge's Cavendish Laboratory. Josephson is fond of using the Royal Society's motto, *nullius in verba* – our favourite translation being 'take nobody's word for it' – against scientists who dismiss parapsychology without deigning to look at the evidence. In an interview for *New Scientist* in 2006 on this very topic he railed:

I call it 'pathological disbelief'. The statement 'even if it were true I wouldn't believe it' seems to sum up this attitude. People have this idea that when something can't be reproduced every time, it isn't a real phenomenon. It is like a religious creed where you have to conform to the

'correct' position.<sup>[20](#)</sup>

He added: 'These things are not hard to prove, they're just hard to get accepted.'<sup>[21](#)</sup>

The general trend towards linking consciousness and quantum physics promises parapsychologists real hope. If mind and matter prove to be connected at that deep level it could offer an explanation for psi that keeps it within physical laws. This is the line taken, for example, by leading American parapsychologist Dean Radin in *Entangled Minds: Extrasensory Experience in a Quantum Reality* (2006).

The most exciting discoveries to emerge from parapsychology in recent years do appear to confirm a link between consciousness and the material world at the quantum level. This began serendipitously during research by Bierman.

In the mid-1970s Bierman pioneered the use of Random Event Generators (REGs, also called Random Number Generators) in psi experiments. The advantage of REGs is that they circumvent one of the main problems in evaluating psi experiments. To

substantiate claims of extraordinary abilities, the outcome of an experiment has to be compared to chance, which is why all too often parapsychology disappears into a fog of tedious calculations and statistics that become hard to interpret – or have several possible interpretations. Bierman first used a REG in experiments where volunteers tried to mentally influence the output. It was therefore easy to see whether the output had deviated statistically from chance – as indeed it had, unequivocally.<sup>[22](#)</sup>

In 1995 Bierman was using an REG in a house in Amsterdam where poltergeist activity was allegedly taking place, testing whether the REG behaved differently when the invisible hooligan was at work. When the results were analysed for one particular day they did indeed show a ninety-minute period of non-random output – but puzzlingly this related to no spooky goings on in the house. Bierman and his team realized it coincided

with something rather more mundane: the 1995 UEFA Champions League final, in which Ajax – the famous Amsterdam football team – was playing AC Milan. Even more tantalizing, the moment of greatest non-randomness coincided with Ajax scoring the only goal of the game.<sup>23</sup>

The REG output was obviously affected by some aspect of the game, the most obvious candidate being the country's intense focus and collective excitement. The same effect has been found since, for example in a 2004 study by German researchers at the Institut für Psycho-Physik in Cologne, during an important football match in the city.<sup>24</sup> This suggested a completely new avenue for research, not involving the special mental states associated with psi but the collective workings of ordinary people's consciousness in everyday situations.

Bierman's accidental discovery particularly excited a group of American parapsychologists, including Dean Radin. Seeking the same effect in 1996, he and his colleagues began the REG monitoring of mass events such as the Oscars, the Super bowl and the opening and closing ceremonies of the Atlanta Olympics – anything with television audiences of many millions. Although the results were variable, they seemed to confirm Bierman's discovery. This encouraged them to follow a new line. Rather than picking selected events in advance, they decided to set up a system to permanently monitor fluctuations in global randomness. This way they could find out if a similar effect coincided with unplanned news events – major disasters or the death of an international celebrity, for example.

The idea was given a dry run with the television coverage of Princess Diana's funeral in August 1997, which obviously had the

advantage of being both global and live. Using twelve REGs, they found deviations of 100 to 1 against chance in their output. Can- nily, they used Mother Teresa's funeral a few days later as control. This was also broadcast live, but the peaceful death of an old lady, however much respected, carried little of the raw emotion associated with the demise of a glamorous young princess and mother in horrifying circumstances. This time they found no effect.

Encouraged by these preliminary results, the Global Consciousness Project was created in 1998, funded by the Institute of Noetic Sciences, where Radin is a senior researcher, and headed by Roger Nelson of Princeton University. The Institute of Noetic Sciences is the California-based research institute founded in the 1970s by Apollo astronaut Edgar Mitchell, the sixth man to walk on the moon. ('Noetics' comes from the Greek *nous*, the faculty of 'inner knowing', which has no

exact equivalent in English. The word is liberally sprinkled throughout the Hermetic texts.)

There is now a network of some 65 REGs – nicknamed ‘eggs’ – located all over the world, from large American cities to remote Pacific islands, connected by the Internet. All the REGs do is continually churn out their counts, one per second, day in, day out. The data from each egg is downloaded every five minutes to a server in Princeton, which is accessible to any interested party. The results are then analysed for periods of non-randomness, either from individual or all eggs, which are then compared to world events. Conversely, when major news events occur, the REG data is examined for signs of non-randomness.

One of the most elegant aspects of this set-up is that because the data from all the eggs has to be grouped together, put through a series of statistical analyses and then plotted



on graphs before any anomalies can be noticed, it isn't readily apparent just from the streams of numbers that anything interesting has happened. The analysts can't bias the results even by subconscious selection of the data. Dates and times of all the major global events – both pre-planned such as sporting fixtures and awards ceremonies or random occurrences like major disasters – which happen within a particular period can be listed from an independent source such as the annual review of a news service. The data from the eggs during that period can be analysed independently, and then the two compared for correlations. And the calculations can be checked on request.

The results have been unequivocal. The periods of anomalous non-random output coincide with times of major global events. Dean Radin demonstrated this most vividly in 2001, when the REGs' output deviated from pure chance many times, but one day

above all stood out for the sheer size of the deviation ... 11 September, when the eyes of a horror-struck world were riveted on television footage of the terrorist attack on New York's Twin Towers and its sickening traumatic aftermath. Likening the sharp peaks and troughs on the graph to the ringing of a bell, Radin wrote that, 'in metaphorical terms, our bell rang more loudly on this day than any other day in 2001'.<sup>25</sup>

Even more compelling evidence that the REGs were measuring something real came from a more detailed analysis showing that it wasn't just the amalgamated data from all the eggs that 'rang the bells'; all the individual eggs around the world rang that day. As Radin declared: '*Something*, perhaps changes in mass attention, caused the random data to behave in a dramatically non-random way on 9/11, whereas it behaved normally on other days'.<sup>26</sup>

Inevitably, critics claim that the apparently striking results of the Global Consciousness Project are due to methodological flaws in analyzing the data. But given the sheer amount of accumulated information from the last decade, it is hard to see the results as demonstrating anything other than a real effect. Human consciousness really does seem to have a tangible effect on the material world.

So given the enormous implications, why isn't this 'global coherence effect' much more widely known? Probably because to non-scientists its significance might be hard to grasp and even seem rather dull. After all, this is not exactly moving mountains by the power of mind alone. The experiments show that the focused attention of millions of people is needed to cause just tiny fluctuations in a few REGs – which is not even in the same league as one dramatic spoon-bending.

What exactly do these results tell us? The Global Consciousness Project team use them to support the idea of the evolution of a planetary consciousness – the noosphere, a term borrowed from Teilhard de Chardin. However, that may be extrapolating way too much from the current data. It is true that such an effect is exactly what Teilhard and others would have predicted, and it may indeed turn out to be a sign of the emergence of a global consciousness. But right now the evidence simply doesn't stretch that far.

What can be said at the moment is that the network of REGs is not being *deliberately* influenced by the massed minds of the people on the planet, only a relative handful of whom even know it exists. The REGs can only be registering a side-effect of something else, something that people are unaware of doing. And the effect can't be confined to the REGs; if their output is less random, the effect can only be because all and any random

processes are being smoothed out in some way. When a large number of people pay attention to the same thing, for some as yet unknown reason the world becomes more ordered, particularly at the quantum level where randomness and unpredictability rule. It is not even deliberate; it just seems to be the effect that consciousness creates, simply by existing.

Perhaps what is even weirder is that this is also the thinking of certain top physicists, who propose that consciousness – human or otherwise – is literally what keeps the universe in place. And even that consciousness created the universe in the first place.

# **‘THE MYSTERY WHICH CANNOT GO AWAY’**

We all know the world of quantum mechanics is head-spinningly weird, but it does have a clear relevance to our understanding of life, the universe, everything – and humanity’s role in all of it. And despite the implications of quantum theory being so left-field that even Einstein had problems with it, it does provide some potential clues in our search for the mind of God – or, indeed, our Great Universal Designer, GUD.

Einstein clashed, albeit in a friendly fashion, with Neils Bohr, the great champion of quantum theory, in a debate that went on for nearly thirty years. John Archibald Wheeler, who studied under both luminaries, wrote in his autobiography:

These two giants, full of admiration for each other, never came to agreement. Einstein refused to believe that quantum mechanics provides an acceptable view of reality, yet he could never find an inconsistency in the theory. Bohr defended the theory, yet he could never escape being troubled by its strangeness. Reportedly, once when Einstein remarked, as he liked to do, that he could not believe that God played dice, Bohr said, 'Einstein, stop telling God

what to do'.<sup>[27](#)</sup>

One of the most bizarre aspects of quantum mechanics is that it recognizes an intimate relationship between the mind of an observer and what happens at the quantum level. It is really just a question of how deep the relationship goes.

The classic example comes from the famous 'wave-particle duality' conundrum, the recognition that subatomic particles (in most experiments photons, the particles of light, but it applies to all of them) sometimes behave like particles and sometimes like waves. Richard Feynman called the enigma 'the mystery which cannot go away'.<sup>[28](#)</sup>

The classic demonstration of wave-particle duality is the renowned 'double slit' experiment, the earliest version being carried out

as long ago as 1803, by the woefully little-known English polymath Thomas Young (1773–1829). The scientist, physician, philologist and Egyptologist disproved the prevailing view, established by Newton, that light was made up of particles, by demonstrating it was really a wave. By shining a single beam of light through two narrow slits onto a screen, Young showed that bands of light and dark appeared. Such interference patterns are only explicable if light moves in waves: the light passes through both slits and, just like water in similar circumstances, the two waves emerging from each slit either cancel each other out or reinforce each other to produce the interference pattern.

However, when quantum theory came along a century later, physicists realized that light ought to be made up of particles after all. Young's interference patterns were not initially too much of a problem, since photons en masse could work in waves, just



as sand can be made to ripple in a wave-like fashion. The real difficulties began when even just a single photon at a time was fired at the screen and the same interference patterns built up.

The results were totally counter-intuitive. If one slit is closed and a beam of light shone through the other then – as expected – just a single sharp line appears on the screen. If the slit is closed and the other opened, then again a single line appears in a different place on the screen. But if both slits are open at the same time, you get the interference patterns – even when just a single photon is involved. The photon seems to be interfering with itself, so to speak. As Paul Davies comments: ‘It’s almost as if the photon can be in two places at once, that is pass through both slits.’<sup>[29](#)</sup>

It gets odder. The outcome – whether light behaves as a wave or particle – depends on how the photon is detected after passing

through the slits. When a light-sensitive screen such as a photographic plate is used, the interference patterns typical of a wave appear. If two telescopes or similar devices are instead trained separately on each slit, then every individual photon will be detected by only one device, showing that the particle had, as expected, passed through only one slit. But as the method of detection is chosen by the experimenter, in a sense the observer decides how he or she wants the particle to behave.

There is a more subtle but enormously significant implication. The difference between the two outcomes reflects the difference in the experimenter's knowledge. When a light-sensitive screen is used to detect a photon, the experimenter has no way of telling which slit it has passed through, so it appears as if it has passed through both, giving a wave-like effect. With telescopes the experimenter *can* tell which slit the photon went through

and the photon therefore obligingly acts like the particle it is supposed to be. In other words, it is not just the outcome of the experiment, but the behaviour of the particle itself that seems to depend on what the observer knows – almost as if it depends on the physicist to give it form. When he or she has specific information, the particle behaves specifically; when they have only vague information, the particle behaves vaguely, as if nobody had told it exactly what to do.

In the 1950s Richard Feynman came up with an interpretation of the double-slit experiment based on quantum mechanics that may seem bizarre – even for this strangest of disciplines – but which fits both its theory and practice. According to his interpretation a photon does not take a single path towards the target, but *simultaneously* takes every possible path – it really does go through both slits. The potential paths of the particle represent a series of probabilities, or

possibilities, known as a 'wave function'. It is only when the particle is observed that the wave function 'collapses' and the particle takes on a definite position and path. As John Archibald Wheeler, who taught Feynman, explains (his emphasis): 'Each photon is governed by laws of probability and behaves like a cloud *until it is detected* ... The act of measurement is the transforming act that collapses uncertainty into certainty.'<sup>30</sup> Put another way, until it is measured the photon 'remains an ethereal cloud of probability precisely because it is unobserved'.<sup>31</sup>

If this is correct it would apply to every particle in the universe, and to every property of every particle. They are all wave functions, waiting to receive specific values by being observed. Of course this doesn't mean physicists have a special power that makes subatomic particles submit to their will. What the double-slit experiment and others reveal is the existence of an intimate, and

positively spooky, connection between *any-one's* mind and any matter in the universe.

# THE MECHANISM OF GENESIS

John Archibald Wheeler (1926–2008) proposed the most far-reaching interpretation of the observer effect. One of the giants of theoretical physics, Wheeler studied under Neils Bohr and Einstein. During the 1930s he worked with Bohr and Enrico Fermi on the theory behind the atomic bomb before then moving on to work on the wartime Manhattan Project. He coined the terms ‘black hole’ (the existence of which he predicted theoretically) and ‘wormhole’. In the 1979 *New York Review of Books*, the mathematician Martin Gardner wrote of Wheeler:

No one knows more about modern physics than Wheeler, and few physicists have proposed more challenging speculative ideas. In recent years he has been increasingly concerned with the curious world of QM [quantum mechanics] and its many paradoxes which suggest that, on the microlevel, reality seems more like magic than like nature on the macrolevel. No one wants to revive a solipsism that says a tree doesn't exist unless a person (or a cow?) is looking at it, but a tree is made of particles such as electrons, and when a physicist looks at an

Wheeler made a simple but profound observation about the double-slit experiment that took the observer effect to a whole new level. As we have seen, the outcome – particle or wave – depends essentially on how much information the experimenter chooses to have. Wheeler argued this would even apply if the experimenter possessed the information only *after* the experiment had been run.

To demonstrate this he devised the simple ‘delayed choice’ thought-experiment. Imagine the double-slit experiment was set up so it had *both* a light-sensitive screen and, behind it, two telescopes, one trained on each slit. If the experimenter could somehow decide *after* the photon had passed through the slits which type of detector would come into play then, Wheeler pointed out, logically

exactly the same results would apply as if the experimenter had decided what would happen in advance. The screen would show waves, the telescopes particles.

Again, the outcome would reflect the experimenter's knowledge, but this time they would only have this knowledge after the event. So, if in the normal experiment the observer determined how the particle was going to behave, in Wheeler's delayed choice version, they determined how it *did* behave. The observer could decide how a particle behaved in the past, even if only a few microseconds before. As Wheeler pointed out, thinking this process through logically, you come up with backwards causality – time working the 'wrong' way round.

When it was first advanced, the delayed choice experiment could only be an intellectual exercise. After all, how could the experimenter make the decision and throw the switches in the infinitesimally short time the



photon was between the slit and the detector, travelling at the speed of light? But in 2006, after many unsuccessful attempts, a means of running this experiment for real was devised. A team of French physicists led by Vincent Jacques used a device that allowed a single photon to take either a single or double path, the choice being determined by a quantum Random Event Generator. In this version the experimenter had to make no choice at all, and just had to gather the information at the end of the test. Needless to say the experiment confirmed Wheeler's predictions absolutely.<sup>33</sup>

The delayed choice experiment showed that observations determine events in the past – but how far back could it go? Wheeler came up with another sequence of arguments that showed that it could also work on a cosmic scale. At the time of writing this has yet to be tested experimentally – but the logic holds up.

A well-known phenomenon in astronomy involves the light from a distant star being bent by a body with a massive gravitational force – say a black hole – between the star and Earth. An effect of this ‘gravitational lensing’ is that, if the star is immediately behind the black hole, then from Earth we see two images of the star, one either side. Wheeler pointed out that as the light from the star consists of individual photons, this double imaging means that some have been bent round one side of the black hole, and some round the other. Effectively like being passed through the two slits in a lab. If an experimenter on Earth ran the double-split experiment using light from the star, the result should be exactly the same as with the traditional experiment and the delayed choice version: particles or waves depending on how the experimenter chooses to detect them.

Only in this version of the experiment, the light would have been emitted from the star millions, even billions, of years ago. Obviously it would hardly be possible to decide in advance whether they should be particles or waves. So the choice of today's observer would be to decide which side of the black hole the photons would pass, even though it happened many millions of years ago. As Wheeler explains:

Since we make our decision whether to measure the interference of the two paths or to determine which path was followed a billion or so years after the photon started its journey, we must conclude that our very act of measurement not only revealed the nature of the photon's history on its way to us, but in some sense *determined* that history. The past history of the

universe has no more validity than is assigned by the measurements we make – now! <sup>34</sup>

Paul Davies and John Gribbin comment on the implications of Wheeler's argument: 'In other words, the quantum nature of reality involves non-local effects that could in principle reach right across the Universe and stretch back across time.'<sup>35</sup>

From such reasoning, Wheeler came to a truly extraordinary vision of the role of the mind in the universe. Realizing that the idea that observers affect what they observe only scratches the surface, Wheeler proposed that we should think not in terms of observers but of *participants*. He then asked whether the difference between observation and participation might be ‘the most important clue we have to the genesis of the universe’:<sup>36</sup>

The phenomena called into being by these decisions reach backward in time in their consequences ... back even to the earliest days of the universe .... Useful as it is under everyday circumstances to say that the world exists ‘out there’ independent of us, that view can no

longer be upheld. There is a strange sense in which this is a ‘participatory universe’.<sup>37</sup>

In a positively *Star Trek*-like sound bite, Wheeler declared that: ‘We are participators in bringing into being not only the near and here but the far away and long ago.’<sup>38</sup> From this reasoning he formulated an even more extreme version of the anthropic principle. We saw earlier that this has been conceptually divided between the weak anthropic

principle (the universe looks as if it was designed for life but this is probably an illusion) and the strong anthropic principle (the universe is designed for life). But Wheeler came up with what he termed the participatory anthropic principle – that we are designing the universe. The theory's many knee-jerk detractors were delighted to discover that its acronym is 'PAP'.

According to Wheeler's big idea, the universe was not designed to produce intelligent life for the fun of it; intelligent life is necessary for the universe itself to exist. Writing in 1977 he stated:

The quantum principle shows that there is a sense in which what the observer will do in the future defines what happens in the past – even in a past so remote that life did not then exist, and shows even more, that 'observership' is a prerequisite for any useful version of 'reality'. One is led by these considerations to explore the working hypothesis that 'observership is the

mechanism of genesis'. <sup>39</sup>

Recognizing the momentous nature of Wheeler's hypothesis, Bernard Carr comments:

Wheeler has suggested a more radical interpretation in which the universe does not even come into being in a well-defined way until an observer is produced who can perceive it. In

this case, the very *existence* of the universe depends on life. <sup>40</sup>

The theory eliminates the need for the multi-universe as a solution to the dilemma of the anthropic principle. If the universe needs observers in order to exist then, ‘no universe at all could come into being unless it were guaranteed to produce life, consciousness and observership somewhere and for some little length of time in its history-to-be’.<sup>41</sup>

PAP is admittedly an extreme theory. Its potential for being misunderstood and exploited by a whole range of non-scientists including New Agers and science fiction fantasists is only too obvious. Wheeler was particularly incensed that his cosmological ideas were continually used in attempts to explain parapsychological and paranormal phenomena or were even taken to mean that they had already explained them. As a fierce

opponent of psi and a board member of the American Association for the Advancement of Science, in 1969 he (unsuccessfully) opposed the admission of the Parapsychological Association as an affiliate member. Ten years later – furious at finding himself speaking alongside parapsychologists at an AAAS conference – he tried to have the decision rescinded, writing a hard-hitting paper entitled ‘Drive the Pseudos Out of the Workshop of Science’ that he and fellow sceptics circulated widely, again unsuccessfully.

At first it might seem odd that Wheeler took such a line given that his own ideas seem even weirder than the most incontinent claims of the paranormalists. However, his fury was a result of the fact that his interpretation of quantum mechanics has often been twisted to validate unexplained phenomena. Given that Wheeler seems to be saying that the minds of human observers

affect the universe at a quantum level, some parapsychologists and many New Agers have taken this to mean that the minds of psychics can, for example, cause changes in the subatomic structure of a spoon, making it bend according to their will alone. Wheeler objected that this was not what he was saying at all.

Wheeler's argument is that by discovering the laws of physics that make the universe tick, sentient observers were and are bringing them into being. But they are not actually *making* the laws. There is no free choice involved. In the double slit experiment, for example, the experimenter can 'make' the particles behave as a particle or a wave, but not as anything else. And whatever the observer is doing to influence behaviour is entirely unconscious. Such experiments show that mind and matter are intimately connected, but in a circular relationship where neither has the upper hand. It's not a case of



mind *over* matter, or even matter over mind – both are acting as part of the same process.

One of the intuitive difficulties with the idea of a designer universe is the notion that building a whole universe just to populate odd corners of it with intelligent beings seems rather excessive. Surely GUD could have found a more economical way to work? But Wheeler argues it makes perfect sense if we think not in terms of size but *time*. The universe has to be as big as it is, and to have existed for as long as it has, for the conditions required for life to have arisen. The size and age of the universe are directly related: if the universe contained only enough matter to make one galaxy, it would not be able to exist long enough to make life. (In fact, Wheeler calculated that a galaxy-sized universe would only exist for about a year.)<sup>42</sup> Barrow and Tipler observe that certain of Teilhard de Chardin's arguments supporting his contention that the purpose of the

universe is to produce life are ‘strikingly similar to Wheeler’s idea that the Universe must be at least as large as it is in order for any intelligent life at all to exist in it.’<sup>43</sup>

Even more relevant to this present discussion, Wheeler relates his theory of the participatory universe to the work of Leibniz, one of his scientific and philosophical heroes. In so doing, Wheeler is therefore, however unknowingly, linking his theory to the Hermetic vision. In an article written in 1970, ‘Beyond the Edge of Time’, he suggests that the weak anthropic principle ‘may only be a halfway point on the road toward thinking of the universe as Leibniz did, as a world of relationships, not a world of machinery’ and asks:

Does the universe ... derive its meaning from ‘participation’? Are we destined to return to the great concept of Leibniz, of ‘pre-established harmony’ ... before we can make the next great advance?<sup>44</sup>

John Wheeler is by no means the only eminent physicist to accept such an apparently outrageous idea that we – and all the other intelligent species in the universe – are actually creating the universe, not only now but also back at its beginning. Stephen Hawking, along with collaborators such as American physicist James Hartle and Thomas Hertog of CERN, have embraced much the same idea, and for many of the same reasons. They, too, take the implications of the double-slit experiment and other paradoxes of the quantum world and apply them on a cosmic scale. The major difference with Hawking's vision is that he embraces the multiverse, and so accepts that there are many other universes in which conditions do not support life. Quite how these universes are supposed to exist without benefit of observers is something that is left open.

In his work with Hartle, Hawking extended the idea of wave functions to the entire

universe, devising a mathematical formulation – the ‘Hawking-Hartle state’, developed from one of Wheeler’s equations – to express it. Just as the experimenter in the laboratory collapses the wave function of a photon in the double-split experiment, so observations of the universe collapse its wave functions – not only now but in the past. Backward causality, in other words.

In *The Grand Design* Hawking argues that the traditional ‘bottom-up’ approach to the history of the universe is wrong. Instead of starting with the big bang and working forwards, extrapolating the laws of physics to work out why the universe now is the way it is, we should take a ‘top-down’ line, working backward from the present. This would allow for the fact that, building on the work of Feynman and Wheeler, our existence now determines how the universe began and evolved: ‘We create history by our observation, rather than history creating us.’<sup>45</sup> Or as

*New Scientist* put it in a report on Hawking and Hertog's recent work: 'A measurement made in the present is deciding what happened 13.7 billion years ago; by looking out at the universe, we assign ourselves a particular, concrete history.'<sup>46</sup>

Although the comparison would no doubt have truly appalled Wheeler and probably wouldn't find favour with Hawking, such ideas chime very well with the global coherence effect found by Dick Bierman and the Global Consciousness Project. This shares with the participatory universe hypothesis the basic idea that mind is intimately bound up with matter – indeed, that the mind is even a property of matter. Both show that the very presence of thinking entities affects the physical universe at a quantum level.

# ARE WE GOD?

This idea of the participatory universe understandably fuels all manner of speculation. Perhaps, as humans observe more and more of the universe, both on a cosmic scale and at a quantum level, the relationship between our consciousness and the universe is becoming more and more interdependent. Perhaps, too, as Teilhard de Chardin thought, we, along with extraterrestrial races, are evolving into a cosmic consciousness. This was the plan all along: in the end, we will all *be* the universe. If this is the case then humans are or will be God, the creator of the universe in the first place.

Or maybe there is a hierarchy of observers, with more advanced beings already taking a more active role in shaping the cosmos.

## Barrow and Tipler describe a possible extrapolation of Wheeler's vision:

That there is some Ultimate Observer who is in the end responsible for coordinating the separate observations of the lesser observers and is therefore responsible for bringing the entire

Universe into existence.<sup>47</sup>

If so, they speculate, the unfolding of the universe is leading up to the Ultimate Observer's Final Observation, when the cosmic plan will be complete.

Others still seek to keep a more-or-less traditional God in the picture. Keith Ward, British theologian, philosopher and born-again Anglican minister, takes Wheeler's participatory anthropic principle to a new, if predictable, extreme. Ward suggests that it is not humans and extraterrestrials doing the observing and creating: 'God is the ultimate observer or consciousness which creates the reality.'<sup>48</sup> However, he does accept that human consciousness makes a small contribution to the shaping of the universe. But even

that small contribution represents a huge leap for a born-again Christian priest.

Despite being speculative, all three extrapolations agree that intelligent, conscious beings – such as humans – are in some way partly the creator.

As we have seen throughout this book, understanding God was one of the central inspirations for science. Isaac Newton, for example:

... strove for a unified solution that would encompass not only the mysteries of celestial and terrestrial physics, but also the perennial religious problem of the relation between the Creator and his creation. [49](#)

Echoing this, the man who is in many ways Newton's modern equivalent, Stephen Hawking, writes in the memorable phrase that concludes *A Brief History of Time* (1988) that the ultimate goal of science is to 'know the mind of God'. [50](#)



In fact, the quest for the mind of God may effectively be over. In the end, the journey was not a long one and the destination has proved much closer to home than anyone could have imagined. We all have a share in God's mind simply by being human.

The Hermetic quest also sought primarily to understand the mind of God through knowledge of the cosmos, as can be seen from Treatise XI of the *Corpus Hermeticum*, in which Mind explains to Hermes:

So you must think of god in this way, as having everything – the cosmos, himself, <the> universe – like thoughts within himself. Thus, unless you make yourself equal to god, you cannot understand god; like is understood by like ... Having conceived that nothing is impossible to you, consider yourself immortal and able to understand everything, all art, all learning, the temper of every living thing ... And when you have understood all these at once – times,

places, things, qualities, quantities – then you can understand god.<sup>51</sup>

The Hermetic cosmos itself is also described as a thought of God's, the product of his mind – in a sense, his mind itself.

Even if John Wheeler and other celebrated scientists such as Stephen Hawking are not

aware of it, the universe they describe has such close parallels with the Hermetic vision – the solar child of the ancient Egyptian religion of Heliopolis – that they might as well be the two encircling strands around the same caduceus of wisdom.

In Wheeler's participatory universe, the consciousness of observers is embedded in its structure and both mind and universe are therefore shaped by and shape each other. We are, or at least are part of, the creative force. If for creative force we read God – and the distinction is only a matter of semantics or personal taste – then essentially all humanity is divine or at least an integral part of the divine.

The creative force and the material universe are locked in an eternal embrace or endless creative waltz. Shifting the terminology again, God is the universe, and vice versa. Intelligent beings are part of God, and also, as their minds help shape the universe,

they enjoy a special role in creation. Creator, created and creation are constantly circling in a dazzling dance of ultimate meaning and purpose, an endless jump of joy.

Yet as encapsulated in the *Hermetica*, this apparent welter of transcendentalism has not been the Holy Grail of many of the world's most brilliant minds simply because they liked the mysticism and poetry – although that certainly has its own appeal. To the Hermeticist, pursuing any intellectual endeavour without including the idea of God would be simply absurd. Very succinctly Treatise XI describes the all-pervasive divine: ‘God makes eternity; eternity makes the cosmos; the cosmos makes time; time makes becoming.’<sup>52</sup>

Glenn Alexander Magee writes of the ‘Hermetic doctrine of the “circular” relationship between God and creation and the necessity of man for the completion of God’.<sup>53</sup>

According to the Hermetica then, human-kind has a special place in God's creation. God needs human beings to exist because we are part of God. And we also need God, we need worship, we need awe. The concept of ordinary, everyday humanity in some very real way actually completing God is anathema to, for example, Catholicism, with its fixation on sin, purgatory and subservience to priests and a deity whose separate being is always above and beyond us.

Wheeler says essentially the same thing as the Hermetica: the circular relationship between mind and the universe makes human consciousness necessary for the completion of the universe. The same idea is found in Neoplatonism, which is hardly surprising given the Egyptian roots it shares with Hermeticism, through its founder Plotinus, student of the mysterious Egyptian sage Ammonius Saccas. As Magee notes: 'Like the Hermeticists, Plotinus holds that

the cosmos is a circular process of emanation and return to the One'. [54](#)

From the same basic reasoning as our own – which is based on the growing evidence of design and purpose being uncovered by all the sciences – Austrian astrophysicist Erich Jantsch argued in the 1970s that the universe was ‘self-organizing’: ‘God is not the creator, but the mind of the universe.’[55](#) Although Jantsch found this concept behind many of the great mystical religions, one lay behind them all. He explains that, ‘In the oldest recorded world view, Hermetic philosophy ... this wholeness resting in itself is called the “all”.’[56](#) Jantsch seems to here recognize the origins of Hermeticism in the religion of Heliopolis, whose Pyramid Texts are indeed the world’s oldest cosmological writings.

The same matrix of connections exists between the picture emerging from quantum

physics and Heliopolitan thought. In Wheeler's system, the laws of physics build the material universe, which eventually gives rise to living organisms, which eventually produce sentient beings able to observe and understand the cosmos. By discovering how the universe works, observers are actually creating it in the far distant past – even at the beginning of time. Wheeler saw this as a cycle or feedback loop whereby the universe creates sentient beings who then return the loop back to the beginning. He encapsulated this cycle in his famous diagram showing the eye of the observer looking back at the beginning of the universe (see illustrations) and in the words:

Beginning with the big bang, the universe expands and cools. After eons of dynamic development it gives rise to observership. Acts of observer-participancy ... in turn give tangible 'real-

ity' to the universe not only now but back to the beginning. [57](#)

Exciting though this may be, what Wheeler describes is by no means a new concept. It

resoundingly echoes key ideas of the Pyramid Texts, which speak of how Atum created the big bang – very literally – giving rise to the expanding and ever-more complex universe that ultimately created people, who live on the edge of manifestation, in what Karl Luckert calls the ‘turn-around realm’, the inner place where human consciousness begins its return journey to Atum. And it is not just to him that human consciousness returns, but to his very act of creation – in other words, back to the big bang.

In a deeply satisfying exchange, not only does the latest scientific thinking support the Hermetic cosmology but Hermeticism in turn makes sense of the discoveries of science ... This is as it should be, for it was a brutal operation that severed the two. And now they seem to be calling to each other like separated twins, aching to be as one again.

# Chapter Twelve

- 1 Popper, p. 173.
- 2 Conway Morris, *Life's Solution*, p. 316.
- 3 In his sixth and final Gifford Lecture, 'Towards an Eschatology of Evolution', at the University of Edinburgh, 1 March 2007. Audio file available at the University of Edinburgh's Humanities and Social Science's website: [www.hss.ed.ac.uk/giffordexemp/2000/details/ProfessorSimonConwayMorris](http://www.hss.ed.ac.uk/giffordexemp/2000/details/ProfessorSimonConwayMorris).
- 4 Conway Morris, *Life's Solution*, p. xv.
- 5 *Ibid.*, p. xii.
- 6 Abstract to Conway Morris' Gifford Lecture 'Life's Solution', see Chapter 11, note 13.
- 7 Conway Morris, *Life's Solution*, pp. 292–5.
- 8 In his fourth Gifford Lecture 'Becoming Human: The Continuing Mystery', given at the University of Edinburgh on 26 Feb 2007. Audio file available at the University of Edinburgh's Humanities and Social



Science's website: [www.hss.ed.ac.uk/giffordexemp/2000/details/ProfessorSimonConwayMorris](http://www.hss.ed.ac.uk/giffordexemp/2000/details/ProfessorSimonConwayMorris).

[9](#) Conway Morris, *Life's Solution*, p. 328.

[10](#) Shapiro, p. 807.

[11](#) De Duve, *Vital Dust*, p. 297.

[12](#) In his fourth Gifford Lecture – see note 8 above.

[13](#) Polanyi, p. 47.

[14](#) From the abstract of his fourth Gifford Lecture – see note 8 above.

[15](#) 'Who Speaks for the Earth?', thirteenth and final episode of the TV series *Cosmos*, first broadcast 21 December 1980. DVD released by Freemantle Home Entertainment, 2009. Directed by David F. Oyster, written by Carl Sagan, Ann Druyan and Steven Soter.

[16](#) In the documentary movie *A Brief History of Time*, produced by David Hickman and directed by Errol Morris, Anglia Television/Gordon Freedman Productions, 1991.

[17](#) The papers were published in Halliwell, Pérez-Mercader and Zurek.

- [18](#) Bierman, 'A World With Retroactive Causation', p. 1.
- [19](#) Davies, *The Goldilocks Enigma*, p. 274.
- [20](#) George, p. 56.
- [21](#) *Ibid.*
- [22](#) Bierman and Houtkooper.
- [23](#) See Bierman, 'Exploring Correlations Between Local Emotional and Global Emotional Events and the Behavior of a Random Number Generator'.
- [24](#) Hagel and Tschapke.
- [25](#) Radin, 'Exploring Relationships Between Random Physical Events and Mass Human Attention', p. 538
- [26](#) Radin, *Entangled Minds*, p. 206.
- [27](#) Wheeler, *Geons, Black Holes, and Quantum Foam*, p. 334.
- [28](#) Quoted in Jacques et al, p.1.
- [29](#) Interviewed for 'The Anthropic Universe', *The Science Show*, ABC National Radio, 18 February 2006, presented by Martin Redfern, produced by Pauline Newman.

Transcript available at: [www.abc.net.au/rn/sci-enceshow/stories/2006/1572643](http://www.abc.net.au/rn/sci-enceshow/stories/2006/1572643).

- [30](#) Wheeler, *Geons, Black Holes, and Quantum Foam*, p. 331.
- [31](#) *Ibid.*, p. 333.
- [32](#) Gardner and Wheeler.
- [33](#) Jacques *et al.*
- [34](#) Wheeler, *Geons, Black Holes, and Quantum Foam*, p. 337.
- [35](#) Davies and Gribbin, p. 208.
- [36](#) Wheeler, from his foreword to Barrow and Tipler, p. 6.
- [37](#) John Archibald Wheeler, 'Law Without Law', in Wheeler and Zurek (eds.), p. 194.
- [38](#) On *The Science Show*, ABC National Radio. See note 29 above.
- [39](#) John Archibald Wheeler, 'Genesis and Observership', in Butts and Hintikka (eds.), p. 3.

- [40](#) B. J. Carr, 'On the Origin, Evolution and Purpose of the Physical Universe', in Leslie (ed.), p. 152.
- [41](#) John Archibald Wheeler, 'Genesis and Observership', in Butts and Hintikka (eds.), p. 21.
- [1](#) *Ibid.*, p. 19.
- [43](#) Barrow and Tipler. p. 203.
- [44](#) John Archibald Wheeler, 'Beyond the Edge of Time', in Leslie (ed.), p. 214.
- [45](#) Hawking and Mlodinow, p. 140
- [46](#) Gefter, p. 30.
- [47](#) Barrow and Tipler, p. 470.
- [48](#) On *The Science Show*, ABC National Radio. See note 28 above.
- [49](#) P. M. Rattansi, 'Newton's Alchemical Studies', in Debus (ed.) *Science, Medicine and Society in the Renaissance*, p. 179.
- [50](#) Hawking, *A Brief History of Time*, p. 175.
- [51](#) Copenhaver, p. 41.

[52](#) *Ibid.*, p. 37.

[53](#) Magee, p. 10.

[54](#) *Ibid.*

[55](#) Jantsch, p. 308.

[56](#) *Ibid.*, pp. 308–9.

[57](#) Wheeler, ‘Law without Law’, in Wheeler and Zurek, p. 209.

## CHAPTER THIRTEEN

# ESCAPING FROM FLATLAND

The Hermetica should, at the very least, be given its due because of its truly towering influence over our culture and history since the fifteenth century, especially its powerful role in creating science – though today's practitioners themselves are either unaware of or unwilling to accept this fact. As Richard Westfall writes in relation to Newton:

The Hermetic elements in Newton's thought are not in the end antithetical to the scientific enterprise. Quite the contrary, by wedding the two traditions, the Hermetic and the mechanical, to each other, he established the family line that claims as its direct descendant the very science that sneers today uncomprehendingly at the occult ideas associated with Hermetic

philosophy.<sup>[1](#)</sup>

This convergence of the mechanistic with the mystical is recognized, albeit apparently unconsciously, by the likes of Wheeler, who repeatedly related his work to Leibniz – in turn, at the very least a closet Hermeticist whose own hero was Giordano Bruno – writing, for example:

Inspect the interior of a particle of one type, and magnify it up enormously, and in that interior see one view of the whole universe (compare the concept of monad of Leibniz (1714), “The monads have no window through which anything can enter or depart”); and do likewise for another particle of the same type. Are particles of the same pattern identical in any one cycle of the universe because they give identically patterned views of the same universe? No acceptable explanation for the miraculous identity of particles of the same type has ever been put forward. That identity must be regarded, not as a triviality, but as a central mystery of

physics.<sup>[2](#)</sup>

Westfall points out that the term ‘occult’ first took on its negative connotation when seventeenth-century mechanistic scientists began to use it as a putdown. And so the golden age of scientific mystics was brought down to the level of the sinister, illusory, cheap and nasty. But in fact, ‘occult’ was originally a synonym for ‘Hermetic’.<sup>[3](#)</sup>



After immersing ourselves over the years not only in the history of religions and heresies but also in the history of science, in talking to scientists, delving into the obvious and less obvious learned papers and attending lectures from the very abstruse and arcane to the most direct mechanistic science, we have concluded – along with many of those we quote in this book – that science still needs the Hermetic wisdom.

Science would have found it considerably easier to make sense of the data that it is now uncovering – the designer universe, life as a cosmic imperative, the directionality of evolution, the participatory universe – if it had never jettisoned the Hermetic framework. In fact, it would have *predicted* these discoveries. And although it is impossible to know for sure, we believe the signs are there in the texts themselves that a Hermeticized science would have already advanced far beyond the point that we have reached today. But all is

not lost. David Fideler, editor of *Alexandria: the Journal of Western Cosmological Tradition*, argues that modern science is moving ever more in a Neoplatonic (for which read Hermetic) direction:

Over the last century the mechanistic view of the universe has started to completely break down. Because the implications of quantum mechanics, chaos theory, and the realization that we inhabit an evolutionary, self-organizing universe are starting to work themselves out, it is no exaggeration to say that we are truly living in the midst of a new Cosmological Revolution that will ultimately overthrow the Scientific Revolution of the Renaissance. And if the mechanistic world view left us stranded in Flatland – a two dimensional world of dead, atomistic matter in motion – the emerging cosmological picture is far more complex, multi-dimensional, and resonant with the traditional Neoplatonic metaphor of the living uni-

verse.<sup>4</sup>

Is the ‘living universe’ merely a metaphor? Was it ever? Hermeticists certainly meant it literally. Yet humanity is stranded in ‘Flatland’, shut off from the radiance of the Hermetic vision and all the vast benefits it bestows. This, however, is not inevitably humanity’s end. We can – and must – escape from Flatland.

Fideler refers to the holistic nature of existence, citing the fact that in 1982 physicists showed particles of light from a common

source ‘continue to act in concert with one another’ no matter how far apart they are, a phenomenon known as ‘quantum nonlocality’. He explains:

The tantalizing implication of quantum nonlocality is that the entire universe, which is thought to have blazed forth from the first light of the big bang, is at its deepest level a seamless holistic system in which every ‘particle’ is in ‘communication’ with every other ‘particle’, even though separated by millions of light years. In this sense, experimental science seems to be on the verge of validating the perception of all mystics – Plotinus included – that there is

an underlying unity to the cosmos which transcends the boundaries of space and time.<sup>5</sup>

Fideler argues that the breakdown of the mechanistic worldview requires a new type of science, and proposes that a fusion of the philosophy of Plotinus and Wheeler’s concept of the participatory universe should provide the model. The consequence, says Fideler, is that:

... the focus of life will become more multidimensional, contemplative, and celebratory as we as individuals come to see ourselves as living embodiments of the-universe-in-search-of-its-

own-Being, and as active participants in the ongoing creation of the world.<sup>6</sup>

Unsurprisingly, the ancient source of both Neoplatonism and Hermeticism, the wisdom

of Heliopolis, also offers a way forward, out of Flatland. Karl Luckert states emphatically:

Logic is not abandoned when one tries to understand human existence the ancient Egyptian way; namely, from the perspective of divinely radiated energy and life, from within emanations of divine purpose and pleasure, or from sun rays which in turn engender what we, nowadays, regard as being more 'substantial' protoplasm and genes ... Eternity itself will arbitrate between moribund analytic and disjunctive reasoning, on one hand, and the type of

holistic reasoning which was cherished by Heliopolitan priests on the other.<sup>7</sup>

Yes, science should undoubtedly be more contemplative, inviting practitioners to utilize every level of their minds without embarrassment or shame. The subconscious mind, usually quiescent under a welter of reason and mundane concerns, has long been acknowledged as the most fertile repository of inspiration and even otherwise hidden knowledge. Take the famous case of the German chemist August Kekulé (1829–1896), who, together with a great many of his scientific peers, had been puzzling for a long time about the structure of benzene, but without success. Falling into a daydream or reverie he saw a snake swallowing its own

tail. On coming back to normal consciousness, he realized he had been presented with the answer: six carbon atoms in a ring ... This was not his only example of subconscious prodding. On an earlier occasion a reverie had also provided him with crucial information. On the top of a London omnibus an image of dancing molecules floated into his head, giving him the insight into the theory of chemical structure – and securing him a place in scientific history.

Backed up by arduous study and hard facts, the use of intuition and hunches often provide similar short-cuts – if they are allowed to. Had Kekulé dismissed his insights as ‘just daydreams’ he might never have made his great discoveries.

As that episode reveals, the subconscious mind deals in symbolism and poetry – hence the distinctive surrealism of dreams – the very language that enables the Hermetic texts to seduce and penetrate all levels of the

mind at once. Such symbolism is not moonshine or mumbo jumbo. It is a direct message to the centre of every mind.

# THE NEW SCIENCE

The history of science portrays the mechanistic revolution as an inevitable coming to our senses, a right and proper intellectual maturation. But the reality is that the move away from the mystical side of science was a historical accident. James I's paranoid hatred and fear of witches made it expedient for the likes of Francis Bacon to be seen to have no occult connections, so that side of Hermeticism rapidly became not only unwise, but unfashionable. And the Counter Reformation made it equally dangerous for non-Catholics to be occultists (Catholic occultists not being terribly welcome either), while the French Catholics built up Descartes to oppose the despised Rosicrucianism. If events in the seventeenth century had been slightly different, no doubt all our science

would have continued to work undisturbed within the Hermetic principles right through to today. After all, with such a distinguished track record it would have been foolish to junk it for no reason.

And if the Hermetica had remained influential in academia, science is not the only field that would be different, since the understanding of the universe it bestows affects pretty much everything else in our culture.

When accepting the Liberty Medal on 4 July 1994, Václav Havel, the former dissident playwright who became the first President of the new Czech Republic after the end of the Cold War, lamented the way human rights and freedoms, despite all the big changes that came with the downfall of communism and end of the Cold War, had become 'mere froth floating on the subsiding waters of faith in a purely scientific relationship to the world'.<sup>8</sup> He went on to say that:



Paradoxically, inspiration for the renewal of this lost integrity can once again be found in science. In a science that is new – let us say post-modern – a science producing ideas that in a

certain sense allow it to transcend its own limits.<sup>9</sup>

Havel cited as examples of this ‘post-modern science’ both the anthropic cosmological principle and the Gaia hypothesis. Of the anthropic principle he said:

This is not yet proof that the aim of the universe has always been that in a certain sense it should one day see itself through our eyes. But how else can this matter be explained?<sup>10</sup>

In his view the anthropic principle shows that ‘we are mysteriously connected to the entire universe; we are mirrored in it just as the entire evolution of the universe is mirrored in us’.<sup>11</sup>

If science had been uninterruptedly Hermetic, would the environment be in the same terrifying condition we find it in today? Almost certainly not. Without over-sentimentalizing, the Earth itself would have been cherished as a living being. There would be

no question of having to fight for human rights or the right of animals to be treated gently and with respect. If every human and every beast is an integral part of all creation, then they are all part of us in a very real way. Hurting them would be hurting ourselves. The Hermetic system adds amoral centre to science, which is largely lacking in its amoral mechanistic manifestations and depends almost entirely on the ethics and integrity of individual practitioners.

We began this book by arguing that the magical worldview is essentially hardwired into humanity. Now we can see this is because human beings are aware, at some deep level, of the true nature of the universe and our astonishingly significant role in it. We are indeed hardwired to feel the hollowness of the God-shaped hole deep inside, as the Hermetica acknowledges: 'Praising god is in our nature as humans because we happen to be in some sense his descendants ...'<sup>12</sup>

The evidence that science itself has produced supports the essential ideas that underpin the sense of Otherness innate to human beings. Inconvenient though it may be for the Dawkins' school, there is no doubt that cosmology, physics and many other disciplines, including even biology, present evidence that the universe is non-random, meaningful and designed for life. Science has even felt compelled to rewrite its own rules when it comes across evidence of purpose and design, as is evidenced by the overzealous embracing of the multiverse. It is as if the scientific world is terrified that admitting *anything* non-random will let all the religious 'nonsense' back in.

As with any philosophy worth contemplating, it is the implications that really matter. The path of Hermes Trismegistus illuminated the radiant Renaissance spirit, which burst forth from Pico della Mirandola's *Oration on the Dignity of Man*, which with its

high praise for ‘miraculous man’ cleared with one bound the bigot-built walls that imprisoned human ignorance. Human beings are brilliant because we are all potentially gods and creators. Not born in sin and dirt but in joy and brightness, entering the world not as devil-filled infants but in William Wordsworth’s famous words ‘trailing clouds of glory’. The implications of being god-like humans are enormous. Nothing is beyond us. We can literally reach for the stars. As the Hermetica emphasizes:

For the human is a godlike living thing, not comparable to the other living things of the earth but to those in heaven above, who are called gods. Or better – if one dare tell the truth – the one who is really human is above these gods as well, or at least they are wholly equal in

power to one another.<sup>13</sup>

Likewise, Plotinus wrote of ‘finding the strength to see divinity within’.<sup>14</sup>

However, the Hermetic impetus to find new worlds to conquer carries with it a sense of responsibility. True Hermeticists can never be dictators nor seek to crush the weak

and the vulnerable. For if they themselves, as they believe, are also the universe and even God, how can they damage a fellow god in need of their help? As the *Corpus Hermeticum* states profoundly: ‘There is but one religion of god, and that is not to be evil.’<sup>15</sup>

In the 1970s there was a vogue for books linking the discoveries of physics with Eastern mysticism, such as the works of Fritjof Capra, which provided many seekers with some degree of nourishment to assuage their spiritual hunger. But we should acknowledge that the West has its own, forgotten tradition – Hermeticism – just waiting to provide comfort, knowledge, excitement and freedom.

Like any idea that can turn the world around, the Hermetic universe has been forbidden by the powers of intellectual darkness. The Church demonized it, fearing its potential for firing up generations of men and women to think for themselves about

any subjects that seized their hearts and minds. And after science disowned and disinherited it, originally out of expediency, it became an ingrained prejudice. But the Hermetic flame never died and now, thanks to science itself, the fire – in all but name – seems ready to erupt into the world.

If any one individual symbolizes the tormented history of the Hermetic tradition it is Giordano Bruno. Although a rather sinister statue now stands in Rome at the site of his execution, providing a focus for crowds of pilgrims, few of them seem to realize exactly what he died for. Poor Bruno is either completely ignored or totally misunderstood – if he is remembered at all. He is ultimately portrayed as condemned by the Church either for preaching the existence of the infinite universe or for his support for Copernicus. In a 2010 Reith Lecture, Lord Rees said: ‘The Italian monk and scholar Giordano Bruno, burnt at the stake in 1600,

conjectured that the stars were other “suns”, each with their retinue of planets.’<sup>16</sup> The implication is that he died for science in the modern sense. But Bruno was, in reality, a martyr for *the Hermetic tradition*.

In Europe, the Church told their flock that they were individually weak, miserable sinners, but then the Hermetic Renaissance declared they were quite the opposite, lighting the way to the scientific revolution. In the beginning all science was Hermetic science. But something went badly wrong. When it junked the Hermetic philosophy, science began to preach that we owe our existence to a long series of accidents and that ultimately our lives have no meaning. The sense of unlimited horizons and the joy of being alive were eroded.

When the scientific wisdom was plucked from Hermeticism to fuel the engines of progress for today’s world and the underlying transcendentalism rejected, the whole

tradition lost its soul – specifically the feminine aspect of its soul. When science set its stern face towards the test tube and the slide rule it was in effect turning its back on *Sophia*, the female aspect of the Hermetic knowledge, literally God's other half. And in the ironic replay of the excision of the sacred feminine from Christianity, here science lost not only its soul but also its heart.

Although the names of the great Hermeticists that have come down to us are resolutely male, practitioners such as Bruno took pains to emphasise the rightful place of the feminine, of Isis and Sophia, in the great scheme of things. We suggest that this was not merely some poetic turn of phrase, but a profound acknowledgement of the necessity to embrace the female side of learning and understanding. Whereas men tend to be literal and logical, women tend to think in much more holistic and symbolic ways. To most women who understand the divine, it



can be understood immediately, as a whole. It is not necessary to spell things out or limit their participation in the cosmic dance with hard dogma and punishment. That is what terrified the Inquisitors, and what continues to disturb the Church authorities today.

To be a Hermeticist, no matter what one's gender, is to accept and utilise both male and female mindsets, embodied in the ancient Hermetic and alchemical symbol of the hermaphrodite. Only by becoming whole oneself can the universe be finally understood and totally participated in.

But science, like the Judeo-Christian religions, severed its ties with Sophia, with its other half. And although it can weigh, measure, calculate and send men to play golf on the moon, the real awe and glory of the universe lies in the human heart and soul. If it is allowed to be whole. This was Bruno's message. This was the ancient wisdom. And

simple though it may seem, it is in itself one of the profoundest secrets of all.

The moment to restore the sense of wonder is long overdue. There has never been a better time to let the ‘miracle of man’ back in.

## Chapter Thirteen

- [1](#) Richard S. Westfall, ‘Newton and the Hermetic Tradition’, in Debus (ed.), *Science, Medicine and Society in the Renaissance*, p. 195.
- [2](#) John Archibald Wheeler, ‘Beyond the End of Time’, in Leslie (ed.), p. 212.
- [3](#) Richard S. Westfall, ‘Newton and the Hermetic Tradition’, in Debus (ed.), *Science, Medicine and Society in the Renaissance*, p. 185.
- [4](#) David Fideler, ‘Neoplatonism and the Cosmological Revolution: Holism, Fractal Geometry, and Mind-in-Nature’, in Harris (ed.), vol. I, p. 104.

- 5 *Ibid.*, p. 106.
- 6 *Ibid.*, p. 117.
- 7 Luckert, p. 61.
- 8 National Constitution Centre website: [www.constitution-center.org/libertymedal/recipient\\_1994\\_speech](http://www.constitution-center.org/libertymedal/recipient_1994_speech).
- 9 *Ibid.*
- 10 *Ibid.*
- 11 *Ibid.*
- 12 Copenhaver, p. 65. (Treatise XVIII)
- 13 *Ibid.*, p. 36. (Treatise X)
- 14 Quoted in Fideler, 'Neoplatonism and the Cosmological Revolution: Holism, Fractal Geometry, and Mind-in-Nature', in Harris (ed.), vol. I, p. 116.
- 15 Copenhaver, p. 48 (Treatise XI).
- 16 'What We'll Never Know', Rees' third Reith lecture, broadcast on BBC Radio 4 on 16 June 2010. A transcript is available at: [downloads.bbc.co.uk/rmhttp/radio4/transcripts/20100615-reith.rtf](http://downloads.bbc.co.uk/rmhttp/radio4/transcripts/20100615-reith.rtf).

## APPENDIX

# HERMES AND THE FIRST HERETIC

Sometimes research turns up exciting connections that frustratingly don't belong to the main argument of a book. As some of the information we uncovered on the origins of the Hermetica isn't directly relevant to *The Forbidden Universe* but relates to unfinished business in our previous book, *The Masks of Christ*, we have included it in this appendix.

The inclusion of Hermetic texts such as a Coptic copy of *Asclepius* in the famous collection of books discovered at Nag Hammadi in Egypt in 1945 (often referred to as the Gnostic Gospels) revealed the close connection between Gnosticism and Hermeticism.

## Brian P. Copenhaver explains its significance (his emphases):

The impact of the Nag Hammadi discoveries on our understanding of the *Hermetica* has been enormous. To find theoretical Hermetic writings in *Egypt*, in *Coptic* and alongside the wildest efflorescences of the *Gnostic* imagination was a stunning challenge to the older view

... that the *Hermetica* could be entirely understood in a post-Platonic Greek context.<sup>1</sup>

Other Nag Hammadi books may be largely innocent of the ‘wildest efflorescences’ but they do have ‘doctrinal parallels’<sup>2</sup> with the *Hermetica*. Although this shows that the writers came from a similar school, they often extrapolated their ideas very differently, sometimes in strangely incompatible ways. (Plotinus wrote a tract called *Against the Gnostics*, accusing them of developing their ideas erroneously.)

The discovery had a major impact, and went so far as inspiring the classic *The Gnostic Religion* (1958), by the German-American philosopher Hans Jonas, to

discuss Hermeticism alongside the more familiar Gnostic systems.<sup>3</sup>

Thanks to Dan Brown's blockbusters, millions of people worldwide now know about Gnosticism, the version of Christianity that was eventually anathematized by the emergent Catholic Church and which is associated most with what the Church would have concealed from us. (One of the main revelations of the Nag Hammadi books was the importance of Mary Magdalene and her apparently intimate relationship with Jesus.)

The precise origins of Gnosticism are uncertain and controversial. In a religious sense the term first surfaces towards the end of the second century CE in a Christian context, referring to a sect deemed heretical by the early Church because of its different view of God, Jesus and the path to salvation. The word itself derives from the Greek *gnostikos*, which simply means the ability to acquire knowledge. These heretics called themselves

*gnostikoi* – ‘knowers’ – but the term was also applied to many similar Christian sects, each with its very different views.

The essential difference – what really set them beyond the pale to their detractors – was that these sects believed an understanding of God and individual salvation could be won through direct *personal* experience. Furthermore there was no need for a Church or priesthood as intermediaries – which posed an obvious challenge to the power of the Vatican, with its emphasis on faith rather than understanding, and on collective experience.

Until the last century or so, the earliest known accounts of Gnosticism were found in hostile Christian writings, which stated it grew out of Christianity and therefore post-dated Jesus and Paul. However, more recent research has revealed that Gnostic beliefs were not confined to Christianity, and that the Christian Gnostics had drawn their



worldview from earlier pagan sources, adapting them to the teachings of Jesus.

As a result, the question of the origins of Gnosticism has been hotly debated ever since, but without reaching any conclusive answer. What is known is that it first appeared in the Middle East, particularly Egypt. Different historians champion a Greek, Jewish or Iranian background, or a fusion of all three in Hellenic Alexandria. But once again it is Egypt that beckons.

The fundamental problem in attempting to trace Gnosticism to its source is that there is no agreed definition of 'Gnostic'. To non-specialists (and New Agers) it simply refers to the attitude that salvation or enlightenment is in one's own hands, and requires personal communion with the divine. For academics it describes a specific set of beliefs about the nature of the material world. But there is no consensus about what they are. Even the accepted definition varies between different

countries.<sup>4</sup> That being said, they do agree on certain basic facts.

Gnostics see the material world as inherently flawed, separated from its creator, and believe that the divine and material are mutually antithetical, a belief known as dualism. For Gnostics, salvation is escaping from the prison of the material world, although different Gnostic sects came up with wildly different ways of doing so. For the Christian Gnostics, this meant devising a radically different interpretation of the nature and role of Jesus from the one held by the early Church – another reason why it hated them. (Whether the Church was wrong and the Gnostics right is sadly outside the scope of this book.) Another defining characteristic of Gnosticism is a belief that the god of this world isn't the *real* God. A Kafkaesque, and even *Matrix*-like sense of illusion permeates much of Gnostic thinking. This is hardly a

coincidence: *The Matrix* movies unashamedly draw on Gnostic ideas.

Different Gnostic schools veered off in different directions: the god of this world may be acting under the true God's instructions, may be an evil entity masquerading as God or may be deluded into believing that they actually are God. Then there is the question of the practical applications of spirit-matter dualism: it might lead to asceticism and mortification of the flesh, as it often did. Or it might lead into hedonistic indulgence in the world of the senses – as indeed it did also.

But the parallels with Hermetic and Neoplatonic (and for that matter Platonic) thinking are striking. Neoplatonist belief in the Demiurge and theurgy are essentially the same as that of Gnosticism, as are the Hermeticists' belief in the 'second god' and the possibility of enlightenment through direct communion with the divine.

Excitingly for us, especially considering our conclusions in *The Masks of Christ*, the clearest signs of Egyptian influence are unequivocally right there in the writings of the man the Church declared the ‘first heretic’, the Samaritan Simon Magus, a contemporary of Jesus.<sup>5</sup> The extraordinarily colourful Magus is regarded by modern scholars as a ‘proto-Gnostic’ rather than a Gnostic proper, blending ideas from which Gnosticism, according to the standard definition, was to emerge.

This is Simon’s own summary of his theology:

There is one Power, divided into upper and lower, begetting itself, increasing itself, seeking itself, finding itself, being its own mother, its own father ... its own daughter, its own son ...

<sup>6</sup>  
One, the root of All.

As Karl Luckert points out, this belief system is strikingly similar to that of the distinguished priests of ancient Heliopolis, revealing yet again their presence throughout

history and their resurgence in the early centuries CE.<sup>7</sup> But in the context of Simon Magus we also see how it underpinned even – perhaps especially – the Samaritan religion.

The revelation of an intense kinship between proto-Gnosticism and the Heliopolitan/Hermetic tradition was frankly music to our ears. In *The Masks of Christ* we argue that the exercise of comparing Simon Magus with Jesus strangely elucidates many of the key mysteries and paradoxes about the life and mission of Christ. Although this is the last thing Christians want to hear, the two men were so similar – embodying the same paradoxical blend of the Judaic and pagan – that Simon threatened to undermine Jesus' special status. As a result, the early Church literally demonized him. But according to very early Christian sources, the two men even shared John the Baptist as teacher. Astonishingly, the evidence is that John

chose, of all people, Simon Magus as his successor – and that the headquarters of the Baptist's sect were in Alexandria.<sup>8</sup>

We concluded that the explanation of Jesus' mission lies with the Samaritans, who preserved a more faithful version of the original Israelite religion, and which both Simon Magus and Jesus – as well as John the Baptist – were attempting to restore to all the peoples of Israel, including the then-dominant Judeans, or Jews as they became known. But Luckert's identification of a common thread between Simon Magus' theology and ancient Egypt raises certain basic questions with remarkably far-reaching implications. What does the Samaritan link mean for the history of Christianity? And what does it imply about the true significance of Hermeticism?

If the teachings of Simon Magus were ultimately derived from Heliopolis, this would not only suggest that John the Baptist shared

that legacy, but a very real connection with the Hermetica also emerges. So perhaps it is significant that the Dutch theologian and historian Gilles Quispel, one of the editors of the Nag Hammadi texts, writes:

Owing to the new Hermetic writings that were discovered near Nag Hammadi in 1945, it has become certain that the Hermetic Gnosis was rooted in a secret society in Alexandria, a sort of Masonic lodge, with certain rites, like a kiss of peace, a baptism of rebirth in spirit and a

sacred meal of the brethren.<sup>9</sup>

At the very least this connection reinforces the beliefs of the Renaissance Hermeticists, as expressed most robustly by Giordano Bruno, who also considered Jesus to have attempted to return Judaism to its Egyptian roots. Bruno taught that Jesus practised Egyptian magic. Partly based on the comparison with Simon Magus and partly on other historical evidence, in *The Masks of Christ* we argue that Jesus was perceived in his own time primarily as an Egyptian-style magus.<sup>10</sup>

These links are both exciting and tantalizing, and offer golden opportunities for yet

more profound, even in their own way sensational, discoveries to be made about Egypt's true legacy to the intellectual, emotional and spiritual life of the West.

## Appendix

[1](#) Copenhaver, p. xlv.

[2](#) Fowden, p. 4.

[3](#) Jonas, Chapter Seven.

[4](#) See Yamauchi, Chapter one.

[5](#) There is controversy over whether the few surviving writings ascribed to Simon Magus – which we only have because they were quoted by early Christian writers as fodder for hellfire and damnation fulmination – were written by him or his followers, but either way they reflect his theology and philosophy.

[6](#) Quoted in Luckert, p. 301.

[7](#) *Ibid.*, pp. 299–308.

[8](#) See *The Masks of Christ*, p. 243–51.



- [9](#) G. Quispel, 'The *Asclepius* – From the Hermetic Lodge in Alexandria to the Greek Eucharist and the Roman Mass', in van den Broek and Hanegraff, p. 75.
- [10](#) Picknett and Prince, *The Masks of Christ*, pp. 222–5.

# NOTES AND REFERENCES

## Introduction

1 Quoted in Leake and Sniderman.

2 Quoted in *ibid.*

3 Dawkins, *The God Delusion*, pp. 200–8.

## Chapter One

1 Morris A. Finocchiaro, from his introduction to Galileo, *Galileo on the World Systems*, p. 2.

2 Davies, *The Goldilocks Enigma*, p. 147.

- 3 Our translation of the Latin: '*Siquidem non inepte quidam lucernam mundi, aln mentem, aln rectorem vocant. Trismegistus visibilem Deum ...*'
- 4 For example, Washington State University's World Civilizations website: [www.wsu.edu:8001/~dee/REN/PICO.HTM](http://www.wsu.edu:8001/~dee/REN/PICO.HTM)
- 5 Pico della Mirandola.
- 6 *Ibid.*
- 7 *Ibid.*
- 8 *Ibid.*
- 9 *Ibid.*
- 10 See Yates, *Giordano Bruno and the Hermetic Tradition*, pp. 87–91.
- 11 Some academics prefer 'Hermetism', while others use that term for the original philosophy of the early centuries CE and 'Hermeticism' for its Renaissance reincarnation.
- 12 Tuveson, p. 9.

13 E.g. the opening of Treatise XVI (Copenhaver, p. 58).

14 Lindsay, p. 166.

15 Tuveson, p. xi.

16 Magee, p. 10.

17 Copenhaver, p. 36.

18 Magee, p. 9.

19 Copenhaver, p. 69.

20 Tuveson, p. xii.

21 The relationship between the Sabians of Harran and the Sabians mentioned in the Qur'an – known to us today as the Mandaeans, a baptismal sect whose homeland is in southern Iraq and Iran and who venerate John the Baptist as their great teacher – is a matter of controversy. The line taken by the Arab chroniclers who first set down the al-Mamun story – the earliest account was written about a hundred years after it was supposed to have happened – is that the Harranians took the name simply because although it appears in the Qur'an by then everyone had forgotten who the Sabians were. This is

also the position of most historians. However, there is an intriguing complication, as the Mandaeans also have an ancient link with Harran, which seems to be stretching coincidence rather far, especially for us personally since they were central to our research on the true status of John the Baptist, as discussed in our books, *The Templar Revelation* (Chapter 15) and *The Masks of Christ* (Chapter 7).

22 Gündüz, pp. 157–8 and 209.

23 *Ibid.*, p. 208.

24 Churton, *The Golden Builders*, p. 27.

25 See *ibid.*, p. 38.

26 E.g. Copenhaver, p. xlvi.

27 Tuveson, p. ix.

28 Parks, p. 207.

29 Tompkins, p. 52.

30 Yates, *Giordano Bruno and the Hermetic Tradition*, p. 7, quoting an 1871 translation by William Fletcher.

Copenhaver (p. 71) renders the phrase as ‘progeny of his own divinity’.

31 Copenhaver, p. 2.

32 *Ibid.*, p. 89.

33 E.g. in *Asclepius* (*ibid.*, p. 85).

34 *Ibid.*, p. 59.

35 *Ibid.*, p. 61.

36 Yates, *Giordano Bruno and the Hermetic Tradition*, pp. 154–5.

37 Churton, *The Golden Builders*, p. 59.

38 1 Chronicles 16:30 (TNIV).

39 Joshua 10:12–13 (TNIV).

40 Kepler, p. 391.

41 *Hamlet*, Act II, scene 2.

42 *Ibid.*

43 *Ibid.*

44 Gingerich, p. 23.

45 See Couper and Henbest, pp. 111–3.

46 Quoted in *ibid.*, p. 116.

## Chapter Two

1 Arianism was an alternative view of the nature of Christ that had been rejected and condemned during the formative years of the Catholic Church in the fourth century. In contrast to what became the Church's official position – that God and Christ were of the same substance and that Christ had co-existed with God from the beginning of time – Arianism held that God had created Christ at a specific moment in time. This made him something more like the Gnostic Demiurge – or Hermes' 'second god' – implying that Christ was distinct from God and that there was a time when he had not existed. The Arian view, contrary to a common misconception, was not that Jesus was a mortal chosen by God.

2 Yates, *Giordano Bruno and the Hermetic Tradition*, p. 11.

3 Copenhaver, p. 83.

4 See Picknett and Prince, *The Masks of Christ*, pp. 371–81.

- 5 Quoted in Yates, *Giordano Bruno and the Hermetic Tradition*, p. 340.
- 6 *Ibid.*, p. 215.
- 7 Quoted in *ibid.*, *Giordano Bruno and the Hermetic Tradition*, p. 204.
- 8 Quoted in *ibid.*, p. 206.
- 9 *Ibid.*, p. 288.
- 10 See our *The Masks of Christ*, pp. 197–201 and 222–4.
- 11 Yates, *Giordano Bruno and the Hermetic Tradition*, p. 211.
- 12 Quoted in *ibid.*, pp. 281–2.
- 13 Quoted in Tompkins, p. 75.
- 14 Atanasijevic, p. xxiii.
- 15 *Ibid.*, p. xx.
- 16 Singer, *Giordano Bruno*, p. 363. Singer's book includes a full translation of Bruno's *On the Infinite Universe and Worlds*.



17 *Ibid.*, pp. 322–3.

18 Copenhaver, p. 83.

19 Gingerich, p. 23.

20 Stephen Johnston, ‘Like Father, Like Son? John Dee, Thomas Digges and the Identity of the Mathematician’, in Clucas (ed.), p. 65.

21 See Westman and McGuire, p. 24.

22 Singer, *Giordano Bruno*, p. 285.

23 Tompkins, p. 83.

24 Gribbin, p. 3.

25 Gatti, *Giordano Bruno and Renaissance Science*, pp. 80–5.

26 Gatti, ‘Giordano Bruno’s Copernican Diagrams’, pp. 43–6.

27 Debus, ‘Robert Fludd and the Circulation of the Blood’.

28 *Ibid.*

29 Copenhaver, p. 33.

30 Atanasijevic, p. xvii.

31 *Ibid.*, p. xviii.

32 Yates, *Giordano Bruno and the Hermetic Tradition*, p. 304.

33 Quoted in Tompkins, p. 23.

34 Quoted in Yates, *Giordano Bruno and the Hermetic Tradition*, p. 312.

35 Quoted in *ibid.*, p. 312.

36 See *ibid.*, pp. 320–1.

37 *Ibid.*, p. 341.

38 This is the description given to the extract from Boccalini's work that was included with the first of the Rosicrucian manifestos.

39 Findlen, 'A Hungry Mind'.

40 *Ibid.*

# Chapter Three

- 1 Ferris, pp. 85–6.
- 2 Yates, *Giordano Bruno and the Hermetic Tradition*, p. 360.
- 3 *Ibid.*, p. 363.
- 4 Mason, p. 462.
- 5 *Ibid.*, p. 468.
- 6 See Morley for a translation of *City of the Sun*.
- 7 Interviewed in Burstein and de Keijzer, p. 242.
- 8 Yates, *Giordano Bruno and the Hermetic Tradition*, p. 233.
- 9 Quoted in Olaf Pedersen, ‘Galileo’s Religion’, in Coyne (ed.), p. 75.
- 10 In his notes to Galileo, Salusbury translation, p. 15.
- 11 Oxford University science historian Allan Chapman, quoted in Couper and Henbest, p. 154.

- 12 In his forward to Stillman Drake's translation of Galileo, p. xvii.
- 13 Pedersen, 'Galileo's Religion', in Coyne (ed.), pp. 80–1.
- 14 Quoted in *ibid.*, p. 80.
- 15 Yates, *Giordano Bruno and the Hermetic Tradition*, p. 383.
- 16 This was in a conversation in 1610 with Martin Hasdale, the librarian at Rudolph II's court, who relayed Kepler's remarks to Galileo in a letter. (Singer, *Giordano Bruno*, p. 189.)
- 17 Bruno, *The Ash Wednesday Supper*, pp. 122–3.
- 18 Quoted in Finocchario, p. 88.
- 19 Pedersen, 'Galileo's Religion', in Coyne (ed.), p. 97.
- 20 *Ibid.*, p. 92.
- 21 Finocchiaro, p. 13.
- 22 Quoted in Pedersen, 'Galileo's Religion', in Coyne (ed.), p. 81.
- 23 *Ibid.*, p. 97.

24 Quoted in *ibid.*, p. 81.

25 Yates, *Giordano Bruno and the Hermetic Tradition*, p. 361.

## Chapter Four

1 Fowden, p. xxii.

2 Yates, *Giordano Bruno and the Hermetic Tradition*, p. 21.

3 From Thomas Vaughan's 1652 English translation of the *Fama*, reproduced in the appendix to Yates, *The Rosicrucian Enlightenment*, p. 238.

4 See Churton, *The Golden Builders*, pp. 105–17.

5 Yates, *The Rosicrucian Enlightenment*, p. 250.

6 Churton, *The Golden Builders*, p. 93.

7 *Ibid.*, p. 132.

8 Yates, *The Rosicrucian Enlightenment*, p. 47.

9 Churton, *The Golden Builders*, p. 131.

10 *Ibid.*, p. 143.

11 Theophrastus Bombastus von Hohenheim (1493–1541) – he adopted the name Paracelsus to show he was greater than Celsus, the Roman author of a classic encyclopaedia of medicine – was a Swiss botanist, herbalist and physician. He was heavily influenced by the works of Pico and Ficino, applying the principles of Hermeticism and talismanic magic to healing. His ideas about the combination and manipulation of the elements also led to him to alchemy. Some think that Christian Rosenkreutz was intended to represent Paracelsus, despite the fact that the *Fama* explicitly says that he wasn't a member of the Rosicrucian fraternity, although adding that it did allow him access to the book containing their accumulated wisdom, the 'Book M'.

12 Churton, *The Golden Builders*, p. 157.

13 See Yates, *The Art of Memory*, chapters XV and XVI.

14 Quoted in Yates, *The Rosicrucian Enlightenment*, pp. 101–2.

15 *Ibid.*, p. 136.

16 Purver, p. 223.

17 Quoted in Tompkins, p. 86.

18 Quoted in Yates, *Giordano Bruno and the Hermetic Tradition*, p. 445. (Our translation from the French.)

19 Yates, *The Rosicrucian Enlightenment*, p. 113.

## Chapter Five

1 Couturat, p. 131.

2 Yates, *The Art of Memory*, pp. 387–8.

3 *Ibid.*, p. 382.

4 Quoted in the online Stanford Encyclopaedia of Philosophy: [plato.stanford.edu/entries/leibniz](http://plato.stanford.edu/entries/leibniz).

5 Stanford Encyclopaedia of Philosophy website: [plato.stanford.edu/entries/cambridge-platonists](http://plato.stanford.edu/entries/cambridge-platonists).

6 See Yates, *The Art of Memory*, p. 388, and Atanasijevic, p. xviii.

7 Yates, *The Art of Memory*, p. 388.

8 Quoted in *ibid.*, p. 385.

9 Quoted in *ibid.*

10 Strange Science website: [www.strangescience.net/kircher.htm](http://www.strangescience.net/kircher.htm).

11 Quoted in Tompkins, p. 90.

12 *Ibid.*, p. 97.

13 Interviewed in Burstein and de Keijzer, pp. 239–40.

14 See ‘Bernini’s Elephant and Obelisk’ in Hecksher. This is a reproduction of an article that appeared in *The Art Bulletin* in 1947.

15 Quoted in Tompkins, p. 88.

16 Tod Marder, ‘A Bernini Expert Reflects on Dan Brown’s Use of the Baroque Master’, in Burstein and de Keijzer, p. 255.

17 Tompkins, p. 97.

18 Quoted in Ingrid D. Rowland, ‘Athanasius Kircher, Giordano Bruno, and the *Panspermia* of the Infinite Universe’, in Findlen (ed.), *Athanasius Kircher*, p. 56.

19 See Picknett, *Mary Magdalene*, pp. 27–9.



20 Tompkins, p. 100.

21 Ingrid D. Rowland, 'Athanasius Kircher, Giordano Bruno, and the *Panspermia* of the Infinite Universe', in Findlen (ed.), *Athanasius Kircher*, pp. 201–2.

## Chapter Six

1 Quoted in Yates, *The Rosicrucian Enlightenment*, p. 186.

2 Stanford Encyclopaedia of Philosophy, online: plato.stanford.edu/entries/cambridge-platonists.

3 Quoted in Dobbs, *The Foundations of Newton's Alchemy*, p. 115.

4 P. M. Rattansi, 'Some Evaluations of Reason in Sixteenth- and Seventeenth-Century Natural Philosophy', in Teich and Young (eds.), p. 151.

5 Quoted in Yates, *Giordano Bruno and the Hermetic Tradition*, p. 424.

6 Purver, p. 217.

7 Quoted in *ibid.*, pp. 221–2.

- 8 Quoted in *ibid.*, p. 219.
- 9 Quoted in *ibid.*, p. 198.
- 10 Quoted in *ibid.*, p. 199.
- 11 Bacon, p. 67.
- 12 Rossi, pp. 13–14.
- 13 Tuveson, p. 52.
- 14 Bacon, p. ix.
- 15 J. R. Ravetz, 'Francis Bacon and the Reform of Philosophy', in Debus (ed.), *Science, Medicine and Society in the Renaissance*, vol. II, p. 101.
- 16 Bacon, p. 1.
- 17 *Ibid.*, pp. 2–3.
- 18 *Ibid.*, p. 3.
- 19 E.g. Tuveson, pp. 170–9, Yates, *The Rosicrucian Enlightenment*, chapter XV.
- 20 Lomas, p. 320.

21 From Lomas' lecture 'Sir Robert Moray – Soldier, Scientists, Spy, Freemason and Founder of the Royal Society', given at Gresham College, 4 April 2007. A transcript is available on the Gresham College website: [www.gresham.ac.uk/event.aspPageId=45&EventId=589](http://www.gresham.ac.uk/event.aspPageId=45&EventId=589).

22 Quoted in Purver, p. 221.

23 Quoted in *ibid.*, pp. 221–2.

24 Quoted in *ibid.*, p. 232.

25 Quoted in *ibid.*

26 Quoted in Bluhm, p. 185.

27 *Ibid.*, pp. 183–6.

28 Gribbin, p. 229.

29 Lord Rees, today's President of the Royal Society, quoted in Bragg, p. 22.

30 Gribbin, pp. 238–9.

31 Hollis, p. 262.

- 32 Richard S. Westfall, 'Newton and the Hermetic Tradition', in Debus (ed.), *Science, Medicine and Society in the Renaissance*, vol. II, pp. 185–6.
- 33 'Newton, the Man' in Keynes, p. 363.
- 34 *Ibid.*, p. 366.
- 35 Quoted in Yates, *The Rosicrucian Enlightenment*, p. 200.
- 36 McGuire and Rattansi, p. 109.
- 37 *Ibid.*, p. 127.
- 38 *Ibid.*, p. 124.
- 39 Westfall, 'Newton and the Hermetic Tradition', in Debus (ed.), *Science, Medicine and Society in the Renaissance*, vol. II, p. 193.
- 40 Dobbs, *The Janus Face of Genius*, p. 68.
- 41 *Ibid.*, pp. 185–6.
- 42 Quoted in Westfall, *Never at Rest*, p. 434.
- 43 Westfall, 'Newton and the Hermetic Tradition', in Debus (ed.), vol. II, pp. 194–5.

44 Hitchens, p. 65.

## Chapter Seven

1 Fowden, pp. 68–74.

2 See below, p. 185.

3 Festugière, p. 102.

4 Luckert, p. 55.

5 Lurker, p. 121.

6 *Ibid.*

7 Ray, p. 65.

8 *Ibid.*, p. 160.

9 Fowden, p. 34.

10 Lurker, pp. 69–70.

11 Ray, p. 165.

12 Fowden, p. 27.

13 *Ibid.*, pp. 40–1.

14 According to Plutarch (p. 161) the establishing of the Serapis cult was the work of Manetho and a member of the family that held the hereditary priesthood of the Greek mystery centre of Eleusis, which makes sense if it was to be a 'hybrid' cult for Egyptians and Greeks. Although some doubt Plutarch's story, Manetho was certainly associated with the cult – see J. Gwyn Griffith's notes to *ibid.*, pp. 387–8.

15 Iamblichus, p. 5.

16 Fowden, p. xxv.

17 Churton, *The Gnostic Philosophy*, p. 120.

18 Plotinus, p. 9.

19 Luckert, p. 261.

20 *Ibid.*, p. 262.

21 Quoted in *ibid.*, p. 260.

22 See *ibid.*, chapter 14.

23 *Ibid.*, p. 257.

- 24 Eunapius, 'Lives of the Philosophers', in Philostratus and Eunapius, pp. 419–25.
- 25 Herodotus, p. 130.
- 26 Luckert, p. 42.
- 27 E.g. Lurker, p. 99.
- 28 See Luckert, chapter 2.
- 29 *Ibid.*, p. 52.
- 30 Lurker, p. 31.
- 31 Lehner, p. 34.
- 32 Luckert, p. 52.
- 33 *Ibid.*, p. 45.
- 34 *Ibid.*, p. 57.
- 35 Campbell and Musès, p. 138.

## Chapter Eight

- 1 'Humanism' is a fluid term, coined in the mid-nineteenth century and applied not just to contemporary ideas but

also retrospectively to earlier philosophers and social reformers. It is applied to any philosophy that places human beings at the centre of things, asserting not only their fundamental right to control their own destiny but also stressing their *ability* to do so. But beyond that, the precise definition varies depending on the era in question: the values and ideals of a twenty-first century humanist are very different from a fifteenth-century one. The biggest difference is that today's humanism tends to eschew the metaphysical and religious. Under this definition, the likes of Pico, Ficino and Bruno qualify as humanists, but they would never have recognized the term.

2 Magee, p. 7.

3 P. M. Rattansi, 'Some Evaluations of Reason in Sixteenth- and Seventeenth-Century Natural Philosophy', in Teich and Young (eds.), p. 149.

## Chapter Nine

1 In the radio programme 'The Multiverse', part of the *In Our Time* series, broadcast on BBC Radio 4 on 21 February 2008.



- 2 Barrow and Tipler, p. 5.
- 3 Susskind, 'A Universe Like No Other', p. 38.
- 4 Weinberg, *The First Three Minutes*, p. 154.
- 5 Carr and Rees, p. 612.
- 6 Dyson, p. 44.
- 7 Quoted in Davies, *The Mind of God*, p. 199.
- 8 Stockwood (ed.), p. 64.
- 9 Davies, *The Mind of God*, Chapter 8.
- 10 Feynman, p. 12.
- 11 Davies, *The Mind of God*, p. 197.
- 12 In the BBC Radio 4 programme 'The Multiverse' (see note 1 above).
- 13 Hawking and Mlodinow, p. 161.
- 14 Davies, *The Goldilocks Enigma*, pp. 166–70.
- 15 Susskind, 'A Universe Like No Other', p. 37.
- 16 *Ibid.*, p. 39.

- 17 In the BBC Radio 4 programme 'The Multiverse' (see note 1 above).
- 18 Smolin, *The Trouble with Physics*, pp. 166–7.
- 19 Jeans, p. 96.
- 20 Davies, *The Mind of God*, p. 173.
- 21 In the BBC Radio 4 programme 'The Multiverse' (see note 1 above).
- 22 Carr, p. 14.
- 23 *Ibid.*
- 24 Quoted in Smolin, *The Trouble With Physics*, p. 125.
- 25 *Ibid.*, pp. 158–9.
- 26 Al-Khalili, p. 23.
- 27 Smolin, *The Trouble with Physics*, p. 163.
- 28 Quoted in Malone, p. 191.
- 29 See Nick Bostrom, 'Are We Living in *The Matrix*? The Simulation Argument', in Yeffeth (ed.).
- 30 Davies, *The Goldilocks Enigma*, pp. 213–4.

31 Hawking, 'The Grand Designer', p. 25.

32 Al-Khalili, p. 23.

33 Weinberg, *Dreams of a Final Theory*, p. 182.

34 Susskind, 'A Universe Like No Other', p. 36.

35 Weinberg, *Dreams of a Final Theory*, p. 182.

36 Hoyle, pp. 217–8.

37 Davies, *The Mind of God*, p. 16.

38 Quoted in Schönborn.

39 *Ibid.*

40 Davies, *The Goldilocks Enigma*, pp. 228–30.

## Chapter Ten

1 Quoted in Dawkins, *The Greatest Show on Earth*, p. 417.

2 *Ibid.*, p. 416.

3 De Duve, *Vital Dust*, p. xv.

4 Watson and Crick, p. 738.

- 5 See Ingrid D. Rowland, 'Athanasius Kircher, Giordano Bruno, and the *Panspermia* of the Infinite Universe', in Findlen (ed.).
- 6 Hoyle and Wickramasinghe, *Evolution from Space*, p. xiii–xv.
- 7 Quoted in Carey.
- 8 Quoted on BBC News website, "“Life Chemical” Detected in Comet', 18 August 2009: [news.bbc.co.uk/1/hi/sci/tech/8208307.stm](http://news.bbc.co.uk/1/hi/sci/tech/8208307.stm).
- 9 Schueller, p. 34.
- 10 Quoted in *ibid.*, p. 31.
- 11 *Ibid.*, p. 34.
- 12 Quoted in *ibid.*, p. 35
- 13 Lovelock, *Gaia* (1979 edition), p. vii.
- 14 In the documentary, 'Life, the Universe and *Everything*: James Lovelock' in the *Beautiful Minds* series, produced and directed by Paul Bernays, ARC Productions for BBC Four, 2010.

15 Interviewed in the above documentary.

16 Lovelock, *Gaia* (2000 edition), p. xv.

17 *Ibid.*, p. ix.

18 Lovelock, *The Ages of Gaia*, p. 15.

19 De Duve, *Vital Dust*, p. 20.

20 *Ibid.*, pp. 286–9.

21 *Ibid.*, pp. 292–3.

## Chapter Eleven

1 E.g. Dawkins, *The God Delusion*, p. 173.

2 Crick, p. 58.

3 Monod, p. 167.

4 Hoyle and Wickramasinghe, *Evolution from Space*, p. 119.

5 Davies, *The Cosmic Blueprint*, p. 109.

6 Smith, *Did Darwin Get It Right?*, p. 167.

7 Crick, p. 113.

8 Narby, *The Cosmic Serpent*, p. 92.

9 De Duve, *Life Evolving*, p. 51.

10 See Leipe, Aravina and Koonin.

11 Hamilton, p. 29.

12 *Ibid.*

13 In his Gifford Lecture ‘Life’s Solution: The Predictability of Evolution Across the Galaxy (and Beyond)’, given at the University of Edinburgh on 19 Feb 2007. Audio file available at the University of Edinburgh’s Humanities and Social Science’s website: [www.hss.ed.ac.uk/gifford-exemp/2000/details/ProfessorSimonConwayMorris.html](http://www.hss.ed.ac.uk/gifford-exemp/2000/details/ProfessorSimonConwayMorris.html).

14 Dawkins, *The God Delusion*, pp. 164–5.

15 Cavalier-Smith, p. 998.

16 Prokaryotes have, since Carl Woese’s discovery in 1977, been divided between bacteria and archaea, as described above, but neither this nor the evolution of the apparent independent DNA of bacteria, affects our point here.

17 Cavalier-Smith, p. 978.

18 *Ibid.*

19 Margulis and Sagan, pp. 115–6.

20 *Ibid.*, p. 118.

21 Quoted in Ridley, p. 315.

22 Williams, p. v.

23 *Ibid.*, p. 11.

24 Smith, *The Evolution of Sex*, p. 10.

25 Smith, *Did Darwin Get It Right?*, p. 165.

26 Ridley, p. xxii.

27 Smith, *Did Darwin Get It Right?*, p. 165.

28 Williams, p. 8.

29 Margulis and Sagan, p. 157.

30 Smith, *Did Darwin Get It Right?*, pp. 166–7.

31 Williams, p. 11.

32 See Guarente and Kenyon.

- 33 A. M. Leroi, A. K. Chippindale and M. R. Rose, 'Long-Term Laboratory Evolution of a Genetic Life-History Trade-Off in *Drosophila Melanogaster*', in Rose, Passananti and Matos (eds.). (This is a reproduction of a paper that first appeared in the journal *Evolution* in 1994.)
- 34 Stephen Jay Gould, 'G. G. Simpson, Paleontology, and the Modern Synthesis', in Mayr and Provine, pp. 153–4.
- 35 Mayr, pp. 529–30. A genus is the next step up from a species in biological classification, a group of distinct species that are closely related genetically, sharing a close common ancestor. Examples are the genera *Canis*, to which dogs, wolves, jackals, coyotes and dingoes belong, and *Equus*, which includes horses, donkeys and zebras.
- 36 In the radio show 'The Whale – A History', in the *In Our Time* series presented by Melvyn Bragg, broadcast on BBC Radio 4 on 21 May 2009.
- 37 See, for example, Smith, *Did Darwin Get It Right?*, pp. 148–9.
- 38 Goodwin, pp. xii–xiii.



39 Many have the impression from the title of his book *The Selfish Gene* that Richard Dawkins proposes that natural selection acts at the level of the gene. But he doesn't: he argues that evolution should be *viewed* from the level of genes, because animals and plants are basically big bags of genes. Natural selection acts on the individual, but its ultimate effect is on the gene pool of the species, determining what genes are in it and how many of each gene there are. Although offering a potentially useful new perspective for evolutionists to look at certain questions, this theory ultimately only describes the same things in different words.

40 Fort, p. 38.

41 Le Page, p. 26.

42 Dawkins, *The Greatest Show on Earth*, pp. 297–8.

43 See Dawkins, *Climbing Mount Improbable*, chapter 5.

44 Mayr, p. 541.

45 Popper, p. 171.

46 *Ibid.*, p. 168.

47 *Ibid.*, p. 172.

48 Smith, *Did Darwin Get It Right?*, p. 180.

49 Smith, *The Evolution of Sex*, p. ix.

50 Dawkins, *The Blind Watchmaker*, p. 287.

51 Conway Morris, *Life's Solution*, pp. 315–6.

52 See Mayr's preface to Mayr and Provine, pp. ix–x.

53 Mayr and Provine, p. xv.

54 Stephen Jay Gould, 'The Hardening of the Modern Synthesis', in Grene (ed.), p. 88.

55 *Ibid.*, p. 90.

56 *Ibid.*, p. 91.

57 Dawkins, *The Ancestor's Tale*, p. 262.

58 Quoted in Costas R. Krimbas, 'The Evolutionary World-view of Theodosius Dobzhansky', in Adams (ed.), p. 188.

59 Dobzhansky, *Genetics of the Evolutionary Process*, p. 430.

60 *Ibid.*, p. 431.

- 61 Costas R. Krimbas, 'The Evolutionary Worldview of Theodosius Dobzhansky', in Adams (ed.), p. 189.
- 62 Dobzhansky, *Genetics of the Evolutionary Process*, p. 391.
- 63 Teilhard de Chardin, p. 258.
- 64 See Curtis L. Hancock, 'The Influence of Plotinus on Bergson's Critique of Empirical Science', in Harris, vol. I.
- 65 Bergson, p. 384.
- 66 Barrow and Tipler, p. 204.

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- 1 Popper, p. 173.
- 2 Conway Morris, *Life's Solution*, p. 316.
- 3 In his sixth and final Gifford Lecture, 'Towards an Eschatology of Evolution', at the University of Edinburgh, 1 March 2007. Audio file available at the University of Edinburgh's Humanities and Social Science's website: [www.hss.ed.ac.uk/giffordexemp/2000/details/ProfessorSimonConwayMorris](http://www.hss.ed.ac.uk/giffordexemp/2000/details/ProfessorSimonConwayMorris).

- 4 Conway Morris, *Life's Solution*, p. xv.
- 5 *Ibid.*, p. xii.
- 6 Abstract to Conway Morris' Gifford Lecture 'Life's Solution', see Chapter 11, note 13.
- 7 Conway Morris, *Life's Solution*, pp. 292–5.
- 8 In his fourth Gifford Lecture 'Becoming Human: The Continuing Mystery', given at the University of Edinburgh on 26 Feb 2007. Audio file available at the University of Edinburgh's Humanities and Social Science's website: [www.hss.ed.ac.uk/giffordexemp/2000/details/ProfessorSimonConwayMorris](http://www.hss.ed.ac.uk/giffordexemp/2000/details/ProfessorSimonConwayMorris).
- 9 Conway Morris, *Life's Solution*, p. 328.
- 10 Shapiro, p. 807.
- 11 De Duve, *Vital Dust*, p. 297.
- 12 In his fourth Gifford Lecture – see note 8 above.
- 13 Polanyi, p. 47.
- 14 From the abstract of his fourth Gifford Lecture – see note 8 above.

- 15 'Who Speaks for the Earth?', thirteenth and final episode of the TV series *Cosmos*, first broadcast 21 December 1980. DVD released by Freemantle Home Entertainment, 2009. Directed by David F. Oyster, written by Carl Sagan, Ann Druyan and Steven Soter.
- 16 In the documentary movie *A Brief History of Time*, produced by David Hickman and directed by Errol Morris, Anglia Television/Gordon Freedman Productions, 1991.
- 17 The papers were published in Halliwell, Pérez-Mercader and Zurek.
- 18 Bierman, 'A World With Retroactive Causation', p. 1.
- 19 Davies, *The Goldilocks Enigma*, p. 274.
- 20 George, p. 56.
- 21 *Ibid.*
- 22 Bierman and Houtkooper.
- 23 See Bierman, 'Exploring Correlations Between Local Emotional and Global Emotional Events and the Behavior of a Random Number Generator'.
- 24 Hagel and Tschapke.

- 25 Radin, 'Exploring Relationships Between Random Physical Events and Mass Human Attention', p. 538
- 26 Radin, *Entangled Minds*, p. 206.
- 27 Wheeler, *Geons, Black Holes, and Quantum Foam*, p. 334.
- 28 Quoted in Jacques et al, p.1.
- 29 Interviewed for 'The Anthropic Universe', *The Science Show*, ABC National Radio, 18 February 2006, presented by Martin Redfern, produced by Pauline Newman. Transcript available at: [www.abc.net.au/rn/scienceshow/stories/2006/1572643](http://www.abc.net.au/rn/scienceshow/stories/2006/1572643).
- 30 Wheeler, *Geons, Black Holes, and Quantum Foam*, p. 331.
- 31 *Ibid.*, p. 333.
- 32 Gardner and Wheeler.
- 33 Jacques *et al.*
- 34 Wheeler, *Geons, Black Holes, and Quantum Foam*, p. 337.

- 35 Davies and Gribbin, p. 208.
- 36 Wheeler, from his foreword to Barrow and Tipler, p. 6.
- 37 John Archibald Wheeler, 'Law Without Law', in Wheeler and Zurek (eds.), p. 194.
- 38 On *The Science Show*, ABC National Radio. See note 29 above.
- 39 John Archibald Wheeler, 'Genesis and Observership', in Butts and Hintikka (eds.), p. 3.
- 40 B. J. Carr, 'On the Origin, Evolution and Purpose of the Physical Universe', in Leslie (ed.), p. 152.
- 41 John Archibald Wheeler, 'Genesis and Observership', in Butts and Hintikka (eds.), p. 21.
- 42 *Ibid.*, p. 19.
- 43 Barrow and Tipler. p. 203.
- 44 John Archibald Wheeler, 'Beyond the Edge of Time', in Leslie (ed.), p. 214.
- 45 Hawking and Mlodinow, p. 140
- 46 Gefter, p. 30.

47 Barrow and Tipler, p. 470.

48 On *The Science Show*, ABC National Radio. See note 28 above.

49 P. M. Rattansi, 'Newton's Alchemical Studies', in Debus (ed.) *Science, Medicine and Society in the Renaissance*, p. 179.

50 Hawking, *A Brief History of Time*, p. 175.

51 Copenhaver, p. 41.

52 *Ibid.*, p. 37.

53 Magee, p. 10.

54 *Ibid.*

55 Jantsch, p. 308.

56 *Ibid.*, pp. 308–9.

57 Wheeler, 'Law without Law', in Wheeler and Zurek, p. 209.



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- 1 Richard S. Westfall, 'Newton and the Hermetic Tradition', in Debus (ed.), *Science, Medicine and Society in the Renaissance*, p. 195.
- 2 John Archibald Wheeler, 'Beyond the End of Time', in Leslie (ed.), p. 212.
- 3 Richard S. Westfall, 'Newton and the Hermetic Tradition', in Debus (ed.), *Science, Medicine and Society in the Renaissance*, p. 185.
- 4 David Fideler, 'Neoplatonism and the Cosmological Revolution: Holism, Fractal Geometry, and Mind-in-Nature', in Harris (ed.), vol. I, p. 104.
- 5 *Ibid.*, p. 106.
- 6 *Ibid.*, p. 117.
- 7 Luckert, p. 61.
- 8 National Constitution Centre website: [www.constitution-center.org/libertymedal/recipient\\_1994\\_speech](http://www.constitution-center.org/libertymedal/recipient_1994_speech).
- 9 *Ibid.*

10 *Ibid.*

11 *Ibid.*

12 Copenhaver, p. 65. (Treatise XVIII)

13 *Ibid.*, p. 36. (Treatise X)

14 Quoted in Fideler, ‘Neoplatonism and the Cosmological Revolution: Holism, Fractal Geometry, and Mind-in-Nature’, in Harris (ed.), vol. I, p. 116.

15 Copenhaver, p. 48 (Treatise XI).

16 ‘What We’ll Never Know’, Rees’ third Reith lecture, broadcast on BBC Radio 4 on 16 June 2010. A transcript is available at: [downloads.bbc.co.uk/rmhttp/radio4/transcripts/20100615-reith.rtf](http://downloads.bbc.co.uk/rmhttp/radio4/transcripts/20100615-reith.rtf).

## Appendix

1 Copenhaver, p. xliv.

2 Fowden, p. 4.

3 Jonas, Chapter Seven.

4 See Yamauchi, Chapter one.

- 5 There is controversy over whether the few surviving writings ascribed to Simon Magus – which we only have because they were quoted by early Christian writers as fodder for hellfire and damnation fulmination – were written by him or his followers, but either way they reflect his theology and philosophy.
- 6 Quoted in Luckert, p. 301.
- 7 *Ibid.*, pp. 299–308.
- 8 See *The Masks of Christ*, p. 243–51.
- 9 G. Quispel, ‘The *Asclepius* – From the Hermetic Lodge in Alexandria to the Greek Eucharist and the Roman Mass’, in van den Broek and Hanegraff, p. 75.
- 10 Picknett and Prince, *The Masks of Christ*, pp. 222–5.

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    biblical theory [1](#), [2](#)

    heliocentric [1](#), [2](#), [3](#), [4](#), [5](#), [6](#), [7](#), [8](#), [9](#), [10](#), [11](#)

    hybrid model [1](#), [2](#)

    Ptolemaic [1](#), [2](#), [3](#), [4](#)

Worthington, John [1](#), [2](#), [3](#), [4](#)

Wotton, Sir Henry [1](#)

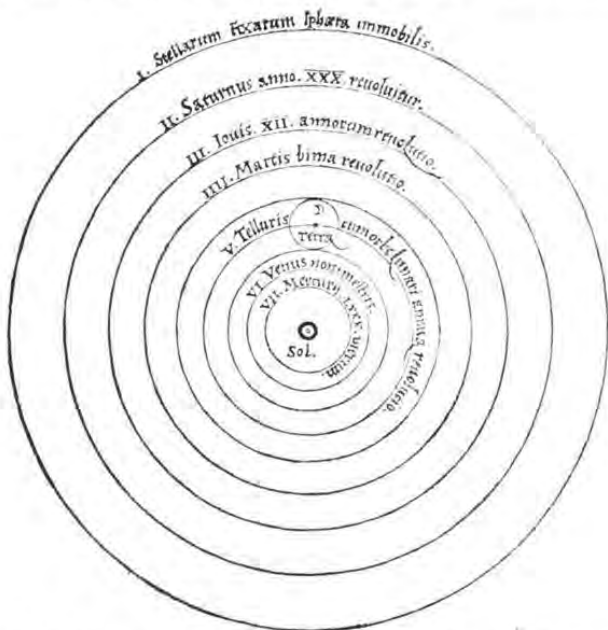
Wren, Sir Christopher [1](#)

Yeats, W. B. [1](#)

Young, Thomas [1](#)



net, in quo terram cum orbe lunari tanquam epicyclo contineri diximus. Quinto loco Venus nono mense reducitur, Sextum deniq; locum Mercurius tenet, octuaginta dierum spacio circū currens, In medio uero omnium residet Sol. Quis enim in hoc



pulcherrimo templo lampadem hanc in alio uel meliori loco poneret, quàm unde totum simul possit illuminare. Siquidem non inepte quidam lucernam mundi, alij mentem, alij rectorem uocant. Trimegistus uisibilem Deum, Sophocles Electra intuentiā

The famous page from Copernicus *On the Revolutions of the Celestial Spheres* (1543) showing his world-changing diagram of the sun-centred solar system. Less famously, just four lines below, he acknowledges his inspiration, the esoteric works of ‘Trismegistus’ – the legendary Egyptian sage Thrice-Great Hermes.

(Bridgeman)



Detail from the lavish decoration of the Vatican's fifteenth-century Appartamento Borgia, showing Hermes Trismegistus and Moses receiving divine inspiration from the

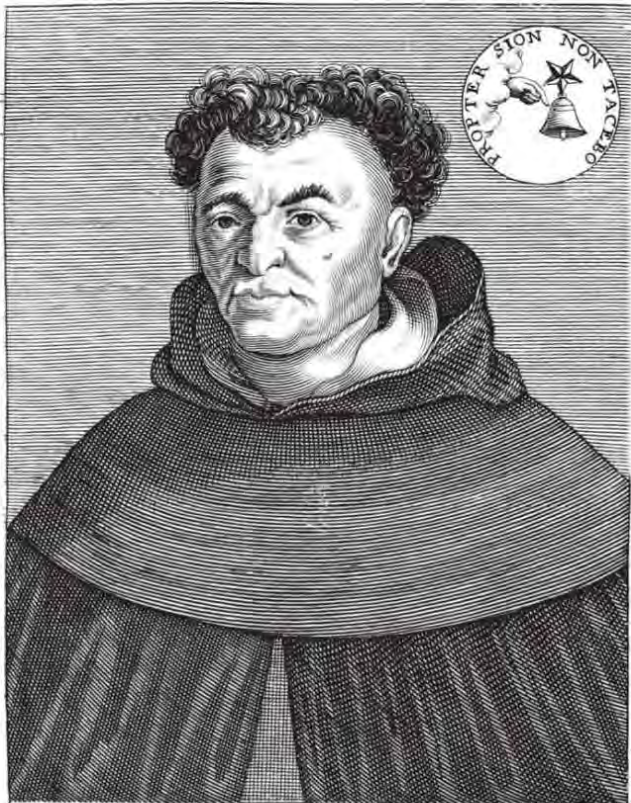
Egyptian goddess Isis – somewhat unusual for a pope's personal rooms. But this does show the extreme veneration that even the head of the Church accorded the demi-god of the Hermeticists.

(Author's collection)



The belief that Christianity could trace its origins via Hermeticism to ancient Egypt was taken to its extreme by the uncompromising Giordano Bruno, whose statue now stands on the spot in Rome where he was burned to death by the Church for heresy in 1600.

(Science Photo Library)

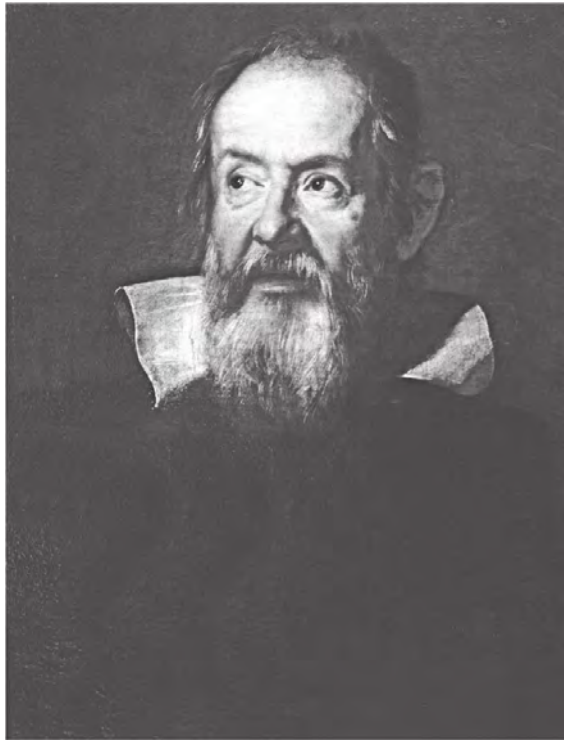


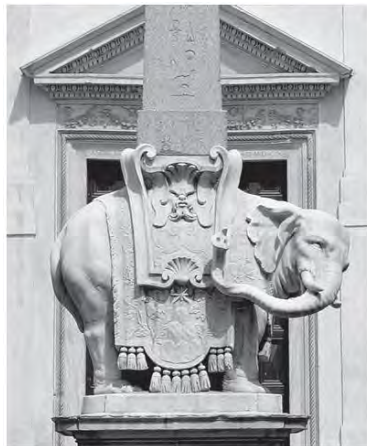
THOMAS CAMPANELLA —  
*De Larnioff, sculp.*

Bruno's belief that Copernicus' new model of the solar system would literally trigger a new age of spiritual and scientific enlightenment was shared by his successor Tommaso Campanella (*left*), who in turn was a close friend and advisor to Galileo(*below*). Considered science's great martyr because of his persecution by the Church, the evidence indicates that Galileo was motivated at least as much by the Hermetic significance of heliocentricity.

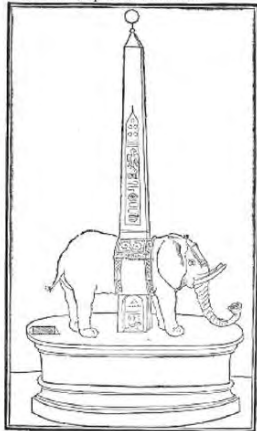
(top: Mary Evans Picture Library; bottom: Bridgeman)







detta ad intrare nella Elephantina machina excelsa.



Still standing tall in Rome, these ancient Egyptian obelisks re-erected in the 1650s by the remarkable Hermetic Jesuit Athanasius Kircher and the famous sculptor Bernini are replete with Hermetic symbolism. Their monument (*above left*), erected outside the church where Bruno and Galileo were condemned, draws its symbolism from the extraordinary esoteric work

*Hypnoerotomachia Poliphili* (above right).

(both images: Bridgeman)



*Obélisque de la Place Navone.*

Similarly the Fountain of the Four Rivers in Rome's Piazza Navona, encodes Hermetic secrets, as does Kircher's book on the subject, *Obeliscus Pawphilius* (*its frontispiece, opposite*).



(opposite bottom: Herzog August Bibliothek Wolfenbüttel:  
A: 13.1 Eth, 2<sup>o</sup>; above: Herzog August Bibliothek  
Wolfenbüttel: A: 66.1 Quod. 2<sup>o</sup>)



ISAAC NEWTON, F.R.S.

1642-1727

PHILOSOPHIÆ  
NATURALIS  
PRINCIPIA  
MATHEMATICA

AVCTORIS ET EDITORIS JOHANNIS STURGESON, AUCTORIS ET EDITORIS  
JOHANNIS STURGESON, AUCTORIS ET EDITORIS

IMPRIMATUR  
J. STURGESON, AUCTORIS ET EDITORIS

LONDON

JOHANNES STURGESON, AUCTORIS ET EDITORIS  
JOHANNES STURGESON, AUCTORIS ET EDITORIS



Sir Isaac Newton (1642–1727), whose work on gravity and the laws of motion was set out in the *Principia mathematica*, effectively created the modern technological world. Although the most famous scientist in history he was utterly dedicated to the magical Hermetic tradition, whose principles actually drove his masterpiece.

(Science Photo Library)



Although long doubted, recent research has shown that the Hermetic texts do indeed have their roots in ancient Egypt. Not only is Hermes Trismegistus the Egyptian god Thoth (*above left*) but the books encapsulate the wisdom of Heliopolis, magical city of the sun. This was the religion that inspired the building of the pyramids of Giza (*above right*) and the world's oldest religious writings, the Pyramid Texts (*below*).

(top left: Bridgeman; other images: author's collection)





*(top-left)* The early universe as captured by the Hubble Space Telescope. The understanding emerging from modern physics is rapidly converging with the ancient Hermetic vision of the cosmos as the mind of God – and, to the Hermeticists, therefore also the mind of humankind. The notion of the ‘participatory universe’ developed by the eminent American physicist John Archibald Wheeler

(below) intimately links human consciousness to the creation and growth of the universe, as depicted in his famous 'U and eye' diagram (*top-right*). In the act of observing the universe, intelligent life is in some way actually creating it.

(top-left: NASA; top-right: Keith Prince; bottom: Science Photo Library)

